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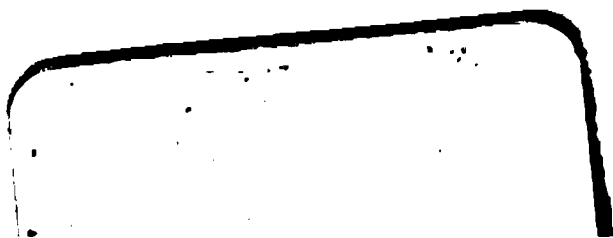
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1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
25
26
27
28
29
30
31
32
33
34
35
36
37
38
39
40
41
42
43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86
87
88
89
90
91
92
93
94
95
96
97
98
99
100



THE
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CONTENTS OF No. XXIX.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1855.

Analytical and Critical Reviews.

	PAGE
REV. I.—On the Structure and Use of the Spleen. By HENRY GRAY, F.R.S., Demonstrator of Anatomy and Surgical Curator of the Pathological Museum at St. George's Hospital	1
REV. II.—Lettsomian Lectures on Insanity. By FORBES WINSLOW, M.D., D.C.L., late President of the Medical Society of London	25
REV. III.—Traité des Maladies du Sein et de la Région Mammaire. Par A. VELPEAU, Membre de l'Institut, &c.	35
A Treatise of the Diseases of the Breast and of the Mammary Region. By A. VELPEAU, &c.	ib.
REV. IV.—Klinik der Geburtshilfe und Gynækologie. Von Med. und Chir. Dr. J. CHIARI, Med. und Chir. Dr. CARL BRAUN, und von Med. und Chir. Dr. J. SPAETH	50
Clinical Observations on Obstetric Medicine. By Drs. CHIARI, BRAUN, and SPAETH	ib.
REV. V.—Beiträge zur Vergleichenden Pathologischen Anatomie der Gelenk- krankheiten. Von Dr. E. GURLT, Prakt-Artz und Assistenz-Artz der Chirurgisch-Augenärztlichen Klinik der Universität zu Berlin. 1853	57
Contributions to the Comparative Pathological Anatomy of Diseases of the Joints. By Dr. E. GURLT	ib.
REV. VI.—1. De la Mort et de ses Caractères. Par le Dr. JOSAT	71
2. Du Signe Certain de la Mort. Par MICHEL-HYACINTHE DESCHAMPS	ib.
3. Traité des Signes de la Mort. Par E. BOUCHUT	ib.
4. The Medical Aspects of Death. By JAMES BOWER HARRISON	ib.
REV. VII.—1. Cure and Prevention of Scarlet Fever. By SAMUEL HAHNEMANN. (Lesser Writings of the Author, collected and translated by R. E. DUDGEON, M.D.)	77
2. Travaux Thérapeutiques sur la Belladone. Publiée par A. L. J. BAYLE. (Tome Seconde de 'Bibliothèque de Thérapeutique')	ib.
3. Homœopathy: its Tenets and Tendencies. By Professor SIMPSON	ib.
4. Homœopathy fairly represented. By Professor HENDERSON	ib.

	PAGE
REV. VIII. —1. Lehrbuch der Physiologischen Chemie. Von Professor Dr. C. G. LEHMANN. Dritter Band, 1852	102
A Compendium of Physiological Chemistry. By Professor LEHMANN. The Chapter on Digestion	ib.
2. Handbuch der Physiologischen Chemie. Von Professor Dr. C. G. LEHMANN	ib.
A Manual of Physiological Chemistry. By Professor LEHMANN	ib.
3. The Bakerian Lecture, on Osmotic Force : delivered at the Royal Society. By Professor GRAHAM, F.R.S., Corresponding Member of the Institute of France. ('Medical Times and Gazette')	ib.
REV. IX. —Epilepsy and other Affections of the Nervous System, which are marked by Tremor, Convulsion, or Spasm : their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M.D., &c. &c.	118
REV. X. —Cases of Bright's Disease, with Remarks. By SAMUEL WILKS, M.D. Lond. ('Guy's Hospital Reports,' Second Series, vol. viii.)	122
REV. XI. —Sudden Death. By A. B. GRANVILLE, M.D., F.R.S., M.R.C.P. Lond., &c. &c.	134
REV. XII. —Bericht über die elektro-physiologische Arbeiten des Dr. DUCHENNE, de Boulogne, zu Paris. Von Dr. HERMANN EBERHARD RICHTER. ('Schmidt's Jahrbuch,' No. 11, 1853)	138
Report upon the Electro-physiological Researches of Dr. DUCHENNE. By Dr. H. E. RICHTER. ('Schmidt's Jahrbuch' for October, 1853)	ib.
2. Die Electricität in ihrer Anwendung auf Practische Medicin. Von Dr. MORITZ MEYER, artz in Berlin. 1854	ib.
Electricity in its Application to Practical Medicine. By Dr. MORITZ MEYER, of Berlin. 1854	ib.
3. Histoire de l'Electricité Médicale. Par M. J. GUITARD, M.D., Président de la Société Médicale d'Emulation de Toulouse. 1854	ib.
History of Medical Electricity. By M. J. GUITARD, M.D., Toulouse. 1854	ib.
4. De l'Action Thérapeutique de l'Electrisation Localisée dans le Traitement des Paralysies Consécutives à l'Hémorrhagie Cérébrale. Par M. le Dr. DUCHENNE, de Boulogne. ('Bulletin Général de Thérapeutique Médicale et Chirurgicale,' Mars et Avril, 1854)	ib.
On the Therapeutic Action of Topical Electrification in the Treatment of Paralysis, consecutive to Cerebral Hæmorrhage. By Dr. DUCHENNE	ib.
5. Heilung der Impotentia virilis, mittels Electricität. Von Dr. B. SCHULZ, in Wien. ('Wien Wochenschrift,' 10—12. 1854)	ib.
On the Cure of Impotence by means of Electricity. By Dr. B. SCHULZ, of Vienna. ('Schmidt's Jahrbuch,' August, 1854)	ib.
REV. XIII. —Census of Ireland for the Year 1851. Part III. Report on the Status of Disease. By W. DONNELLY and W. R. WILDE. Presented to Parliament	151
REV. XIV. —1. De cognocendis et curandis Placentæ Morbis. WILDE	156
2. Handbuch der Speciellen Pathologischen Anatomie. Von CARL ROKITANSKY	ib.
3. Lectures on the Theory and Practice of Midwifery. By ROBERT LEE, M.D.	ib.
4. Ueber die Krankheiten des Eies und der Placenta. GIERSE und H. MECKEL. ('Verhandl. der Ges. für Geburtsk.')	ib.
5. Abhandlungen über den Bau der Molen. Von H. MÜLLER	ib.
6. Ueber den Bau der Molen. METTENHEIMER. ('Müller's Archiv,' 1850)	ib.
7. Note sur les Altérations du Placenta. Par CHARLES ROBIN. ('Archives Gén. de Méd.,' Juin, 1854)	ib.

Bibliographical Record.

	PAGE
ART. I.—1. A System of Instruction in Quantitative Chemical Analysis. By Dr. C. R. FRESSENIUS. Second Edition. Edited by J. LLOYD BULLOCK, F.C.S.	173
2. A Course of Practical Chemistry, arranged for the Use of Medical Students. By WILLIAM ODLING, M.B., F.C.S., Professor of Practical Chemistry and of Natural History at Guy's Hospital	ib.
3. Anleitung zur Qualitativen und Quantitativen Zoochemischen Analyse. Von E. VON GORUP-BESANEZ. Zweite Auflage	ib.
Guide to Qualitative and Quantitative Zoo-chemical Analysis	ib.
4. Anleitung zur Qualitativen und Quantitativen Analyse des Harns. Von CARL NEUBAUER	ib.
Guide to the Qualitative and Quantitative Analysis of the Urine	ib.
ART. II.—Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., Physician to the Hospital for Sick Children. Third Edition .	174
ART. III.—A Practical Treatise on the Diseases of the Eye. By WILLIAM MACKENZIE, M.D. Fourth Edition	ib.
ART. IV.—On the Topical Medication of the Larynx in Certain Diseases of the Respiratory and Vocal Organs. By EBEN. WATSON, M.D., Lecturer on the Institutes of Medicine in the Andersonian University, &c.	ib.
ART. V.—A Treatise on Diseases of the Lungs, having especial reference to Consumption. By ANTHONY WILLIAM CLARKE, M.D.	176
ART. VI.—Principles of Comparative Physiology. By WILLIAM B. CARPENTER, M.D., F.R.S., &c. Fourth Edition	ib.
ART. VII.—Transactions of the Pathological Society. Volume V.	177
ART. VIII.—A Disquisition on Certain Parts and Properties of the Blood. By DAVID TOD, M.R.C.S.	ib.
ART. IX.—An Expository Lexicon of the Terms used in Medical and General Science. By R. G. MAYNE. Parts II. & III.	178
ART. X.—Neurologische Untersuchungen. Von Dr. RUDOLPH WAGNER	ib.
Neurologic Researches. By Dr. WAGNER	ib.
ART. XI.—Lectures on Polarized Light. By the late JONATHAN PEREIRA, M.D., F.R.S., F.L.S. Second Edition. Edited by the Rev. BADEN POWELL, M.A., V.P.R.S.	ib.
ART. XII.—Summary of New Publications	ib.

Original Communications.

ART. I.—The Blood—its Chemistry, Physiology, and Pathology. By THOMAS WILLIAMS, M.D. Lond., Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea	181
ART. II.—The Pathology of Insanity. By JOHN CHARLES BUCKNILL, M.D. Lond., Physician to the Devon County Lunatic Asylum	207
ART. III.—Scarlatinal Dropsy. By JOHN W. TRIPE, M.D.	229
ART. IV.—A Nosological Inquiry into the Acute Diseases of the Chest known to the Greeks—viz., the Pleuritis and the Peripneumonia; with Remarks upon the Peripneumonia Notha of the Moderns. By W. T. GAIRDNER, M.D., Lecturer on the Practice of Medicine in Edinburgh	242

Chronicle of Medical Science.

	PAGE
<i>Annals of Physiology.</i> By HERMANN WEBER, M.D.	
I. Food and Digestion	261
II. Blood, Respiration, Circulation	265
III. Lymphatic System and Ductless Glands	268
IV. Organs of Secretion and Excretion. Skin	ib.
V. Nervous System	271
VI. Organs of Senses	272
<hr/>	
<i>Quarterly Report on Pathology and Medicine.</i> By E. A. PARKES, M.D. (Lond.)	273
<hr/>	
<i>Quarterly Report on Surgery.</i> By HOLMES COOTE, F.R.C.S.	277
<hr/>	
<i>Quarterly Report on Midwifery.</i> By ROBERT BARNES, M.D. (Lond.)	
I. Diseases of Women in the Unimpregnated State	284
II. Labour	286
<hr/>	
<i>Quarterly Report on Forensic Medicine, Toxicology, &c.</i> By W. B. KESTIVEN, F.R.C.S.	
I. Injuries, &c.	288
II. Medico-legal Osteology	290
III. Toxicology	293
<hr/>	
<i>Medical Intelligence :</i>	
Dr. TAYLOR on the Cause of the Explosion at Newcastle	296
<hr/>	
BOOKS RECEIVED FOR REVIEW	299

CONTENTS OF No. XXX.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1855.

Analytical and Critical Reviews.

	PAGE
REV. I.—1. Hints towards the Formation of a more comprehensive Theory of Life. By SAMUEL TAYLOR COLERIDGE. Edited by SETH B. WATSON, M.D.	301
2. The Human Body, and its Connexion with Man, illustrated by the Principal Organs. By JAMES J. GARTH WILKINSON, M.R.C.S.L.	ib.
3. Cours de Philosophie Positive. Par M. AUGUSTE COMTE, Ancien Elève de l'École Polytechnique. Tome troisième, contenant la Philosophie Chimique et la Philosophie Biologique	ib.
Positive Philosophy. By AUGUSTE COMTE. Vol. III.	ib.
4. The Positive Philosophy of Auguste Comte. Freely Translated and Condensed by HARRIET MARTINEAU. Books IV. and V. ('Chapman's Quarterly Series')	ib.
5. Système de Politique Positive. Par AUGUSTE COMTE. Tome premier, Chapitre troisième; Introduction Directe, Naturellement Synthétique ou Biologie	ib.
System of Positive Politics. By AUGUSTE COMTE. Vol. I. chap. iii.	ib.
6. The Positive Philosophy of Auguste Comte. By G. H. LEWES. ('Bohn's Scientific Library')	ib.
7. Psychological Inquiries. By B. C. B.	ib.
8. Histoire Naturelle Générale des Règnes Organiques principalement Etudiée chez l'Homme et les Animaux. Par M. ISIDORE GEOFFROY SAINTE-HILAIRE, Membre de l'Institut, &c. &c. Tome premier	ib.
General Natural History of the Organic Kingdoms. By M. ISIDORE GEOFFROY SAINTE-HILAIRE. Vol. I.	ib.
REV. II.—On some Diseases of Women admitting of Surgical Treatment. By ISAAC BAKER BROWN, F.R.C.S.	315
REV. III.—Handbuch der Speciellen Pathologie und Therapie. Redigirt von RUD. VIRCHOW. Band I. Heft 1	323
Manual of Special Pathology and Therapeutics. By R. VIRCHOW. (The Chapters on Fever and Inflammation)	ib.
REV. IV.—Traité Clinique et Pratique des Maladies des Enfants. Par MM. RILLIET et BARTHEZ, anciens internes lauréats de l'Hôpital des Enfants Malades de Paris, &c. Ouvrage couronné par l'Académie des Sciences et par l'Académie de Médecine. Deuxième Edition, entièrement refondue considérablement augmentée	332
A Clinical and Practical Treatise on the Diseases of Children. By MM. RILLIET and BARTHEZ. Second Edition	ib.

	PAGE
REV. V.—1. A Manual of Pathological Anatomy. By C. HANDFIELD JONES, M.B., and EDWARD H. SIEVEKING, M.D.	346
2. Handbuch der Speciellen Pathologischen Anatomie. Von Dr. AUGUST FÖRSTER	ib.
Manual of Special Pathological Anatomy	ib.
3. Atlas der Mikroskopischen Pathologischen Anatomie. Gezeichnet und Bearbeitet von Dr. AUGUST FÖRSTER	ib.
Atlas of Microscopical Pathological Anatomy. Observed and Drawn by Dr. AUGUSTUS FÖRSTER	ib.
REV. VI.—Chloroform, its Properties and Safety in Childbirth. By EDWARD WILLIAM MURPHY, A.M., M.D., Professor of Midwifery, University College Hospital, &c. &c.	359
REV. VII.—1. The Decline of Life in Health and Disease; being an Attempt to Estimate the Causes of Longevity. By B. VAN OVEN, M.D.	369
2. De la Longévité Humaine, et de la Quantité de Vie sur le Globe. Par P. FLOURENS	ib.
On Human Longevity and the Amount of Life on the Globe. By M. P. FLOURENS	ib.
REV. VIII.—1. Die Hömodynamik nach Versuchen. A. W. VOLKMANN. (Cap. xii. Von den Kräften welche das Blut bewegen)	372
On the Forces which Move the Blood. By A. W. VOLKMANN	ib.
2. Le Système Capillaire Sanguin. Thèse par L. A. SEGOND	ib.
On the Sanguineous Capillary System. By L. A. SEGOND	ib.
3. Principles of Comparative Physiology. By W. B. CARPENTER, M.D., F.R.S., F.G.S. (Chap. v.)	ib.
REV. IX.—On the Formation and Extension of Cancer-Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation. By J. L. C. SCHROEDER VAN DER KOLK. ('Nederlandsch Lancet,' Sept. 1853)	390
REV. X.—The Medico-Chirurgical Transactions. Second Series. Vol. XIX.	400
REV. XI.—On Pain after Food: its Causes and Treatment. By EDWARD BALLARD, M.D. Lond.	407
REV. XII.—1. A View of the Formation, Discipline, and Economy of Armies. By the late ROBERT JACKSON, M.D., Inspector-General of Army Hospitals. Third Edition	411
2. The Queen's Regulations for Army Hospitals	ib.
3. The Medical Codes of the Bengal, Madras, and Bombay Armies. Fort William, Fort St. George, and Fort St. David (various dates)	ib.
4. Etudes sur le Service de Santé Militaire en France, son passé, son présent, son avenir. Par L. J. BÉGIN, Chirurgien-Inspecteur, &c.	ib.
Remarks on the Military Medical Service in France; its Past, its Present, and its Future. By M. L. J. BÉGIN	ib.
5. Réorganisation du Service Sanitaire de l'Armée Belge	ib.
Reorganization of the Sanitary Service of the Belgian Army	ib.
6. Letter to the Right Hon. the Secretary at War on the Medical Department of the Army. From Sir GEORGE BALLINGALL, Regius Professor of Military Surgery in the University of Edinburgh	ib.
7. Recueil de Mémoires de Médecine, de Chirurgie, et de Pharmacie Militaires rédigé, sous la Surveillance du Conseil de Santé. Par MM. JACOB, Docteur-en-Médecine, Ancien Pharmacien-Major; PARDIN, Médecin-en-Chef de l'Hôpital Militaire du Roule; et par interim A. JUDAS, Ancien Médecin Principal de Première Classe. Publié par ordre du Ministre de la Guerre. Deuxième Série. Troisième Volume	ib.
Collection of Memoirs of Military Medicine, Surgery, and Pharmacy, drawn up under the Superintendence of the Council of Health	ib.
REV. XIII.—Mode of Communication of Cholera. By JOHN SNOW, M.D.	449
REV. XIV.—Vertigo: a Paper read to the North London Medical Society, April 12th, 1854. By J. RUSSELL REYNOLDS, M.D. Lond., University Medical Scholar	463

Bibliographical Record.

	PAGE
ART. I.—Medical Jurisprudence. By ALFRED S. TAYLOR, M.D., F.R.S., &c. &c.	467
ART. II.—Handbuch der Speciellen Pathologie und Therapie. Redigirt von R. VIRCHOW, Professor in Würzburg	ib.
Manual of Special Pathology and Therapeia	ib.
ART. III.—Food and its Adulterations; comprising the Reports of the Analytical Sanitary Commissioner of the 'Lancet.' By ARTHUR HILL HASSALL, M.D.	468
ART. IV.—Report on the Mortality and Public Health of Oxford, during the years 1849–50	469
ART. V.—Notes on some of the Developmental and Functional Relations of certain Portions of the Cranium. Selected by F. W. PAVY, M.D. Lond., from the Lectures on Anatomy delivered at Guy's Hospital by JOHN HILTON, F.R.S.	ib.
ART. VI.—The Prize Essay on the Changes since the Time of Pinel in the Moral Management of the Insane, &c. &c. By DANIEL H. TUKE M.D., Assistant Medical Officer to the York Retreat	471
ART. VII.—The Pathology of Drunkenness. By CHARLES WILSON, M.D.	472
ART. VIII.—Transactions of the Belfast Clinical and Pathological Society for the Session 1853–4	ib.
ART. IX.—Archives de Physiologie, de Thérapeutique, et d'Hygiène. Par M. BOUCHARDAT. No. II. Octobre, 1854. Mémoire sur l'Action Physiologique et Thérapeutique des Ferrugineux. Par T. A. QUÉVENNE	ib.
On the Physiologic and Therapeutic Action of Preparations of Iron	ib.
ART. X.—Lithotomy Simplified. By GEORGE ALLARTON, M.R.C.S.	473
ART. XI.—The Practitioner's Pharmacopœia. By JOHN FOOTE, M.R.C.S. Lond.	474
ART. XII.—The Diagnosis of Surgical Cancer. (The Liston Prize Essay for 1854.) By JOHN ZACHARIAH LAURENCE, Surgeon to the Northern and Farringdon Dispensaries.	ib.
ART. XIII.—Summary of New Publications	476

Original Communications.

ART. I.—On the Mode of Development of Tubercle in the Lungs in Chronic Phthisis: its connexion with Fatty Degeneration of the Epithelium of the Air-Vesicles, and its Early Manifestation by Fatty Epithelial Cells in the Sputa. By C. RADCLYFFE HALL, M.D., F.R.C.P.E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay	477
ART. II.—On the Means of Diagnosing the Internal Diseases of the Eye. By C. BADER, M.D., and BRANSBY ROBERTS, Esq., Resident Medical Officer Royal London Ophthalmic Hospital, Moorfields	501
ART. III.—On the Peculiarities in Figure, the Disfigurations, and the Customs of the New Zealanders; with Remarks on their Diseases, and on their Modes of Treatment. By ARTHUR S. THOMSON, M.D., Surgeon of the 58th Regiment of Foot	520
ART. IV.—Note on the Induction of Sleep and Anæsthesia by Compression of the Carotids. By ALEXANDER FLEMING, M.D., Professor of Materia Medica, Queen's College, Cork	529

Chronicle of Medical Science.

	PAGE
<i>Report on the Progress of Animal Chemistry during the years 1852-3-4.</i> By GEORGE EDWARD DAY, M.D., F.R.S.	
Organic Non-Nitrogenous Acids	534
Basic Bodies	537
Fats and Lipoids	543
Non-Nitrogenous Neutral Bodies	544
Pigments	546
The Protein Bodies	547
The Mineral Substances of the Animal Body	548
The Chemistry of the Animal Fluids	ib.
<hr/>	
<i>Annals of Micrology.</i> By ROBERT D. LYONS, M.B., T.C.D., M.R.I.A.—PART I. ANATOMICAL AND PHYSIOLOGICAL MICROLOGY.	
Histo-Genesis	553
Permanent Tissues and Definite Element	554
Regeneration of Tissues	557
Histology of Animal Fluids	558
Cartilage and Bone	561
Nerves	ib.
Various Structures	562
<hr/>	
<i>Quarterly Report on Pathology and Medicine.</i> By E. A. PARKES, M.D. (Lond.)	565
<hr/>	
<i>Quarterly Report on Midwifery.</i> By ROBERT BARNES, M.D. (Lond.)	
I. Diseases of the Sexual System	571
II. Diseases of Fœtus	572
III. Labour	574
<hr/>	
<i>Medical Intelligence :</i>	
Nurses for the Poor	576
The Examination for the East India Company's Medical Service	577
<hr/>	
BOOKS RECEIVED FOR REVIEW	578
<hr/>	
TITLE, CONTENTS, INDEX.	

THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1855.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

On the Structure and Use of the Spleen. By HENRY GRAY, F.R.S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum, at St. George's Hospital.—London, 1854. 8vo, pp. 380. With Sixty-five Wood Engravings.

WHATEVER may be thought of the merits of the system of prize essays in the abstract, little hesitation can be felt by any member of the medical profession in affirming, that the institution of the Astley Cooper Prize has been highly advantageous to science, as well as honourable to the gentlemen to whom it has been successively awarded. To the admirable essays of Mr. Simon on the Thymus Gland, and of Mr. Wharton Jones on Inflammation, a third has now been added, which is certainly second to neither of its predecessors as to the evidence it gives of well-directed and laborious research, and which contributes many valuable materials towards the elucidation of the *questio vexata* that forms its subject. That Mr. Gray should have entirely succeeded in overcoming the difficulties of the investigation, and should have attained the full solution of a problem which has baffled so many able and zealous inquirers, it would be scarcely fair to expect; and we think it just to our author, as well as to our readers, to intimate at the outset, that whilst he has done much, he has also left much undone,—some of those very conclusions which he thinks he has most satisfactorily attained, being, to our minds, the most problematical.

The interest and importance of the subject, and the desire we feel, both on Mr. Gray's account, and for the sake of science, that the value of his labours should be justly appreciated, render it necessary that we should analyse his account of them with some minuteness. It is prefaced by a *Historical Introduction*, in which we naturally expected to find a complete summary of the anatomical results obtained, and the physiological doctrines propounded, by all the principal inquirers who had preceded the

author in the same line of investigation. But whilst very full in regard to the speculative opinions of the older writers, which can now be only referred-to as antiquarian curiosities, it gives but scanty information as to the labours of the most recent and pains-taking investigators. The memoir of Dr. Julian Evans, published in 1844, on the Microscopic Anatomy of the Spleen, is mentioned with deserved commendations, and a summary is given of its contents; but of the numerous and important researches which have been subsequently made in the same direction, especially by Dr. Sanders and Professor Kölliker, scarcely any notice is here taken; some of their physiological conclusions being alone cited. It is true that these later researches are occasionally adverted-to by way of comparison, in the account of the author's own investigations; but it would have been far more convenient, as well as more satisfactory, had the historical summary been carried down to the date of his essay. And it would have also added greatly to the value of the book, had some notice been taken of the recent labours of Remak and Leydig, in preparing the essay for the press.

The *Development of the Spleen* is traced out with such completeness and detail, as to leave little for any one else to accomplish. The results of Mr. Gray's investigations on this subject had been partly communicated to the Royal Society, and published in the 'Philosophical Transactions' for 1852, in connexion with his parallel researches on the 'Development of the Ductless Glands' generally, of which we gave an account at the time;* but we find many important details here, which that paper does not contain; and there are, besides, several interesting illustrations. The first point established by Mr. Gray, is the original distinctness of the spleen from the pancreas, which he states to be very evident at the first appearance of the two organs, each arising from its own mass of blastema, although the increased size of both organs subsequently causes them to approximate so closely, as to have given rise in the minds of some of our best embryologists, to the idea that they formed but a single blended mass. Like other organs, the spleen is at first a homogeneous collection of nuclei and granular matter, which is developed in a fold of the intestinal laminae; and which gradually undergoes increase and differentiation, whereby the several tissues and structures of the organ are evolved; part of these elements becoming developed into fibrous tissue to form the capsule and trabeculae, part giving origin to bloodvessels and blood, whilst the greater part remains but little changed, to form the essential component of the organ, the "pulp-tissue." The Malpighian vesicles, which are such characteristic components of the spleen of the higher vertebrata, are not developed in the chick until near the completion of the period of incubation; being first seen as clusters of nuclei and fine granules, at the angles of division of the smaller bloodvessels, and upon the walls of the vessels themselves, to which they closely adhere; they are at first unenclosed in a special investing membrane, and do not acquire a proper envelope of any kind until some days after incubation has been completed. The following important physiological conclusions are drawn by Mr. Gray from the developmental history of the spleen:

* Vol. xli. p. 541.

"First, the small size of the spleen in the foetus, as compared with its proportionate increase after birth, tends to show that it is not an organ the function of which is mainly exercised during *intrauterine* life. Second, the *entire absence* of any evidence, either of the *formation* of the blood-discs in the spleen (after its connexion with the general vascular system is effected), or of their *disintegration*, shows, I think, that it is neither a blood-forming nor a blood-destroying gland, at least during *foetal* life. Third, in the pulp-parenchyma, a distinct process of cell-growth, of ripening, and of cell-destruction has been observed, and these processes have been seen to occur concomitant with the evolution of the vessels of the gland; that of cell-growth occurring with extreme rapidity as soon as the arteries which supply the organ are formed; then that of ripening, and of cell-destruction, taking place to the greatest extent up to, and during the time that the development of the splenic veins takes place. This would seem to show that some secretion took place in the gland, which became collected in it, ready to be removed by the veins as soon as their development should occur. Such a process is always to be found going on in man and animals, though ever varying in extent at all periods." (pp. 70, 71.)

Mr. Gray further mentions the important fact, that a distinct yellowish-green bile is found in the gall-bladder of the foetal chick, at a period *considerably antecedent* to the development of the splenic vein, and its connexion with the vena portæ; so that the colouring matter of the bile cannot be formed, as supposed by Kölliker, entirely at the expense of blood-corpuscles which have undergone disintegration in the spleen.

The weight of the human spleen, towards the end of foetal life, as compared to that of the entire body, averages about 1 : 350, whilst in adult life it varies from 1 : 320 to 1 : 340, or 1 : 400. In old age, on the contrary, the organ not only decreases in *absolute* weight, diminishing from an average of seven ounces, to an average of four ounces and a half; but it also diminishes in *proportional* weight, its average being only about 1 : 700. From the comparison of 160 observations on the weight of the spleen at different ages, therefore, Mr. Gray comes to the following conclusion:— "That the spleen attains its largest size and exerts its peculiar function [most energetically] between the periods of birth and the later periods of adult life; or, in other words, *during the most active periods of growth and nutrition of the body.*" But as the same comparison serves to show, that the range of variation, both above and below the average, at each period of life, is a very wide one, and is far greater than can be fairly ascribed to individual peculiarity, the question arises, Whether this variation can be legitimately ascribed to differences in the condition of the organ, either as to the diverticular function ascribed to it by Stukely, Dobson, and others, or as to the activity of its glandular operations, and the supply of blood which it may receive for this purpose. As this is a question which needed to be experimentally investigated, Mr. Gray had recourse to the rabbit; and from a comparison of the weights of the spleens of thirty individuals of this species, at different periods after feeding, he has deduced what appears to be the fair conclusion, "that the weight of the spleen *increases considerably* during the time when the digestive process is near to its completion, at the time when the new material is about to be, or has become, converted into blood; and that it *decreases considerably* in weight at varying periods *after* that process has been finally completed." This conclusion is confirmed by the results of experiments upon highly-fed and upon starved (or rather, we presume, insufficiently fed) animals

of the same species; for the increase during digestion was carried in the former to an unusual degree, so as to give the organ more than twice its normal weight, whilst in the latter, no increase after feeding was observable.

Having thus studied the organ as a whole, and shown what conclusions may be drawn from its comparative dimensions under different circumstances, Mr. Gray proceeds to a minute examination of its *Anatomical Structure*. Through this we shall not follow him in any detail, since a large part of his descriptions merely repeat what was previously well known; but we shall select those points on which he has added to our previous information, or as to which he is at issue with preceding observers. His account of the fibrous tissues of which the fibrous capsule and trabeculae are composed, corresponds in the main with that of Professor Kölliker and other histologists; and Mr. Gray agrees with previous experimenters, moreover, in regard to the very low degree of contractility which these tissues exhibit. He has submitted the spleens of sheep, oxen, dogs, and cats to a strong magnetic current, within a short time after death, when the same current produced the most marked contractions in the oesophagus, the large and small intestines, and the bladder; and no effect whatever was produced on the spleens of sheep and oxen; whilst in those of dogs and cats, a slight wrinkling of the surface was produced, though the diameter of the organ was not sensibly lessened, and no blood was expelled from it. No such decided contractions were witnessed, as have been asserted by some observers to take place; and it is strongly to be suspected that those statements were made under the influence of a preconceived idea of the muscularity of the organ, and of its agency in the propulsion of the blood. It would have been more satisfactory, however, if Mr. Gray had used the interrupted current of the magneto-electric apparatus, as well as the continuous current of the ordinary galvanic battery, which is what (from his designation of it) we presume him to have employed in these experiments.

Mr. Gray next proceeds to describe the *Bloodvessels* of the spleen, his account of which corresponds in most particulars with that of his predecessors, but contains some important points of novelty. The walls of the capillaries, he says, lie in immediate contact with the pulp-tissue, by which means this substance is constantly exposed to the influence of the fluid ingredients of the blood. Generally speaking, the capillaries may be traced into direct continuity with the smaller veins, which suddenly increase greatly in size after the junction of the capillaries with them, and have walls of even greater delicacy than those of the capillaries themselves. Some of the capillaries, however, cannot be traced into direct continuity with veins (as Ecker had previously ascertained), but gradually becoming reduced in size, their wall becomes more delicate, and is finally lost; the injected material then escapes into interspaces in the pulp, the walls of which are formed merely by its components; and they appear finally to communicate with the veins by intercellular spaces,—thus presenting us with an example (which, so far as we yet know, is a solitary one) of the exceptional existence of that *lacunar* circulation in man, which is the normal type in the lower invertebrate animals. As this is a point of first-rate importance, and Mr. Gray's conclusions in regard to it

are not in accordance with those of most of his predecessors, we should have liked more precise information as to the mode in which he examined these capillaries, and satisfied himself of their frequent want of distinct walls. The veins originate in three modes:—(1) by direct continuity with the arterial capillaries; (2) by lacunar spaces; (3) by cæcal pouches. Of these modes of origin, the second is stated by Mr. Gray to be by far more frequent than either of the other two; and its physiological relations are, as he justly observes, of very great importance, since they afford a ready explanation of the *constant occurrence* of diffusion of blood, and of its disintegration in the pulp-tissue of the organ, which he believes himself to have substantiated. The origin of the veins by cæcal pouches is not frequently to be observed; but the existence of these pouches (which usually consist of vessels of about $\frac{1}{30}$ th of an inch in diameter, but of variable length, generally situated on the side of a vein just before its smallest branches are given off) is of interest in relation to the supposed diverticular function of the venous system of the spleen. The veins, in their course towards their main trunk, increase in diameter beyond all proportion to the size of the branches they receive; thus, two branches, each of $\frac{1}{30}$ th of an inch in diameter, formed a trunk of $\frac{1}{30}$ th of an inch; and a little further, this trunk had increased to the diameter of $\frac{1}{20}$ th of an inch, without receiving any accessory branches. Thus it appears that, even in the human spleen, there is a provision for the reception of very varying amounts of blood into the venous system; the small as well as the large branches of which are supported by a highly elastic and distensile framework of fibrous tissue.

This provision, however, is far more obvious in the spleens of Ruminant and Pachydermatous mammals; for the fibrous sheaths which envelope the smaller veins in man, even to their minute subdivisions, and which form a part of the general trabecular system, are here wanting on all but the principal branches of the main trunk; and even the proper coats of these vessels thin away, until scarcely anything is left, even in veins of considerable dimensions, but the smooth lining membrane covered with a layer of fusiform epithelial cells. The finer microscopic veins consist of nothing else than this membrane with its epithelium; and these end in the before-mentioned lacunæ, in which the membrane and the epithelial cells can no longer be traced, so that nothing separates the blood from the cells of the pulp between which it meanders.

Mr. Gray's next section treats of the *Blood* of the spleen; but as it seems to us much more appropriate to proceed first to the proper anatomical components of the organ itself (since the question of the alterations which it effects in the blood passing through it, would bring us at once to the consideration of its function, which can be only fittingly discussed after its structure has been fully inquired into), we shall pass this by for the present, and go on to the description of the *Spleen-Pulp*, the *colourless* and *coloured* elements of which have to be separately considered. Mr. Gray's account of the former corresponds in the main with that given by Kölliker, Sanders, and others; and he is led by a comparison of the various forms and conditions presented by the components of the splenic parenchyma, to the conclusion at which they had previously arrived—

“Namely, that a continuous process of cell-development, of cell-growth, and

decay, takes place in the pulp of the spleen, during which nuclei are formed, around which a blastema, or, in other cases, a cell-growth is produced, after which the nuclei disappear, and the cell-membrane and its contents become broken up and vanish." (p. 182.)

He has made an important step in advance, however, by experimentally ascertaining that "the parenchyma-cells of the pulp, taken together, exist in *by far larger quantity*, and form a very *considerable part* of the entire bulk of the spleen, in all animals in which the nutrition of their bodies is in a most perfect condition, and more particularly in those in which the addition of new material *exceeds* that required by the waste of the body;" whilst "the parenchyma-cells *not only diminish*, but actually *do not exist at all*, in those animals in which new material has not been supplied in such quantity as was required for the waste of the body."

The resemblance between the parenchyma-cells of the spleen, and the *colourless* corpuscles abundantly contained in the venous blood returning from the organ, is so close, that it is difficult not to regard them as identical; and the passage of the blood through lacunæ in this parenchyma, without any membranous limitation, seems fully to account for the presence of some of its cells in the return current.—The *coloured* components of the spleen-pulp are affirmed by Mr. Gray to be nothing else than *red corpuscles* variously modified.

Blood-discs have been recognised in the midst of the pulp cells, by every one who has microscopically examined the parenchyma; but they have been usually considered as no more forming part of the proper components of the spleen, than they would be held to do in the case of nerve or muscle. Mr. Gray, however, being led by his view of the nature of the splenic circulation, to the conclusion that there is no distinct separation between the current of blood and the substance through which it passes, regards even normal blood-discs as proper constituents of the spleen-pulp, just as he regards the colourless cells of the parenchyma as normal constituents of the venous blood of the spleen. The great majority of the coloured corpuscles, however, are *altered* blood-discs; and the following is his account of the principal changes which they undergo:

"First, some blood-discs may be observed (and these are somewhat numerous at certain periods), whose size is generally smaller (they vary from the 5500th to the 7000th or 10,000th of an inch), their form variable, circular, or more generally of an irregular circular or oblong form, or indented and wrinkled. Their colour is of a deep scarlet or orange-red, and their outline very dark and highly refractive. These changes may be observed either in single blood-discs, or almost as commonly in small or large heaps, or masses of them collected together; and when such occur in masses, they are of a deep red colour, or reddish-brown, quite distinct in colour from heaps of unchanged blood-discs. In other cases, these single discs may be observed to become still more irregular and indented in form, of a darker red colour, and finally subdivide or break up into small, generally circular, minute, dark red, reddish-brown, or black granules; whilst the heap of globules undergo a similar transition into granules, which either exist separately, dispersed in the substance of the pulp, or as masses of such dark red granules. Such, there is no doubt, is the most frequent method by which the blood-corpuscles in the spleen are disintegrated.

"Secondly. There are observed, although rarely, perfectly unchanged blood-discs contained in a cell; these discs present their usual *normal* appearances. . . . In none of these could I ever detect a nucleus on the wall of the enclosing cell,

which is exceedingly fine and delicate, transparent, and homogeneous in texture. The number of the contained discs varies from one to three. I have never observed more of this variety.

"The more frequent form, however, in which the cells containing blood-discs present themselves is the following: They are large vesicles, which vary considerably in size; their average diameter is $\frac{1}{300}$ th of an inch, the largest $\frac{1}{1600}$ th, the larger size being the more frequent. Their form is generally spherical, sometimes oval or oblong. They consist of an external membrane, perfectly transparent, which *sometimes* contains on its wall a distinct nucleus of a circular form, with a central nucleolus, the nucleus being about the size of an ordinary blood-disc. In many no nucleus can be seen. In the cavity of the vesicle may be seen from one to nine or ten somewhat altered blood-discs, that is to say, blood-discs of a circular or somewhat irregular circular form, of a *deep orange-red* colour, with an outer dark refractive margin, which sometimes presents a wrinkled edge; they do not present also the same distinctly flattened appearance that normal blood-discs do. Others are observed, in which the contents consist partly of the above-described altered blood-discs, one or more of them, however, presenting indented edges, as if previously to their breaking up into dark-reddish granules. Lastly, other vesicles may be seen, containing a varying number of small granules, from four or five to twenty or thirty in number; these granules are of an irregular form, with dark refractive margins, and of a pale red, or dark red, or reddish-yellow colour, or the vesicle contains a mass of colourless granules. These structures, in one or the other form, constitute one of the main elements of the pulp.

"But besides these, there are interspersed throughout the whole of its substance granules of varying form and size, in large quantities; and these are precisely similar to those found in the previously-described cells, and which consequently appear as the débris of the disintegrated blood-discs. They vary in size; some are exceedingly minute fine granules, others exist as single corpuscles about the size of the blood-discs, or aggregations of such, or in various sized masses; their colour is a deep red, or reddish-yellow, or black, and their margins are dark and refractive. In some cases these corpuscles are so numerous as to form the chief constituent of the pulp, at other times, again, they exist only in sparing numbers. Occasionally a large number of *reddish crystalline* forms are found in the substance of the pulp, in the place of the above-described corpuscles. They exist either as free crystals, of an acicular form, exceedingly minute in size, which, when seen separately, have a pale red colour, or of masses of such crystals aggregated together, when they present a brilliant red colour." (pp. 186—189.)

All these changes obviously tend towards *disintegration*; and thus, in Mr. Gray's belief, the coloured constituents of the spleen-pulp wholly consist of red blood-discs in various stages of that process. After numerous observations, however, he has only observed, in man, two cases of blood-corpuscles included in cells, and then only in small numbers; so that the degenerating process in the human spleen takes place without any such agency. In the sheep and pig, he has never observed any disintegration of the blood-discs; in the rat, the disintegration shows itself solely in free corpuscles; in the rabbit, on the other hand, it takes place partly in those included within cells; in the horse and ass, both forms of the process may be observed more perfectly and completely than in any other mammalia. These variations are very instructive, both as accounting in some degree for the very discrepant statements which have been put forth on this subject, and also in their physiological bearing.—We draw from a later section on the Comparative Anatomy of the Spleen, the following additions as regards the manner in which the process is effected in Birds, Reptiles, and Fishes.

The spleen of Birds, though of smaller proportional size than that of mammals, is made up of the same elements; and the fact that the coloured portion of the pulp consists of blood-corpuscles in various states of disintegration, is most distinctly traceable. In some instances, the chief alteration is in size and form, the margin and surface of the corpuscle being more or less corrugated, so that its diameter is diminished, whilst its colour is darker than usual; in other cases, the nucleus and cell-wall of the corpuscle remain unchanged, but the colour has faded. These degenerating corpuscles are often collected into heaps, of varying form and size; and these are sometimes enclosed by a membranous investment. In a few cases, Mr. Gray has seen in the spleen of birds a number of small, elongate, rod-like, crystalline bodies, of a pale reddish colour; these either presented a perfectly straight form, or were somewhat curved; they either existed free, or were contained in unchanged or partly-changed blood-corpuscles. In Chelonian and Ophidian Reptiles, both arteries and veins form a distinctly *closed* plexus, both on the surface and also through the interior of the organ; and no trace of disintegration of blood-corpuscles, either singly, or in cells, has been observed by Mr. Gray, in repeated examination of their spleen-pulp, although he states that small, dark-coloured, granular masses, sometimes enclosed in a vesicular envelope, form a considerable part of the parenchyma. In Batrachia, on the other hand, the plexiform arrangement of the bloodvessels does not exist, and the smaller venous channels are mere interspaces in the substance of the pulp; and in these animals, the process of disintegration of blood-corpuscles may be distinctly made out, the large size of the corpuscles rendering the observations more easy and more certain.

“Some of the blood-discs present their usual form, size, and colour, containing in their interior a small circular-shaped granular nucleus. Others are observed of *smaller size*, of *darker colour*, and having a *circular* instead of an oval form. The margins of these present a wrinkled corrugated appearance, the nucleus either remaining at first unchanged, or else losing its granular form, and becoming more pellucid. This wrinkling or crumpling up of the blood-corpuscle is not confined to the margin, but extends throughout its substance, altering the normal form of the blood-disc to such an extent, as not readily to be recognised until the above-mentioned stages have been observed. This wrinkling and corrugation proceeds until the corpuscle becomes an irregular dark red or reddish-brown jagged mass, the substance of which has become changed into a coloured pigment granule. In the interior of some of the blood-discs which are devoid of nuclei, may be seen three or four minute brilliant reddish granules of hæmatin, which sometimes assumes a rod-like form. Similar reddish-brown or black granules, but of larger size, which exist either singly or collected into large masses, are also to be seen disseminated through the substance of the pulp. In *no single instance*, in at least fifty examinations that I have made, examining the organ in every possible variety as regards nutrition, *have I ever been able to observe the existence of the disintegration of the blood-corpuscles in cells*, as described by Kölliker.” (pp. 319, 320.)

Now there is not here the discrepancy, which at first sight appears to exist. For Professor Kölliker does *not* speak of the blood-corpuscle-holding cells as components of the *spleen-pulp* of batrachia; but distinctly states that they are contained in the bloodvessels, and that he has traced them into the vena portæ and its hepatic branches, and even into the vena cava. He adds: “In any case, these facts may be considered as

conclusive of the not unfrequent occurrence and formation of the cells in question within the bloodvessels of the spleen; although it can scarcely be added that they are not probably also formed in the extravasated blood."* Mr. Gray, on the other hand, does not seem disposed to admit that the disintegrating changes, and the formation of cells around blood corpuscles, ever take place within the bloodvessels of the spleen; but regards the disintegrated corpuscles and blood-corpuscle-holding cells, which he may find in the blood of the splenic vein, as proper components of the spleen-pulp, which have found their way into the vessels, through the want of a limiting membrane. (p. 195.) The difference, therefore, does not seem to be so much one of facts, as of the interpretation of them; and if the blood-corpuscle-holding cells are to be frequently found, as Professor Kölliker states, in the splenic vein and vena portæ of batrachia, whilst they are *not* to be found, as Mr. Gray affirms, in the spleen-pulp of those animals, a strong case seems to us to be made out for their formation within the vessels, as affirmed by Professor Kölliker. Mr. Gray seems to have overlooked the significant fact stated by his predecessor, that in *Triton igneus*, whose spleen is tolerably transparent at its margin, the blood-corpuscle-holding cells are frequently to be seen occupying the capillaries in rows, and may be driven into the larger venous channels by pressure. In the spleen-pulp of Fishes, Mr. Gray has constantly met with blood-corpuscles in progress of degeneration, presenting the same double set of appearances as those already described; he has never, however, seen these blood-corpuscles enclosed in cells, although he has occasionally found dark granular masses, such as are formed by the disintegration of the corpuscles, enclosed in a distinct vesicular envelope. In the tench, bream, barb, and eel, he has observed the formation of rod-shaped crystals within the corpuscles; and as similar crystals are found disseminated, sometimes singly, sometimes in masses, throughout the entire substance of the pulp, it is pretty obvious that they have been originally formed within the blood-corpuscles, and are set free by the disintegration of their enveloping membrane.

It may seem to have been substantiated, then, by Mr. Gray's observations, that blood-corpuscles, either singly or in masses, become disintegrated when effused into the spleen-pulp, without the necessity of their inclusion in cellular envelopes; that blood-corpuscles do become enclosed in simple vesicles, which form in the plasma around them, without the existence of a nucleus; and that blood-corpuscles also become enclosed in distinct nucleated vesicles, the formation of a nucleus of the cell preceding, in this case, that of the investing membrane. We must remark, however, upon the whole description, that we are entirely unable to determine, from Mr. Gray's account of his observations, how he distinguishes the corpuscles of the spleen-pulp from those contained within its vessels. Every one who has himself made a microscopic analysis of the spleen, must be aware of the difficulty of discrimination on this point; and we should like to know by what criterion Mr. Gray has made his determinations. Not only Mr. Wharton Jones, but Reinak and other excellent observers, hold the pigment-cells of the spleen to be *sui generis*, and

* *Cyclopædia of Anatomy and Physiology*, vol. iv. p. 764.

altogether deny that they can be regarded as degenerated blood-corpuscles. And all the blood-corpuscles which may be recognised as such, they refer to the capillary passages, not to the pulp of the spleen. Mr. Gray tells us that he has endeavoured to determine by an extensive series of observations, the circumstances under which these changes take place; and he found, that whilst the proportion of blood-corpuscles undergoing disintegration in the spleen-pulp did not seem to have any relation to the period of the digestive process, it was obviously affected by the state of the animals as regards their nutrition; the number of coloured elements (and especially of those blood-discs in which, as they present their normal characters, the degenerating process may be presumed not to have yet commenced,) being much greater in well-fed than in ill-fed animals. We shall hereafter see that further information upon this point may be derived, from the comparison of the blood of the splenic vein with that of the splenic artery. With reference to the question whether like disintegrating changes take place in the blood-corpuscles of other organs, Mr. Gray states that he has never obtained any evidence of them either in man or in other mammalia, in birds, in reptiles, or in most fishes; but that in the bream, carp, and tench, he has frequently noticed the same appearances in the kidneys and liver, as are to be met with in their spleen. It would be a matter of peculiar interest to determine whether the circulation in these organs has anything of that *lacunar* character in the animals in question, which it seems to possess in the spleen of nearly all vertebrata.

The *chemical composition* of the spleen-pulp has been carefully investigated by Mr. Gray, with the assistance of his colleague, Dr. H. M. Noad. We have only space for the most important results of their inquiries, which seem to have been prosecuted on a very extensive scale.—The chief component of the spleen is an *albuminous* product, of which the colourless portion of the pulp is almost entirely made up; as is shown by the effects of reagents upon them under the microscope. A large quantity of colouring matter, identical in all its properties with the hæmatin of the blood, is also present; and from what has been already stated, it scarcely appears doubtful that this is furnished by the disintegrating corpuscles which constitute the coloured portion of the pulp. The rod-like crystalline bodies seem to consist of the hæmatoidin of Virchow. No relation was traceable, by the effects of reagents, between the colouring matter of the spleen and that of the bile. As much as sixteen per cent. of iron was found in the ash, and also no less than forty per cent. of phosphoric acid; and the large proportion of the latter, as well as of the former, of these substances is significant of their derivation from blood-corpuscles, which contain nearly all the phosphorized fats of the blood. The presence of *lactic acid* in the spleen-pulp, as asserted by Scherer, has been verified by Mr. Gray and his coadjutor: but, notwithstanding that they have followed Scherer's process (implicitly, as they believe), and have operated upon so large a scale that in one of their experiments the spleens of twenty-five oxen were submitted to analysis at once, they have wholly failed in detecting either *uric acid*, or its related compound, *hypoxanthine*, the latter of which substances Scherer states that he found in the human spleen at all ages, as well as in the substance

of the heart. The reputation of Prof. Scherer as an analytic chemist is such, that the negative results obtained by Mr. Gray and Dr. Noad do not allow us to entertain a doubt of the validity of his positive conclusions; and we can only suppose that the discrepancy may have arisen, either from some apparently unimportant difference in the analytical process employed, or from some diversity in the mode of nutrition or general condition of the animals experimented on.

The *Malpighian Corpuscles* of the spleen still remain to be considered. As regards their general position and relations, Mr. Gray has little that is new to tell us. Like Kölliker, he considers these bodies to possess an essentially distinct membranous investinent, formed by the interlacement of fibres (chiefly of the white or non-elastic kind) derived from the sheaths of the arteries to which they are attached, the irregular meshes left between these being filled up by an exceedingly delicate and finely granular membrane; and he considers the cellular structure which has been described by Dr. Sanders as existing in it, as a mere optical illusion. He does not tell us upon what animals his observations have been chiefly made, but simply remarks, that in the human subject the pale white fibres are somewhat less distinct and are smaller than in other mammalia. This is a point of considerable importance, as we shall see further on, when the signification of these corpuscles comes to be considered. With regard to the vascular supply of the corpuscles, Mr. Gray does not make any decided advance upon the description given by Müller in 1834; for while he describes some of the arterial branches as occasionally passing through the centre of these bodies, he makes no mention of any other plexus of capillaries than that covering their external surface, which he considers quite sufficient to bring the blood into relation with their contents. Now upon this important point, Kölliker tells us that from a single observation in the spleen of a cat, he is inclined to regard the pulp of the Malpighian corpuscles to be traversed, like that of the Peyerian follicles, by fine (capillary?) bloodvessels.* Dr. Sanders, in a paper communicated to the Physiological Society of Edinburgh, in January, 1852, and published in the 'Edinburgh Monthly Journal,' for March, 1852, stated that by boiling the tissue in acidulated water, drying it, and then cutting thin sections, he had not only determined the passage of arterial twigs diametrically through the substance of the follicles, but had also perceived in their interior "stains of blood, often in linear arrangement, indicating capillaries." Of neither of these statements does Mr. Gray take any notice whatever; and the question does not seem to have at all attracted his attention. Much light has been recently thrown upon it, however, by Mr. Huxley, who, in a paper 'On the Ultimate Structure and Relations of the Malpighian Bodies of the Spleen and of the Tonsillar Follicles,' in the 'Quarterly Microscopical Journal,' for January, 1854, and in the notes to the English edition of Prof. Kölliker's 'Hand-book,' tells us that the existence of a capillary network in the interior of the Malpighian follicles may be made out with the utmost ease by a proper method of examination; all that is necessary being to make, with a sharp knife, a tolerably fine section, containing a Malpighian follicle: to spread it out with needles, adding nothing but a little weak syrup; and then,

* Handbuch der Gewebelehre des Menschen, p. 438.

after placing a glass plate over the section, to apply a gentle and gradual pressure, just sufficient to render the bodies transparent. It is easy, he tells us, by sliding the plate with a needle, to cause the bodies to roll a little on their axes, and thus to convince oneself, by the relative position which the bodies and the vessels assume, that the latter do really pass through, and not merely over, the latter. We have ourselves verified Mr. Huxley's observations on this point; and having had no difficulty in arriving at the same conclusions, we are the more surprised that Mr. Gray should have passed the matter by.

Like his predecessors, Mr. Gray describes the contents of the Malpighian corpuscles as consisting of granular blastema, nuclei (some of them of an elongated oval form, and containing a variable number of pale granules), and nucleated cells, in all essential particulars corresponding with the colourless components of the splenic pulp. The diversities observable in the relative abundance of these components in different individuals and under different circumstances, are held by Mr. Gray, and we think quite justly, to indicate that "a continuous process of cell-development, of cell-growth, and dissolution, takes place in the splenic corpuscles, as in the pulp; wherein nuclei are formed, around which a granular plasma is arranged, and after which, both nuclei and plasma break up and disappear, probably forming the amorphous blastema in which the nuclear structures are contained."

A very valuable series of researches has been made by Mr. Gray, chiefly on cats, rabbits, and rats, with regard to the conditions which affect the development of these corpuscles, and the amount of their contents. The first of these he found to be the *state of nutrition of the animal generally*; the maximum of size being attained in all instances in which the animals had been highly fed, so that a greater amount of new material was added to the system than was required for the expenditure and waste of the body; whilst in ill-fed, and more particularly in starved animals, they were invariably reduced to their minimum of size, and were sometimes almost totally absent. Hence, as the contents of these corpuscles, like the spleen-pulp generally, consist of an albuminous compound, it may be reasonably surmised that they store up a portion of the surplus alimentary material, when (to use Mr. Gray's rather slovenly phraseology) "the system is in a highly nutritious state," restoring it again to the blood during the demand set up under the opposite condition. But further, Mr. Gray found that the *period of the digestive process* greatly modified the state of repletion of these bodies; their contents being most abundant during the latter part of the act and for a short time after its completion, while they are most scanty long after its completion and during its early stages; little increase is produced in them, however, by the accomplishment of the digestive act, in animals which have been for some time underfed. The *nature of the aliment*, again, affects the development of these corpuscles; for when rats which had been previously kept upon a mixed diet, were fed for several days upon boiled white of egg (on which they thrive well), the whole spleen acquired a large size, its weight being to that of the body as 1:250, and the Malpighian corpuscles were peculiarly obvious; whilst in other rats, fed upon lean meat, upon fat, or upon gelatin, on neither of which diets did they

live long, the spleen was found of less than half the size, and its Malpighian corpuscles were so small as not to be visible to the eye. The *supply of water*, moreover, was found to modify the size of the corpuscles, and the fluidity of their contents.

Mr. Gray argues that, notwithstanding the absence of a distinct limitary membrane, and other structural differences, the Malpighian corpuscles should be regarded as analogous to the follicles of ordinary glands; but he seems to overlook the exact correspondence of their contents with the constituents of the pulp in which these corpuscles are imbedded; and he does not attempt to account for the isolation of these contents from the surrounding mass, nor to show what functional relation they bear to it. This question has recently been considered by Remak* and Leydig;† and their views have been adopted by Mr. Huxley, who has confirmed them by his own researches, with which, too, the statements of Mr. Wharton Jones are in accordance. All these observers agree in denying to the Malpighian bodies any proper membranous capsule; but affirm that their boundary consists only of a part of their own tissue, more or less metamorphosed into fibres, together with, in some instances, a layer of proper white and yellow fibres derived from the coats of the arteries to which they are attached; so that the wall of the Malpighian corpuscle is not separated by any distinct line of demarcation, either from its contents or from the surrounding red pulp. As such is even more evidently the case in the lower vertebrata than it is in the mammalia, we cannot but regret that this part of the comparative anatomy of the spleen has been so incompletely worked out by our author. He correctly tells us, indeed, that the Malpighian corpuscles of birds, which are very abundant, are destitute of any fibrous investment derived from the sheaths of the arteries, and are bounded only by "a fine and exquisitely delicate membrane, which is transparent, homogeneous in texture, presenting in some cases an exceedingly fine dark granular texture," as if made up of condensed granular matter resembling that which it encloses; but he states that, although Müller detected these bodies in the Chelonia, and Oesterlen in the naked Amphibia, he has not been able to discover their existence in any animals belonging to the class of reptiles. Now it seems very probable, from the researches of Leydig and Remak, that in those reptiles whose splenic sanguiferous plexus is entirely closed, and whose spleen-pulp contains no disintegrating blood-corpuscles, there are no Malpighian corpuscles; the signification of this fact being, that the whole spleen is, so to speak, a vast Malpighian body, its composition being such throughout, as is found, in the spleens of higher animals, only in the encysted portions. But it is not true that no representative of the Malpighian corpuscles exists in the spleen of Batrachia; for although not distinctly marked out by a special investment, a collection of colourless pulp lies in the midst of the red pulp which forms the cortical portion of the organ, and into which it passes continuously. Among Fishes, various remarkable modifications present themselves in the condition of these organs. The Malpighian bodies do not always hold their usual relation to the arteries; but they sometimes appear to be quite free in the midst of the spleen-

* Ueber runde Blut-gerinneel und über Pigment-kugel-hältige Zellen: Müller's Archiv, 1852.

† Anatomische Untersuchungen über Fische und Reptilien.

pulp; whilst in other instances (as to which Mr. Gray is in accordance with Leydig), their components are dispersed, without any special investing capsule, through the walls of the vessels; and in the cod (according to Mr. Gray), they are scattered through the whole substance of the pulp. It is curious that in fishes generally, the contents of the Malpighian corpuscles, or what corresponds to them elsewhere, are deeply coloured; nevertheless Mr. Gray assures us that, after the most repeated and careful examination in numerous fishes, he has never been able to detect among them either normal or changing blood-discs; and he regards the colouring matter, not as hæmatin, but as a true secretion, formed in distinct nucleated cells, and in some respects corresponding with that of the bile. It is remarkable that similar agglomerations of nucleated vesicles containing coloured particles are found in many fishes on the walls of the bloodvessels of the liver.

Taking into account, therefore, the diversified forms under which the Malpighian corpuscles or their representatives present themselves,—and connecting with this the evidence afforded by the experimental observations of Mr. Gray, as to the functional correspondence between the contents of the Malpighian corpuscles of mammalia, and the colourless portion of their spleen-pulp (as shown in the marked increase of both at the end of the digestive process, and the diminution of both by deficient nutrition),—we give our full assent to the view of their nature advocated by Remak, Leydig, and Huxley; namely, that they are not to be likened to the acini or follicles of ordinary glands, but that they simply consist of portions of the splenic pulp more or less isolated from the rest. We must, as Remak urges, consider the spleen to be formed of two principal constituents; the first being its parenchyma; whilst the second is a super-added fabric of bloodvessels, nerves, lymphatics, elastic and contractile elements. The manner, however, in which the latter are arranged in and about the parenchyma, is, in a manner, accidental; and varies considerably in different animals. The “indifferent tissue” (or granular blastema containing nuclei and cells in various stages of development) may be *intercapillary*, as in the pulp; it may be specially limited to the *medullary* portion of the organ, as in certain amphibia; it may be *vaginal*, that is, dispersed through the coats of the arteries, as in some fishes; or it may be *encysted*, as in the most characteristic forms of Malpighian corpuscles, which are still to be regarded, however, as offsets from the walls of the bloodvessels. The following extract from Mr. Huxley’s paper will show the importance of this view in its relation to general histology, which has been recently dwelt on by several writers, but has been especially enunciated and explained by Leydig.

“There is no line of demarcation to be drawn between the spleen, the lymphatic glands, Peyer’s patches, the glandulæ solitariae, the supra-renal capsules, the thymus, and the pituitary body; but these form one great class of glands, characterized essentially by being masses of ‘indifferent tissue’ contained in vascular plexuses, which, therefore, may well retain their old name of vascular glands. The primary form of these is represented by the solitary gland of the alimentary canal, which is nothing but a local hypertrophy of the indifferent element of the connective tissue of the part, and possesses no other capsule than that which necessarily results from its being surrounded by the latter. A number of such bodies as these, in contiguity, constitute, if they be developed within a mucous

membrane, a Peyer's patch; if within the walls of the splenic artery and its ramifications, a spleen; if within the walls of lymphatics, a lymphatic gland; if in the neighbourhood or within the substance (as in fishes) of the kidney, a suprarenal body; if in relation with a part of the brain, a pituitary body."*

So, according to Mr. Huxley, the tonsil is not a truly follicular gland, as is usually supposed; but is a "vascular gland" developed around a diverticulum of the pharyngeal mucous membrane, its "follicles" being constituted by imperfect septa of rudimentary connective tissue, containing a solid mass of indifferent tissue traversed by capillaries. And even the liver, so far as its parenchymatous portion is concerned, must be ranged (as Mr. Huxley has shown) in the same category; this part being very distinct, both homologically and functionally, from the excretory portion of the organ, as Dr. Handfield Jones's recent researches may, we think, be reasonably considered to prove.

"It seems odd," concludes Mr. Huxley, "that from being a sort of histological and physiological outcasts, the vascular glands should turn out, if this view be correct, to be the most important and extensive class of organs in the whole body, claiming *the gland par excellence*—the liver—as one of their family."

In the section on the *Lymphatics* of the spleen, we find nothing that we need stop to notice; Mr. Gray agreeing with all recent histologists in the statement, that the lymphatics of this organ present nothing which is in any respect peculiar, either as to size or distribution, and that they cannot be considered as performing the special function that was formerly assigned to them, of serving as the excretory duct for the removal of the products of its elaborating action. Neither is there anything novel in his description of the *Nerves* of this organ; and from the section on its *Comparative Anatomy*, we have already drawn the most important facts relating to its intimate structure; so that, although many very interesting details remain unnoticed, these are of a kind which have no direct bearing upon the principal object of the whole inquiry—namely, the *Function* of the spleen. To this, then, we shall now proceed; taking in connexion the sections on the *Blood* and on the *Physiology* of the spleen, which, in Mr. Gray's essay, are very widely, and somewhat unaccountably, separated, so as to involve a great amount of repetition that might easily have been spared.

The Blood of the Splenic Vein† has been examined by Mr. Gray, both microscopically and chemically; by the former method, in several hundred specimens from different animals; by the latter, in no fewer than a hundred and eleven samples obtained from eighty healthy horses.‡ It was compared with the arterial blood and with the ordinary venous blood of the same animals, or sometimes with the blood of the mesenteric vein;

* Quarterly Journal of Microscopical Science, vol. II. p. 82.

† Mr. Gray means by this, *not* the blood drawn from the splenic vein during the continuance of the circulation, but that which has been expressed from the organ after its cessation. The latter, for reasons which will appear further on, may be far from furnishing a fair sample of the former: and we think that we shall succeed in showing that it must often be very different.

‡ We do not suppose Mr. Gray means to affirm that he has made 111 complete analyses of splenic blood, besides those of aortic and jugular blood, although his words (pp. 137 and 177) seem fairly to convey that idea. We cannot find that he has made more than eleven analyses of splenic blood, in which the amounts of its principal constituents have been determined; and the total 111 seems to include the analyses of other blood which were made for the sake of comparison, as well as all the partial analyses of splenic blood.

and the conditions of the observations were so varied, as to test the influence of *age*, the *period of the digestive process*, and the *general state of nutrition*, on the comparative amounts of their principal components. The general results—allowance being made for these modifying influences—were found to be so remarkably uniform, as apparently to leave no doubt whatever that they represent the general facts of the action of the spleen upon the blood which passes through it; and they seem also to be in precise conformity with the results of microscopical and chemical analysis of the components of the spleen itself; whilst the determination of the *conditions of variation* in the proportions of the principal constituents of the splenic blood, helps us to account for the differences in the analytical results obtained by previous observers.—Here again, however, we find ourselves called upon to take exception to the very loose and slovenly phraseology in which these conclusions are expressed. We have been continually obliged to fall back upon the tabular statements, in order to appreciate the real import of the terms in which their general results are summed up; a principal source of confusion (which we give as a sample of the whole) being that the word *amount* is used by itself in three different senses—namely, the *absolute amount* of each component, the *amount of its increase*, and the *amount of its diminution*—and this without any other indication than can be derived (and this not always with certainty) from the context.

The first point investigated was the *absolute quantity* of blood normally contained in the spleen, and the circumstances under which this undergoes variation. In 12 well-fed horses in which this was examined, at from 4 to 48 hours after a meal, the extremes were 724 and 2641 grains; the *maximum* being found about 16 hours after feeding, when the digestive process may be considered to have arrived at its completion; and the *minimum* at 48 hours, when the new material has probably been expended in the processes of nutrition, &c. In ill-fed, and still more in starved horses, the reduction in the quantity of blood contained in the spleen is most remarkable; for in one of the former, sixteen hours after taking food, there were only 371 grains, and in one of the latter, only 50 grains. As might be expected from the previous results, it was found that an increase of the entire bulk of the blood, by transfusion from another animal, produced a considerable augmentation of the amount of blood in the spleen; and that, on the other hand, by general loss of blood, that quantity underwent a special reduction. So, again, a temporary augmentation of the entire mass was found to take place after the ingestion of large quantities of fluid, especially when absorption proceeded rapidly. And any cause which obstructs the return of the blood through the splenic vein—such as disease of the liver, heart, or lungs, occasioning an impediment to the flow of the blood through either of those organs—produces an enormous increase in the amount of blood contained in the spleen: thus, a well-fed, healthy horse having been kept under the influence of chloroform for half an hour, during which time the respiratory movements were very imperfectly performed, its spleen was found to contain no less than 9000 grains of blood. That the vascular distensibility of the spleen varies greatly in different animals—being much greater in the Ruminants, for example, than in man—is a fact that has long been known; but Mr. Gray has

added much to our previous information upon this point; and one of the most interesting facts which he has noted in regard to the comparative anatomy of the organ as a whole, is its large size and great distensibility in *diving* animals, such as the seal and the ornithorhynchus, which are liable to have their respiratory circulation temporarily suspended, but which are not furnished with those arterial plexuses and multiplied venous reservoirs, that enable the cetacea to undergo a prolonged submersion without inconvenience. Thus it is obvious that one of the functions of the spleen is the regulation of the *quantity* of blood in circulation, since it acts as a *diverticulum* for the reception of a temporary excess in the whole bulk of the fluid, and also serves as a reservoir to relieve any accumulation which may take place, from whatever cause, in the systemic venous circulation. There is no novelty in this doctrine; but Mr. Gray has furnished much valuable evidence in support of it. One mode of experimenting, however, does not seem to have occurred to him, which would have afforded unexceptionable information as to the degree of vascular distensibility of the spleen in different animals, and would, moreover, have probably furnished interesting results with regard to the conditions of the lacunar circulation—namely, to tie the splenic vein during life, and then to ascertain the amount of blood which the organ contained at different intervals subsequently to the operation, and to investigate the state of its parenchyma, both microscopically and chemically.

The peculiarities in the *microscopic* characters of the splenic blood are stated by Mr. Gray to be—1st. Considerable variations in the *size, form, and colour* of the red corpuscles, corresponding with those already described in the corpuscles which he considers to form part of the splenic parenchyma; 2nd. The *rare or occasional* presence of corpuscle-holding cells, corresponding with those met with in the spleen-pulp; 3rd. The *almost constant* existence of numerous pigment-granules, or masses, or rod-shaped crystals, these also existing either free, or contained in cells, as in the spleen-pulp; and 4th. The *constant existence* of a large number of *colourless corpuscles*, some of which are in the condition of nuclei, whilst others present more or less distinct traces (especially when treated with acetic acid) of an investing cell-wall; both kinds, however, being apparently derived directly from the splenic parenchyma. These peculiarities were constantly found to distinguish the blood of the splenic vein from that returning from any other organ; and no variations were discoverable by the microscope, either in the colour of the blood-discs, the number of coloured granules, or the proportion of the colourless corpuscles, according to the time that had elapsed since the ingestion of food.

The results of the *chemical* analyses performed by Mr. Gray (with the assistance of Dr. Noad) are tolerably satisfactory as to their general uniformity; at the same time that they present variations which can be frequently referred to certain modifying influences. It is a note-worthy deficiency in the account of these researches, that the mode of analysis adopted by Mr. Gray is not stated. It is well known to all who have attended to this subject, that the proportions of fibrin, corpuscles, albumen, and salts, that may be determined to exist in any given sample of blood, will vary greatly according to the method which has been followed; and there is perhaps none that can be relied on as giving absolutely true

results.* Of course, where the same method has been employed throughout (as we presume to have been the case in Mr. Gray's researches), the results admit of mutual comparison; but they cannot be fairly compared with those of any other analyst, unless it is known that the like method has been followed by both.

The total amount of *solid matter* contained in the blood of the splenic vein is affirmed by Mr. Gray to be very considerably *less*, under ordinary circumstances, not only than that of arterial blood, but also than that of ordinary venous blood, which usually shows some diminution in this respect; this reduction, however, does not take place in all the components of the blood, being usually confined to the crassamentum, whilst the solids of the serum may even undergo an increase. When the question was still further narrowed by the separation of the several components of the blood, the very remarkable result was obtained, that the proportion of *red corpuscles* in splenic blood does not, under ordinary circumstances, average much above *one-half* of the proportion contained in arterial or in other venous blood. This reduction far exceeds that which presented itself in Bécclard's analyses, the proportion of red corpuscles in splenic blood being stated by him as about *four-fifths* that of arterial blood, and about *nine-tenths* that of ordinary venous blood. The discrepancy is probably to be attributed, in part, to the mode in which the blood was obtained; but may be partly accounted for by differences in the conditions of the animals experimented on; for it was found by Mr. Gray, that the greatest reduction takes place at the time of the greatest turgescence of the spleen,—namely, about the period of completion of the digestive process, whilst the amount of change observed at an early period after the ingestion of food is very trifling; and in starved animals, *no change whatever* is detectible.

The proportion of *fibrin* contained in the emerging blood of the spleen is generally much larger than that contained either in arterial or in ordinary venous blood; the average of twelve experiments giving 6·4 parts per 1000, which was just *double* the average for the arterial blood, and *one-third more* than the average for the ordinary venous blood, of the same animals. Very marked variations presented themselves, however, the *maximum* being 11·53 per 1000, whilst the *mininum* was but 2·5 per 1000; and these variations had no perceptible relation to the period of the digestive process. Mr. Gray thinks that the increase of fibrin bears some relation to the diminution of the corpuscles, but his tables by no means bear out this notion; for although in the blood which presented the above-named maximum of fibrin, the corpuscles were reduced from 188·4 to 60·0, or less than *one-third*, another sample gave nearly the same proportion (10·88) of fibrin, with a far less reduction in the corpuscles; whilst another sample, with a reduction in the amount of corpuscles from 104·8 to 27·93, or not much more than *one-fourth*, yielded only 4·31 of fibrin. Mr. Gray states that the blood of ill-fed or starved horses always affords a very considerable proportion of fibrin, as compared with that of well-fed animals; but this assertion is by no means consistent with his own analyses.

The increase in the proportion of *albumen* in the splenic blood was by

* See the writer's Principles of Human Physiology, fourth edition, pp. 149—152.

no means so remarkable, especially as compared with ordinary venous blood: the average of ten analyses giving 60·0 parts per 1000, whilst in five of these animals the albumen of the arterial blood averaged 37·2 parts per 1000, and in four others the albumen of ordinary venous blood averaged 54·0 parts. The extremes, however, were much wider apart in the case of splenic blood; the *maximum* having been 83·3 parts, while the *minimum* was 35·3 parts. In this last case, which was the one in which the corpuscles were unaltered, there was an *absolute diminution* of the albumen, the arterial blood having contained 40·9 parts of that constituent. The conditions of this variation have not been made altogether clear by Mr. Gray's researches. He thinks that a marked increase in the amount of albumen is generally concurrent with a diminution in the amount of red corpuscles; but that this is not the case in the latest stages of the digestive process in well-fed animals, the corpuscles being then greatly reduced, while the albumen shows but a very trifling augmentation. The number of analyses appears to us far too small, however, and the individual discrepancies far too great, to admit of any such generalization.

The serum of splenic blood is stated by Mr. Gray to be distinguished by the presence of *colouring matters*, in sufficient quantity to impart a reddish-brown tinge to the residue left on evaporation, and to the various substances extracted from it; a fact which harmonizes with the previously-cited results of microscopic examination. He does not consider that the presence of neutral albuminate of soda, which is indicated by the formation of a whitish flocculent precipitate on the addition of about twenty times its own bulk of water, is at all peculiar (as Lehmann supposed) to splenic and hepatic blood; since he found a like deposit, in much greater quantity, in the serum of the arterial and of the jugular blood of the same animals.

The saline constituents of splenic blood do not show any marked peculiarity, either as to quantity or quality, save that the proportion of iron in the crassamentum is much greater than in either arterial or ordinary venous blood. Mr. Gray speaks of its *amount* as increased; but as the whole crassamentum is largely diminished, we apprehend that the increased percentage of iron does not do more than compensate for this reduction, probably not so much; for whilst, as it seems reasonable to suppose, the increased proportion of iron in the crassamentum is due to the dissolution of red corpuscles in the spleen, a part of the iron thus set free must be imparted to the serum through which the granules of altered hæmatin are diffused.*

Connecting the foregoing results of the examination of the *splenic blood* with the results derived from the microscopical and chemical examination of the spleen itself, Mr. Gray considers himself to be justified in drawing the following conclusions as to the influence exerted by this organ upon the *quality* of the blood which passes through it. First, as to the red corpuscles:

* Mr. Gray says (p. 357), that "the blood of the spleen contained in the greater majority of cases a much larger quantity of iron than was found either in the blood entering the gland, or in other venous blood;" but this statement is not justified by any of the analyses which he gives.

"The occasional, and in some animals the constant, occurrence of normal and changing blood-globules in the substance of the pulp, and their partial conversion into coloured pigment granules, or crystalline forms, the chemical analysis of which has shown to be identical with the hæmatin of the blood; the arrangement of the bloodvessels, as admirably adapted to admit of the occurrence of these changes under certain circumstances; the frequency of their occurrence throughout nearly the whole of the vertebrata,—all these facts, I think, are in exact harmony with the results of the analyses of the splenic blood, as far as the diminution of the blood-globules is concerned. They clearly show that, in all animals, under certain circumstances, the spleen modifies the constituents of the blood-discs during their transit through the organ, retaining them for a time in the pulp tissue, and changing the elements of which they are composed." (p. 356.)

It is, however, only when the vascular turgescence of the spleen is such as to cause an extravasation of blood into its pulp, that the corpuscles are thus melted-down: and thus it is that the process is specially observed to take place as a normal occurrence in well-fed animals, at the time when the digestion of a recent meal is introducing a fresh supply of solid matter into the circulation; and that the blood returns from the spleen with little or no change in this respect, in animals whose blood has been impoverished by starvation. Mr. Gray's hypothesis with regard to the alteration in the proportion of *iron*, which he considers that the spleen effects in the blood in its passage through it, is vitiated by the uncertainty which attaches to the assumptions on which he bases it. He supposes that the red pulp of the spleen withdraws iron from the blood at certain epochs, to reimpart it to the circulating fluid at other times; and that the *unaltered* blood-discs of the emerging blood receive an extra charge of this component. The large proportion of iron in the crassamentum of splenic blood, however, seems to us to be readily accounted for by the fact, that this crassamentum does not consist of normal corpuscles alone, but that it is made up of blood-discs in all stages of degeneration, which retain their hæmatin, whilst they part with their other contents; so that an unusually large proportion of hæmatin and its metamorphic derivations will naturally be present, part of it still within cells, but another part probably diffused in the condition of pigment granules. A proportional excess of iron in the crassamentum, moreover, is far from implicating (as we have already pointed out) an absolute excess of iron in the whole mass of splenic blood; so that on neither point do we consider that Mr. Gray's hypothesis can be considered as otherwise than purely speculative.

Mr. Gray considers that the facts which he has collected warrant the conclusion, that the colourless elements of the spleen-pulp and of the Malpighian bodies serve as a sinking-fund for *albuminous* materials, during those conditions of the system in which the supply exceeds the demand; a development of cells and nuclei taking place at the expense of the surplus albumen of the blood during the later stages of the digestive process; and a deliquescence of these lowly-organized forms of tissue occurring in the intervals, whereby the proportion of albumen in the blood which passes through the spleen is augmented. He further supposes that part of the increased amount of albumen usually found in splenic blood, may be derived from the colourless components of the red corpuscles which have undergone disintegration in the organ. To this

source, rather than to any direct change effected by the tissues of the spleen in the blood which passes through it, he is inclined to refer the increase in the proportion of *fibrin* usually presented by splenic blood; but, as already remarked, there is an entire want of conformity between the two classes of facts, as shown in Mr. Gray's own table. Thus in Table II., No. 10 (p. 157), we find the fibrin raised from 0·78 (arterial) to 10·3 (splenic), whilst the corpuscles exhibited *no diminution*; whilst in No. 8, in which the corpuscles were reduced from 104·80 (arterial) to 27·93 (splenic), the proportion of fibrin was only raised from 1·79 to 4·31.—With regard to the function attributed to the spleen by Professor Kölliker, of specially preparing pigmentary matter for the bile, by the disintegration of blood-corpuscles, Mr. Gray shows that the idea derives no support from fact; since (1) there is no special chemical relation between the colouring matter of the splenic blood and the pigment of bile; (2) the removal of the spleen in animals does not affect the colouring matter of the bile, which is as abundant after as before the operation; whilst (3) the bile-pigment is generated before the development of the splenic vein in the chick. He does not deny, however, that part of the free colouring-matter of the splenic blood may be changed into bile-pigment; though this, for the reasons just stated, cannot be the special object of the disintegration of blood-corpuscles in the tissue of the spleen, as Kölliker supposed.

Thus, according to Mr. Gray, the Spleen serves to balance alike the *quantity* and the *quality* of the blood; and is specially adapted for this function by its connexion with that part of the vascular system which is concerned in the introduction of new material into the circulation. This function, however, is so far from being absolutely essential to life, that the complete removal of the organ does not seem to be attended with any injurious consequences; as Mr. Gray has ascertained by his own experiments, which confirm those of many previous inquirers. He does not speak of having observed any symptoms of plethora, such as those noticed by Dobson; but he tells us that two of the cats on which he operated, having been kept for a considerable time, improved much in condition, growing to a much larger size than other uninjured cats of the same age. And he supposes that the function of the spleen, in so far as this consists in the disintegration of the red corpuscles, and in the increase of the albumen of the blood, is taken on by other parts and organs; since there is usually found to be a slight difference in both these particulars between arterial and ordinary venous blood; while the general hypertrophy of the tissues just noticed as an occasional result of the removal of the spleen, would seem to show that the augmented amount of albumen is stored up in the system generally, when it cannot be received by the spleen.

“Although these elements,” he remarks, “cannot as readily, under the above conditions, be restored to the blood, as if the spleen had retained them, to be used at every occasion or requirement of the animal, still their removal from this fluid, where they exist in great excess, serves to effectually prevent the inconvenience which a too great accumulation of them in the blood would certainly occasion.” (p. 372.)

Fully admitting the force of all the facts upon which Mr. Gray has attempted to build up his physiological induction, we must yet confess

that he does not appear to us to have by any means succeeded in affording a complete solution of this obscure but interesting problem. There is a difficulty which does not seem to have presented itself to him, arising out of the very magnitude of those alterations in the constituents of the blood, of which he affirms that the spleen is the instrument. In Table IX. (p. 177), which gives the average results of 111 analyses of the aortic, jugular, and splenic blood of the horse, we find the following to be the amounts of water and of solid constituents in each respectively:

	Aortic.		Jugular.		Splenic.
Water . . .	789·14	...	793·42	...	829·81
Solid matter .	210·86	...	206·58	...	170·19

Thus, whilst arterial blood undergoes a trifling reduction in the amount of its solids, during its passage through the capillaries of the general system, the blood which is transmitted to the spleen loses, on the average, 40·67 parts, or *nearly one-fifth*. And since, in many of the experiments, the loss was very trifling, it must have been far more than this in a large number of instances, as, for example, in No. 8 of Table II., in which the albumen, fibrin, and red corpuscles of aortic blood amounted to 145·85, whilst the same components of the splenic blood amounted only to 73·47; the difference thus being 72·38, or *nearly one-half*. Now the question which we affirm to be altogether unsolved by Mr. Gray's researches, and to be altogether passed by in his physiological conclusions, is twofold:—1. What becomes of all the solid matter of the blood, which is thus apparently kept back by the spleen?—and 2. How is it that this reduction in the amount of solid matter does not produce a greater effect upon the general mass of the solid constituents of the blood? We know well, that *at the ordinary rate of the circulation*, the whole amount of blood in the spleen must be renewed *many times in the course of a single minute*; and all the blood of the body must be submitted to the same operation *many times in an hour*. If, then, the spleen keep back one-fifth of the solid contents of every pound of blood that passes through it, the organ must soon draw into itself nearly the whole mass of albumen, fibrin, and corpuscles contained in the circulating current; and the blood must be impoverished in a corresponding degree. Our readers will easily apprehend our meaning, if they will call to mind the process by which a large receiver may have its air exhausted by a very small pump, provided that the working of this pump be kept up for a sufficient length of time. Now in this case, all the air which has been drawn from the receiver by the pump, is discharged above the piston as fast as it enters beneath; and consequently there is no accumulation. But in the case of the spleen, there is no such outlet; no means can be traced whereby the withdrawn materials can be disposed of through any other channel; and we seem reduced to the inference, that they are solely, or at least chiefly, applied to the augmentation of the substance of the organ itself. But although this does undergo a certain increase, the amount of such increase is as nothing, compared to that which must result from the retention of one-fifth of all the solid matter of the blood flowing through the spleen at the ordinary rate. And, conversely, the amount of reduction in the solid constituents of the whole mass of blood, by the instrumentality of the

spleen, is so small as to be inappreciable; instead of immediately manifesting itself, as it must have done, if the current flowing through the spleen at the ordinary rate, even for a limited time, had suffered a deprivation of one-fifth of its solid constituents. We cannot, ourselves perceive any other escape from this dilemma, than by the assumption,—which we admit to be purely hypothetical, but which seems to us to be the only hypothesis that will at all meet the facts of the case,—that a large part of the current of blood through the spleen is retarded, almost stagnated, at the time when the organ is in fullest action. And if we seek for a cause for such stagnation, we think that one may possibly be found in the increase of pressure within the tributaries of the vena portæ, arising from the absorption of a large quantity of new alimentary material into the mesenteric vein. If that pressure should so augment as to antagonise (or nearly so) the pressure within the splenic artery, a stagnation of the blood-current in the venous reservoirs of the spleen will be the consequence; and the increased pressure will, at the same time, give rise to the escape of red corpuscles from the lacunar system of the spleen into its parenchyma, which Mr. Gray affirms to be a normal phenomenon in well-fed animals. The well-known fact that the splenic vein is destitute of valves, seems to us to add some weight to this hypothesis; since an excess of *vis à fronte* over the *vis à tergo* may cause not only a stagnation of blood in the veins of the spleen, but may even permit a reflux into them from other parts of the portal system. And thus it may happen, that the whole mass of blood obtained from the spleen after death, may differ widely in composition from the blood which flows back from the splenic vein; for it would include the portion which has been long stagnant in the spleen, as well as that which the organ has just before received from the arteries; it being only, in fact, when no such stagnation occurs, that the one will properly represent the other. It is quite conceivable, moreover, that even the blood drawn from the splenic vein during life, might not represent fairly that which is normally returning from the organ; since the relief of the backward pressure afforded by the free emission of fluid, will naturally allow a portion of the stagnant blood of the spleen to flow onwards, and thus to alter the character of the discharged sample.

We offer these suggestions as the only helps that occur to us towards a solution of the difficulty in question. One other possible fallacy, however, seems to vitiate the averages of Mr. Gray's analyses. It is quite clear that whatever the spleen *withdraws* from the blood, it must have *given back* the same amount by the time that it returns to its previous dimensions; and it seems to us likely that, whilst the period of withdrawal may be brief, the period of restoration may be long, or *vice versa*. The former may probably be the case with the red corpuscles, which are withdrawn by extravasation, whilst their contents are given back by slow disintegration. The latter may be the case with respect to albumen, whose appropriation by the process of cell-growth may be slow, whilst its return to the blood, by the bursting or liquefaction of its containing tissues, may be rapid. Thus, in any small number of analyses, the disappearance of corpuscles and the increase of albumen would be the ostensible change; whilst a series made upon a sufficiently long succession of samples

of the blood returning from the spleen, might show that this was antagonised by a converse operation, less in amount, but distributed over a longer period. At any rate, we may safely demand that, before any physiological conclusions whatever be drawn from such tables as Mr. Gray's, the fact shall be accounted for, that, notwithstanding the organ constantly retains in its substance as much as one-fifth (on the average) of the solid matter of all the blood passing through it, it does not undergo more than a limited and occasional increase in dimensions, returning in the intervals to its usual size, while the general mass of the blood is not affected in any appreciable degree by this withdrawal of its most important materials.

Mr. Gray's hypothesis of the mode in which the alteration in the proportions of the principal constituents is effected, remains to be considered. The amounts of these contained in the aortic, jugular, and splenic blood of the same animals, are given in some detail by Mr. Gray, in Table IX.; for our purpose, however, the following summary of averages will suffice:

	Aortic.	Jugular.	Splenic.	Difference between aortic and splenic.
Fibrin	2·30 ...	4·20 ...	6·54 ...	+4·24
Corpuscles	157·20 ...	136·80 ...	88·58 ...	—68·62
Albumen	42·00 ...	54·40 ...	63·00 ...	+21·00
Saline, fatty, and extractive matters	9·36 ...	11·18 ...	12·07 ...	+2·71
Total solids	210·86 ...	206·58 ...	170·19	

Now, if the amounts of albumen, fibrin, and extractive, which present themselves in excess in splenic blood, as compared with aortic, bore any kind of correspondence to the amount of corpuscles which disappear, there might be some ground for supposing the addition of the former to be the result of the disintegration of the latter. But such, on Mr. Gray's own showing, is so far from being the case, that their united increase amounts to no more than 27·95 parts, whilst the diminution in the corpuscles is 68·62. Besides, there appears to us to be strong evidence that the two sets of phenomena have no direct relation, one to the other: for, as we have already remarked, a comparison of the results of individual analyses shows no constant correspondence between them; and besides, although the changes which take place in the colourless portion of the splenic parenchyma might be conceived to be influenced by those occurring in the coloured portion with which it is so intimately intermingled, it is scarcely conceivable that the changes in the interior of the Malpighian bodies (with which, as argued by Mr. Gray himself, those of the colourless parenchyma correspond) are in any way affected by the disintegration of blood corpuscles; more especially since comparative anatomy shows us that the colourless parenchyma may perform its functions without any diffusion of red corpuscles through its substance. We are inclined to believe, therefore, that the office of the *colourless* parenchyma of the spleen is not only to serve as a storehouse for the surplus albumen that finds its way into the circulation on the completion of the digestive process, but also to exert an *assimilating* action upon it, whereby it is rendered more fit for the nutrition of the tissues; and of this assimilating action, we deem the generation of fibrin to be one of the results. And if it be true,

as we have elsewhere suggested,* that one special function of the red corpuscles is to assimilate or prepare that peculiar combination of materials which is required for the nutrition of the nervo-muscular apparatus, the disintegration of these corpuscles in the splenic parenchyma may answer the two-fold purpose of regulating their total proportion in the mass of the blood, and of diffusing through the liquor sanguinis the materials which the nervous and muscular tissues are to draw from it for their own development.

We have thus endeavoured to present our readers with a fair account of Mr. Gray's researches; to show what are the points which they may be considered to have established; to state with explicitness the problems which yet remain unsolved; and to indicate the directions wherein, as it seems to us, the elucidation of these may be sought with the greatest prospect of success. We trust that we shall not be thought to have undervalued Mr. Gray's labours, because we hold some of his conclusions to be less definite and unobjectionable than he has himself supposed them to be. On the contrary, it is in the very suggestion of yet higher questions than those which he believed himself to have answered, that much of their merit in our eyes consists; and we trust that he will not consider his work accomplished, until he has attained to a satisfactory solution of these, and of others yet beyond, which may suggest themselves in the course of his further researches.

William B. Carpenter.

REVIEW II.

Lettsomian Lectures on Insanity. By FORBES WINSLOW, M.D., D.C.L., late President of the Medical Society of London.—London, 1854. 8vo. pp. 160.

WHEN the London Medical and Westminster Societies were amalgamated in 1850, the council, in order to honour the memory of Dr. Lettsom, the founder and benefactor of the parent institution, established two lectureships, to be held annually by a physician and surgeon. These are the Lettsomian Professors of Medicine and Surgery. Dr. Owen Rees was nominated to be the first occupant of the chair of medicine, and Dr. Forbes Winslow was elected to be the second. It was natural to expect that Dr. Winslow would direct the attention of the Society to the subject of insanity. From an early period of his professional studies, he has investigated mental phenomena theoretically, and treated the aberrant forms of mental action curatively; and as the active and enterprising founder and editor of the 'Journal of Psychological Medicine' has manifested a warm interest in the advancement of mental pathology. Dr. Winslow has arranged the results of his researches and experience under three heads, constituting three lectures. The first is entitled 'The Psychological Vocation of the Physician;' its objects are to demonstrate the advantages to the theory and practice of medicine which flow from a more general and accurate knowledge of the science of mind on the part of the physician, to establish the close connexion between the two sciences,

* *Principles of Human Physiology*, fourth edition, p. 187.

and to illustrate the true philosophic character of the medical practitioner, The second lecture is 'On the Medical Treatment of Insanity;' and the third 'On Medico-legal Evidence in Cases of Insanity.' The two latter are therefore especially practical, the first is more nearly related to medical deontology. In it the applications of mental philosophy and logic to the daily routine of medical practice are demonstrated, and the advantages which the wise physician can confer on his patients by *moral* remedies are set forth. Hope, ease of mind, and the calm influences of religion, are powerful means of action on the organization, and are in the hands of the medical practitioner to a large extent. When all human means fail—and sooner or later they certainly must fail—to ward off the most dreaded termination of sickness, when nothing more remains to be done than to "await the inevitable hour," it is still within the psychological vocation of the physician, alike to soothe the parting moments of the dying, and make the solemn event a useful lesson to the survivors. Again, it happens from time to time that the influences of religion and moral training, and the consolations which the Christian derives from his faith, are barren and unfelt, from certain morbid conditions of the corporeal functions. In such cases it is of vast importance that the physician be able to detect these conditions, and learn their exact relation to that bondage in which the soul is held; otherwise crime and despair may overwhelm it. Illustrations of these views are given, and the true psychological vocation of the physician in this respect is fully shown. The following quotation will illustrate Dr. Winslow's enlarged views and eloquent diction:

"Finally, I would observe, that of all the subjects that can occupy the attention of the philosophic physician, none equals in importance or in grandeur those which I have had the honour of recommending to your special attention. What can compare in dignity, in sublimity, in comprehensiveness, or in the lofty aim of its disquisitions, to the study of the nature and operation of that spiritual essence, upon the right knowledge and cultivation of which depends our happiness, both in time and in eternity? As the mind advances in a knowledge of its own phenomena, the intellect expands, new sources of delight open to us, and the pleasure we experience in the pursuit of these exalted speculations impresses forcibly upon the mind itself conclusive evidence of its own DIVINITY. He who has habituated himself to trace out the numerous applications of mental philosophy to the important subjects of education, morals, and legislation; to analyse the nature of thought, the laws regulating the association of our ideas, the springs of action, the origin of our happiness, the laws of moral science, the nature of the passions, the formation of character, the foundation of our hopes, and the influence of our emotions,—will appreciate the value of this branch of science. The physician will be conscious, as he advances in a knowledge of the constitution of the mind, that his love of truth is growing strong; and whilst, in the spirit of true humility, he acknowledges the limited nature of his intellectual powers, he will, whilst contemplating their grandeur and importance, recognise the GOODNESS AND MAJESTY OF GOD." (p. 44.)

The *second* lecture is a clear and concise epitome of the *medical* treatment of insanity, as distinguished from the *moral*. We believe that not a few will concur with Dr. Winslow in the expression of his regret for the neglect of physical remedial agents by some of those practitioners who possess great and special opportunities for observation and practice. He attributes it principally to "the doctrine promulgated by writers of celebrity—

by men referred to and revered as our authorities and guides in their special department of medicine, that for the cure of insanity moral treatment is entitled to the highest rank," and thus the administration of remedies suitable to the morbid corporeal states has been discountenanced, or rendered a secondary consideration. Dr. Winslow quotes one recent writer as remarking "'when one man thinks himself a king, another a cobbler, and another that he can govern the world with his little finger, *can physic make him think otherwise?*'" Another writer of the same school observes, "'To prescribe for the mad, whilst *its nature remains a mystery*, is to prescribe for a *phantom!*'"

Another cause, well indicated by Dr. Winslow, is the unphilosophical nature of the hypotheses which have been broached with the view of explaining the phenomena of insanity. In the early periods of the history of medicine (as it is even now in some countries), it was attributed to a Divine afflatus, to the Divine wrath, to demoniacal, Satanic, or malignant influence. These views, "in a modified, less offensive, and different form," still hold their ground in the convictions of many, and constitute the basis of the belief that insanity is an affection of the immaterial principle. Dr. Winslow argues eloquently and, we think, very conclusively in favour of the doctrine that it is simply a *disease of the brain*:

"When we assert that the 'functional' or 'spiritual' theory will not bear the test of serious examination—that it is at variance with all *à priori* and *à posteriori* reasoning—that it stands in direct opposition to positive, well-recognised, undeniable data, we are met by the interrogatory, Can you demonstrate to us the specific character of the change induced in the nervous matter, which it is alleged gives rise to mental derangement? and do not the scalpel and the microscope of the morbid anatomist in vain endeavour to ascertain in many cases of positive, violent, and unequivocal insanity, any appreciable structural lesion in the nervous matter, in its investing membranes, or organs in close association with the brain, sufficient to account satisfactorily for the morbid phenomena exhibited during life? One would really infer, from the reasoning and assertions of those who take these spiritual views, and who repudiate the idea of insanity ever being the result of physical change in the condition of some portion of the brain or its appendages, that the encephalon has no specific functions allotted to it; that it is altogether a useless and supernumerary organ; that it was created for no wise purposes; and that, as far as the phenomena of mind were concerned, we could have done as well without as with the brain! If this organ be not the material instrument of mind—if it be not the medium through which the spiritual portion of our nature manifests its powers—the centre of sensation—the source of volition—the seat of the passions—

‘The dome of thought,—the palace of the soul’—

I ask, what *are* its functions, its specific uses and operations?—for what object was this most exquisitely organized and complicated structure formed?—why does it receive so large a proportion of the blood, and why is it so carefully protected from injury? These interrogatories naturally arise in the mind, when we hear so unphilosophical and so unphysiological a theory propounded with reference to the possibility of the mind being *subject to disease apart from all derangement of the material organs with which it is so closely and indissolubly associated*. Can we conceive a more preposterous notion than that sanctioned by high authority, and which inculcates that the spiritual principle admits of being distorted, deluded, depressed, exaggerated, perverted, exalted, independently of any form of bodily disease, or modification of nervous matter?" (p. 52.)

The anatomical objection appears always, at a first glance, to be the

most insuperable, but a more mature consideration of the subject will lead to the conclusion, that structural change so important and extensive as to be *easily* discoverable by the anatomist, cannot be reasonably expected to be present in the cerebra of the insane, so long as the mental phenomena are simply *disordered* and not abolished. A certain integrity of *structure* is necessary for the manifestation of even the phenomena of insanity. The phenomena themselves, however, offer the most conclusive data in favour of the cerebral origin of the disease, and the practitioner whose sphere of labour brings him into constant intercourse with the insane would, it might be supposed, fully corroborate these views; but such is not the fact. It is not, therefore, surprising that laymen should oppose the *cerebral* as opposed to the *metaphysical* theory of insanity. Yet no man of common sense, whether practitioner or not, doubts that he thinks with his brain, or that in insanity the brain is affected; and no metaphysician, except a solitary eccentric thinker, will deny the undeniable principle that the brain is the organ of the mind. This singular discrepancy between the premises and the necessary conclusion is of great importance in mental philosophy and pathology, and deserves a more special examination. We believe it is wholly due to the want of a clear and satisfactory system of cerebral physiology. The metaphysician does not possess such a system, nor does he seek to establish it; but the metaphysician guides public opinion, and the general public, with the metaphysician, therefore, ignores cerebral physiology. We put this question one day to a profound and lucid thinker of this class: Has mental philosophy or metaphysics contributed anything to the elucidation of the nature and cure of insanity, or of other aberrant forms of mental action now prevalent? His answer necessarily was, that neither had contributed anything. The writer of an able essay on Locke's Character and Philosophy, in a recent number of the 'Edinburgh Review,' happily illustrates the reason why metaphysical speculations are so sterile *quoad* medical art, and so devoid of all practical application to the needs of everyday life. He remarks:

"The metaphysician, above almost any other thinker, must . . . draw from his own resources; patient excogitation must be his great instrument. Indeed, all great thinkers will rather delight in this than in mere acquisition; it ever has been, and ever will be, their *characteristic*. But, then, to be safe, such self-reliance must be accompanied with a careful survey of what has been done by others in the same field."*

Thus one-half the requisite for inductive inquiry is omitted—namely, *patient observation*; and one-half the object to be observed is left out—namely, the cerebrum and its mode of action, as the organ of thought. The "patient excogitation," which this able writer indicates as the great instrument of metaphysical research, has been wielded in Germany by the (perhaps) greatest intellects that have appeared in any age, and yet what has been the result? In this same article we find that Sir James Mackintosh amusingly characterized this German philosophy as "accursed," and Victor Cousin as "detestable!" We are sure all who, like Sir James Mackintosh, have "endeavoured to master" it, will agree with the conclusion regarding it at which the Edinburgh reviewer has arrived. He likens the inquirer to a lover of the picturesque in a mountain region,

tempted to ascend some unknown peak on which the clouds still rest, assured they will shortly clear off. The early progress is delightful, as he glances back from time to time over the ample valley radiant in the sunlight: but when he has got into the loftier regions, and approaches the object of his aspiring ambition, circumstances are changed:

"He can see nothing but a rolling cloud of vapour, which hides every object ten inches from his nose, and after standing wetted to the skin, and shivering in 'darkness visible' for a couple of hours or so, in which the envious clouds still envelope him—now and then teased, perhaps, by a momentary rent in the veil, which seems to show him something, but too transiently to let him know what—he descends, and is glad to catch a glimpse of things in sunlight again. But for any purpose of pleasure or knowledge in ascending these cloudy regions, he might as well have sat himself down at the base of the mountain, and drawn a thick cotton night-cap over his head."

Such, we are assured by the reviewer, is the general estimate both in England and on the Continent, "of the philosophical value of a vast deal of German philosophy since Kant's time." Patient excogitation has here, then, been worse than useless.

Various reasons may be alleged for the entire neglect of cerebral physiology by professed metaphysicians. Firstly, a thorough *practical* knowledge of neurology is necessary to its investigation; secondly, keen powers of observation. Either of these is rarely possessed by speculative thinkers; very rarely indeed are they combined in one mind. The powerful thinkers, of whom "patient *excogitation*" is a special characteristic, are too much abstracted from the common affairs of life to give that practical turn to their studies which is required to make them useful to curative art; and have too little taste for that minute and incessant *observation* which is necessary to make the accomplished neurologist or cerebral physiologist. They, therefore, almost with one consent, leave this important half of their subject to the physician and physiologist, and turn to them for guidance and instruction. A moral cowardice and deeply-rooted prejudices have influenced others. Mental philosophy has been cultivated mainly with reference to theology and morals, and it has been a constant object to harmonize its principles with the dogmas of the one and the teachings of the other. Now the study of organization in reference to these has been falsely termed materialism, and the *materialist* has ever been viewed by the theologian as little better than an avowed unbeliever. Religious teaching, therefore, and the dread of being stigmatised as heterodox, have both raised an almost insuperable obstacle to the study of mental philosophy, in relation to organization, by those whose special vocation such study ought to be.

It cannot be denied, we think, on the other hand, that professional views as to cerebral physiology have been deservedly neglected. With one exception—phrenology—they have never been systematized or arranged so as to be attractive or even intelligible to the metaphysician. They have too often repelled, indeed, by their crudeness, by the total absence of the metaphysical element, or by the incorporation with them of a real naked materialism. To *phrenology* cerebral physiology owes much: to numerous *phrenologists* it owes little. The great and fundamental doctrines of a sound and *practical* mental philosophy must be the

fundamental doctrines of phrenology—namely, that the brain is the organ of the mind, and that it is double and multiple. Subordinate doctrines of phrenology must also be incorporated with such a system of mental philosophy as the practitioner desires; but we fear phrenology itself—as a system—is irretrievably damaged by its friends. On the one hand, the presuming empiricism which has been grafted upon it, chiefly by its illiterate cultivators, repels the thoughtful and cautious inquirer; on the other, the avowed materialism of another section of phrenologists has shocked those religious convictions which are deeply seated in man's nature, and are based upon the grandest doctrine of theology—the immortality of the soul. Nor is it possible for the impartial inquirer, rigorously divesting himself of all theological prepossessions (if such they may be termed), to avoid the conclusion, that the naked materialism which declares mind has no existence, is alike opposed to the conclusions of inductive inquiry and of common sense. The decline of phrenology may be, we think, dated from June, 1842, when Dr. Engledue delivered, at Exeter Hall, the introductory address, to a general meeting of the Phrenological Association. In that address Dr. Engledue proposed to substitute the word *cerebration* for mind, which he declared to be nonexistent. All the leading phrenologists strenuously repudiated this doctrine, and a large number withdrew from the association. These steps did not, however, arrest the evil consequences anticipated from Dr. Engledue's avowal, but perhaps rather hurried them on, for phrenology became more and more popular in connexion with zoism, phrenomesmerism, &c., and more and more abhorrent to the inductive philosopher, the metaphysician, and the physiologist.

The imperfections of mental philosophy and of cerebral physiology which we have indicated, are eminently of a nature to puzzle the practitioner. Both sciences develop great truths, and yet they seem to be antagonistic; the truths are thus neutralized, and a practical application is impossible.

So long as, in metaphysical inquiries, the existence of the material organ is practically ignored, so long will the state of that material organ be practically ignored also, in those forms of chronic disease of the cerebrum involving the judgment and the morals. Furious mania, dementia, and idiotcy, are easily connected, both theoretically and practically, with cerebral disease, by both metaphysician and practitioner. It is "*rational*" insanity which is referred with the greatest difficulty to structural disorder, and most easily comprehended as a disease of the mind distinct from organization.

These imperfections undoubtedly constitute the reason why the treatment of the insane has been so contradictory and empirical. It cannot be denied, however, that modern neurology (including cerebral physiology), has very considerably advanced the treatment of mental disorder. For a long series of ages mental philosophy had undivided sway; for an equally long series of years the insane were the victims of cruelties innumerable; and not the insane only, but all that large class of persons of aberrant or weak intellect to whom the mysterious has irresistible charms. In every past age, all kinds of enthusiasts (now left harmlessly to their vagaries), were very commonly the subjects of violence and per-

secution. As to these, cerebral physiology has *indirectly* done much. Were it not for the general enlightenment it has afforded, the spirit-rappers, magnetizers, electro-biologists, and the rest, might experience something more serious from the world than a smile at their busy credulity.

Another important consideration arising out of the hypotheses as to the nature of insanity, is the question of its *curability*. Dr. Winslow very happily shows the importance of a sound theory in this respect. He remarks:

"No right estimate can be entertained of the importance of these investigations unless we apply to the study of the diseases of the brain, and the cure of its disorders, the same enlarged and general principles which guide us in the investigation and treatment of the affections of other organic structures. An error of some magnitude has been committed by those who consider insanity to be a special, uniform, specific, and peculiar malady, justifying us in placing those so afflicted out of the ordinary nosological scale and sphere of medical practice. Again, it is necessary that we should, before being able to appreciate the effect of medical treatment, entertain just and enlightened views as to the CURABILITY OF INSANITY. I now speak from a somewhat enlarged experience, from much anxious consideration of the matter, and I have no hesitation in affirming that, if brought within the sphere of medical treatment in the earlier stages, or even within a few months of the attack, insanity, unless the result of severe physical injury to the head, or connected with a peculiar conformation of chest and cranium, and an hereditary diathesis, *is as easily curable as any other form of bodily disease for the treatment of which we apply the resources of our art.* Can there be a more lamentable error, or a more dangerous, false, or unhappy doctrine than that urged by those who maintain that the disordered affections of the mind are not amenable to the recognised principles of medical science? The existence of so vast an amount of incurable insanity within the wards of our national and private asylums, is a fact pregnant with important truths. In the history of these unhappy persons—these lost and ruined minds—we read, in many cases, recorded the sad, melancholy, and lamentable results of either a total neglect of all efficient curative treatment at a period when it might have arrested the onward advance of the cerebral mischief, and maintained reason upon her seat; or of the use of injudicious and unjustifiable measures of treatment under mistaken notions of the nature and pathology of the disease. My experience irresistibly leads to the conclusion that we have often in our power the means of curing insanity, even after it has been of some years' duration, *if we obtain a thorough appreciation of the physical and mental aspects of the case, and perseveringly and continuously apply remedial measures for its removal.*" (pp. 59—61.)

It has already been abundantly proved how much may be done in cases of congenital idiotcy, hitherto considered to be hopelessly incurable; in analogous cases of so-called incurable cases of insanity, it will sooner or later be shown (we feel confident) that the resources of the medical art have been under-estimated.

It being considered to be an established proposition, that insanity is nothing else than a cerebral disease, we have to inquire what is its nature and seat? Every man who has reflected on the phenomena of his own mind—not as a metaphysician only, but as an observing physiologist—must have noted daily what a varied number of physical or corporeal influences change the action of his mind. As to some of the changes thus induced, he can clearly see, also, that if they were more intense and more prolonged—that is to say, different only in extent and duration,

they would constitute insanity. But they are evanescent, enduring only for the shortest period, and quickly ending in normal, healthy activity of the cerebrum. Some forms of insanity are, indeed, nothing else than transient, momentary states of this kind prolonged during a sufficiently lengthened period, so as to influence the actions of the individual to a degree which renders him no longer a free and responsible agent. Such attacks are, however, so evanescent, that it is obvious no important change in the material organ has occurred. Not so, however, with the persistent forms. As to these, we think Dr. Winslow's views are reasonable, and in accordance with experience. He believes them—

“To be the result of a *specific morbid action of the hemispherical ganglia, ranging from irritation, passive and active congestion, up to positive and unmistakable inflammatory action.* This state of the brain may be confined to one or two of the six layers composing the hemispherical ganglia; but all the layers are generally more or less implicated, in conjunction with the tubular fibres passing from the hemispheres through the vesicular neurine. This specific inflammation, from its incipient to the more advanced stage, is often associated with great vital and nervous depression.” (p. 61.)

Conjoined with this condition of the hemispherical ganglia, there may be general congestion or inflammatory action of the brain or its membranes; all those manifest structural changes in these tissues found after death, are, without exception, in Dr. Winslow's opinion, “the results or sequelæ of that specific inflammatory condition of the hemispherical ganglia” which he indicates. That this is true of a very large proportion of cases will be generally allowed. There are some forms of insanity, however, which can hardly be classed with them—namely, those in which there are simply fixed erroneous ideas. We have in the “electro-biologized” a transient, in fanatics and enthusiasts a permanent, form of this kind of cerebral change. In these, it would appear, as if the usual healthy action of the cerebrum *quoad* the erroneous ideas, had been just so much changed as to fix them in such a way, that whenever the succession of ideas passing through the mind brought these within the chain, they were presented to the consciousness, and not the ideas which would normally have arisen, had the cerebral tissue been free to act. It is apparent that a very minute pathological change in the cerebrum need only be assumed in these cases of fixed ideas, and this being granted, it would be difficult to assign it to inflammatory action, or even congestion.

The principles of *medical treatment* laid down by Dr. Winslow, flow from the pathological premises. There is no uniform method of treatment. In proportion as the symptoms approach to those of phrenitis or meningitis, in other words, in proportion as they indicate active inflammatory action, general and local blood-letting is advisable. Local bleeding is not to be confined to the seat of the disease, for it not unfrequently happens that it may be adopted with reference to a distant viscus. Leeches to the vulva and thighs are beneficial in cases concurrent with the menstrual period; to the sphincter ani, in those obviously connected with suppressed hæmorrhoids. In some instances Dr. Winslow has applied leeches to the Schneiderian membrane, particularly in those cases occurring in early life, and in persons of plethoric constitution and of sanguine temperament. He has known illusions of hearing or of vision which had embittered the

patient's life, removed by leeching behind the ears, or over the superciliary ridges. Dr. Winslow also speaks in warm terms as to the great utility, in acute mania, of prolonged hot baths, according to the method of Dr. Brierre de Boismont, of Paris. The patients remain from eight to fifteen hours in warm baths, at 82° to 86° Fahr., whilst a current of water at 60° is continually poured over the head. The method is inadmissible in intermittent mania, and in insanity beginning with great mental derangement, or associated with epilepsy or general paralysis.*

The class of *sedatives*, or in other words agents, modifying directly the condition of the cerebral tissue, constitute very valuable remedies. In recent acute cases they are generally admissible; it is in the various chronic forms of melancholia they are most useful. Dr. Winslow observes,

"In suicidal insanity, when local cerebral congestion is absent, and the general health and secretions are in good condition, the meconite and hydrochlorate of morphia often act like a charm, if *uninterruptedly and perseveringly given* until the nervous system is completely under their influence. . . . I could cite the particulars of numerous cases of this form of insanity radically cured by the occasional local abstraction of blood from the head, the administration of alteratives, the warm bath, and sedatives. In the exhibition of this powerful curative agent, our success will often depend upon a *ready adaptation of the form of sedative to the description of case in which it may be deemed admissible, and a judicious admixture of various kinds of sedatives.* I do not think we pay sufficient attention to this fact. I have often seen an apparently incurable and unmanageable case yield to a combination of sedatives, which had resisted the operation of any one or two when given separately." (p. 75.)

Dr. Winslow specifies the sedatives, and the combinations with other remedial agents, which he has found useful in certain forms of insanity.

We pass over numerous practical remarks, to note specially *endermic medication* in insanity—a plan offering numerous advantages, but little practised. Dr. Winslow states on this point:

"In some chronic forms of insanity, in dementia, and persistent monomania, connected, as it was supposed, with morbid thickening of the dura mater, and with interstitial infiltration of the membrane, as well as with exudations upon its surface, I have occasionally had the head shaved, and have perseveringly rubbed over the scalp a strong ointment of the iodide of potassium combined with strychnine. In other instances I have kept the head painted with the mixture of iodine. I have seen marked benefit from this mode of treatment. When the mental symptoms are supposed to be associated with effusions of serum, I have ordered the iodine to be applied externally, at the same time exhibiting minute doses of calomel, or mercury-with-chalk, to slightly affect the system: this, conjoined with occasional tonics, diuretics, and stimuli to support the vital powers, is occasionally productive of considerable benefit, in cases apparently placed quite beyond the reach of improvement or cure." (p. 78.)

We have seen a *solution* of iodide of potassium constantly applied to the shaven scalp followed by improvement in the mental state. A cap lined with gutta percha muslin suffices to keep the scalp constantly moist. Cases of *dementia*, the consequence of *strumous* irritation or inflammatory action, are those in which endermic medication will yield the most satisfactory results.

Dr. Winslow concludes this lecture with some important remarks on

* A full account of this method may be found in the third number of the *Journal of Psychological Medicine.*

the serious consequences which are resulting from the idea, that insanity is not amenable to medical treatment.

“We see it,” he observes, “influencing the conduct of county magistrates in the architectural proportions, medical organization, and general arrangements of our great national asylums. We also perceive the consequences of the error operating in many of the private institutions for the treatment of the insane, thereby degrading them into places of detention, instead of conferring upon them the character of HOSPITALS FOR THE CURE OF THE INSANE.” (pp. 80, 81.)

These remarks are apposite. It is very true, and a subject of very great congratulation, that a manifest improvement in the treatment of the insane has been effected of late years. It cannot be denied, however, that there is a well-grounded fear that in the large asylums the curative treatment may degenerate into a simple hygienic plan. They are so under-officered, that the medical attendant cannot, from sheer impossibility (having only twenty-four hours in the day), give that minute attention to each individual case, which is often most essential to successful treatment. Then, again, it is necessary to the satisfactory management of these large establishments, that a *system* of government be strictly carried out; the inevitable tendency of this is to degenerate into routine, and routine practice (as is well known) is an insuperable bar to progress. If we add to these considerations another of not less importance—namely, the temptation to meet the wishes of economical magistrates and rate-payers, by making the *labour* of the inmates productive, we have sufficient grounds for expressing the opinion, that it is a great and good service to recal the practitioner's attention to the fundamental fact, that insanity is a disease of a viscus, and, like other diseases of the viscera, is amenable to medicinal agents.

The third lecture, “on Medico-Legal Evidence in cases of Insanity,” comprises a practical statement of the cases in which medical evidence is required in courts of law; an examination of the legal *dicta* of the judges delegated by Parliament in 1843 to decide upon some fixed propositions as to the conditions under which a person is legally irresponsible, both theoretically and in relation to cases on record; and an analysis of the various terms used in courts. Dr. Winslow also gives practical instructions to the medical witness as to his conduct when giving evidence, and as to the manœuvres of counsel, illustrations being drawn from his own experience. All these are of importance to be known; this lecture should therefore be carefully perused by every practitioner who is about to occupy, before a commission of lunacy or elsewhere, the position of a witness.

The “safe rule” referred to in the subjoined paragraph is, we think, correctly characterized as the *only* safe rule; it is certainly the conclusion at which we arrived long ago, after a careful consideration of the question.

“Having, I think, conclusively established that we have no uniform legal or medical test of insanity to which we can safely appeal in criminal cases, you will ask, have I any psychological *criteria* to suggest for the safe guidance of the profession?—can I propound any principles which will assist the medico-legal witness in arriving at a satisfactory result? In reply to these interrogatories, I allow that we have no infallible standard, no certain principles which would admit of general and indiscriminate application. The only safe rule upon which we can act, is that of comparing the mind of the alleged lunatic, at the period of his suspected

insanity, with its prior natural and healthy manifestations; *to consider the intellect in relation to itself*, and to no artificial *à priori* test." (pp. 153, 154.)

Dr. Winslow quotes Dr. Combe's opinion to the same effect. We would suggest a further step in the inquiry—namely, to investigate how far the abnormal mental phenomena depend upon *cerebral disease*; for it is this which constitutes insanity; the morbid mental phenomena being only *symptoms*. This process will separate those cases in which the irregular mental action is *congenital*, but *normal* as to the individual, from those in which it is *induced* and *abnormal* as to the individual. In the former case, we have to investigate idiocy, imbecility, or eccentricity; and these may be considered as removed from the domain of technical inquiry, to that of mere common sense, in which witnesses, counsel, judge, and jury are all equal.

Dr. Winslow finally calls attention to the important relations between the criminal and the insane mind, and, in illustration, quotes a remarkable table from No. 163 of the 'Quarterly Review.' It was not drawn up for the use to which Dr. Winslow applies it—namely, to establish "the painful fact, that there is in existence a large amount of crime connected by hereditary predisposition and descent with diseased mind." These unfortunately constituted persons occupy, as to their mental state, a sort of neutral position between soundness and unsoundness of mind; not legally or medically *insane*, but yet weak in self-control from an over-mastering organization. To these, as well as to the legally insane, Dr. Winslow argues, some mercy should be shown, and in this opinion, all who are practically acquainted with the subject, will fully and approvingly concur. So important a step in legislation, however, can only be taken when public opinion is more enlightened, and when the profession itself is so much better informed in cerebral physiology as to gain the confidence of, and so guide, the bench and the bar.

T. Laycock.

REVIEW III.

Traité des Maladies du Sein et de la Région Mammaire. Par A. VELPEAU, Membre de l'Institut, &c.—Paris, 1854. pp. 717.

A Treatise of the Diseases of the Breast and of the Mammary Region. By A. VELPEAU, &c.

WE are informed by the renowned Professor of La Charité, that this voluminous monograph is composed of materials collected during a public and private practice of thirty years. Nearly two thousand cases constitute its basis. From an experience of this magnitude we might be led to expect a considerable amount of novelty; and how these expectations are realized it will be the object of the following analysis of the work before us to exhibit.

The author disposes of the anatomical and physiological conditions of the mammary gland in less than twenty-one lines.

He arranges the affections of the mammary gland of the female in two principal categories.

- "1. The diseases of benign nature, inflammatory and not inflammatory.
- "2. The diseases of a malignant nature or cancerous."

Those of the first class are thus defined :

“ By benign diseases I mean those which, left to themselves, do not fatally menace life, or of which cure is the natural termination. I divide this class into two groups, one of the different kinds of inflammation, the other of those diseases which, from the first, or in their course, are foreign to inflammatory action.” (p. 2.)

The first chapter is devoted to the subject of the inflammatory diseases of the breast, and comprehends excoriations, fissures, eczema, various forms of erysipelas, congestion of the gland tissue with milk, and all the varieties of phlegmon. Of these various diseases, some attack the nipple or the areola, whilst others are confined to the lactiferous ducts. Many originate either in the gland alone, or in the cellulo-adipose tissue.

The first section of this chapter is devoted to the eczematous affections of the nipple and areola, to excoriations and fissures or chaps of the nipple, and to inflammation and abscess of these important parts of the organ.

The second section treats of inflammation of the breast, properly so called, of which the subjoined table represents certain divisions.

“ Phlegmon	1. Inflammation, superficial or sub-cutaneous,	{ circumscribed. diffused. primitive. secondary. complex.
	2. Inflammation, deep or sub-mammary, <i>Congestion from Milk.</i>	{ idiopathic. symptomatic. general or diffused. partial or circumscribed. primitive or secondary.
	3. Inflammation, glandular or parenchymatous,	{ of the milk ducts. of the gland tissue. complex.” (p. 26.)

Lactic congestion of the breast is illustrated by a table of 25 cases, and M. Velpeau thus sums up the section :

“ Cases of lactic congestion are so numerous, the progress and the symptoms of the disease exhibit so much simplicity, that it would be quite superfluous to accumulate in this chapter a larger number of examples.

- “ 1. In 25 cases, the disease affected—
Both breasts in 7 individuals.
The right in 9 ”
The left in 9 ”

“ 2. Of the patients, 24 had been only just confined, or were suckling, and only one was pregnant.

- “ 3. The congestion was attributed to—
Weaning in . . . 7 individuals.
Cold in . . . 7 ”
Contusion in . . 5 ”
Parturition alone in 6 ”

"4. The respective ages of the individuals were as follows :

Aged 19	years,	2
" 20	"	3
" 20 to 30	"	12
" 30 to 40	"	5
" 40 to 50	"	2
" 55	"	1

"5. A complete cure was the result in—

From 4 to 15 days in 10 individuals.

" 15 to 30	"	3	"
" 30 to 40	"	3	"
" 40 to 60	"	2	"
" 60 to 80	"	2	"

and five women left the hospital before the cure was effected.

"6. Finally, in 18 cases the congestion was purely of milk, whilst in 7 others it was either inflammatory, chronic, or foreign to lactation." (pp. 74, 75.)

It appears remarkable that M. Velpeau does not allude in the third group to the defective development of the nipple as a prolific cause of lactic congestion. In our experience, this condition of that important portion of the organ is by far the most frequent source of trouble.

In the following *résumé*, M. Velpeau gives the result of his experience :

"From the preceding observations, it follows that inflammation may begin in one or other of the elements constituting the mammary region. 1st. In the skin, under the form of erysipelas or eczema. 2nd. In the subcutaneous tissues, under the form of phlegmon or absorbent inflammation. 3rd. In the submammary tissues. 4th. In the gland tissue itself, under various forms. 5th. In a form more general still, in the gland tissue, or in the connective tissue.

"To justify these distinctions it is only necessary to attend to the special characters which the inflammation presents, either in its causes, its symptoms, its progress, its prognosis, or in the treatment which is applicable to it. Thus, subcutaneous idiopathic inflammation arises in the breast under the influence of the same causes as in all other regions. Deep inflammation may result from external violence, or from certain affections of the chest and axilla, but it does not the less frequently arise from diseases of the breast itself. As for phlegmasia of the gland, properly so called, there is a large amount of evidence to prove that it almost exclusively depends upon lactation, parturition, or pregnancy.

"As regards *symptoms*, who has not observed that an inflammation characterized by redness, a circumscribed or diffused swelling, elevating the surface of the skin, and soon accompanied by a sort of œdema, differs essentially from that which, situated under the breast, shows itself from the first by raising and pushing forwards the entire gland: and continues, even to the end, without causing any marked redness or very distinct lumps on the surface of the region? And then, how can these inflammations be confused with those, such as of the gland tissue, which manifests itself, from the first, under the form of lumps, more or less deep and numerous: which, preceded or complicated by the suppression or retention of milk, frequently involves several regions of the breast at one time?

"Subcutaneous inflammation, like common phlegmon, is scarcely more than eight days in terminating in an abscess, which is generally single, and the fluctuation of which, as soon as fluctuation is established, rarely escapes the attention of the surgeon. Deep phlegmon, on the contrary, even when developed more quickly, exhibits this which is remarkable, that when the suppuration is established, it is recognisable at a much later period. From being deeply seated, abscesses, when they form, have not the advantage of opening so quickly as those the result of subcutaneous phlegmon; but the pus traverses the gland from behind forwards,

and then gives rise, secondarily, to subcutaneous phlegmon. Parenchymatous phlegmon differs from the two preceding, and almost always depends upon several phlegmonous attacks, which continue to arise with some women for one, two, or three months consecutively. The same well-marked distinctions characterize the termination and prognosis of these cases. Setting in with violence, from the first, superficial and deep phlegmon sometimes terminates in resolution. In the gland itself, on the contrary, inflammation almost inevitably proceeds to the formation of one or two abscesses.

“As regards therapeutical means, there is always a better chance of a successful issue if special means be employed to combat each form of inflammation. A great number of leeches on the diseased region, mercurial ointments, compression, and the usual topical applications succeed very well in subcutaneous phlegmon. They are quite useless, on the contrary, in deep inflammation, as well as in parenchymatous. Deep phlegmon is aggravated by compression, and requires general bleeding or leeches around the breast, and large cataplasms, as well as mercurial ointments. It is to inflammation of the gland tissue that purgatives, *les tisanes altérantes*, and topical applications, purely emollient, are most advantageous. Ammoniacal, camphorated, and anæsthetic liniments are applicable only to engorgements from milk.

“Lastly, when we see that, in spite of every kind of treatment, mammary adenitis is often prolonged by repeated attacks, a great number of weeks, whilst by appropriate methods subcutaneous phlegmon, as well as submammary, rarely lasts more than eight or fifteen days, the importance of the distinctions before established ceases to be a subject of debate.” (pp. 84—7.)

The third section is devoted to the consideration of abscess of the breast, which the author thus divides:

- “1. Subcutaneous or superficial abscess; (a) of the areola; (b) of the celluloadipose tissue; primary, secondary.
- “2. Deep abscess; (a) idiopathic; (b) symptomatic.
- “3. Glandular or parenchymatous.
- “4. Chronic abscess.” (pp. 87 and 152.)

A table is appended of more than 200 cases, of which the following analysis is given:

Patients cured	139
Deaths	3
Incomplete cures	28
Left hospital not cured	5

“Twenty-one cases exhibited complications, arising from—

Erysipelas in	5
Abscess in axilla in	6
Pleurisy in	2
Abscess in neck and back in	2
Variola in	2
Phthisis, eczema, erythema, and gastric disturbance in	4

“As regards the gland affected :

The right breast in	75
The left breast in	75
Both breasts in	23

“The age of the patients :—

From 15 to 20 years	30 cases.
From 20 to 30 years	116 „
From 30 to 40 years	23 „
From 40 to 50 years	5 „
From 50 to 60 years	3 „

“As regards the site of the abscess it was :

Subcutaneous in	37 cases.
Submammary in	38 „
Parenchymatous in	95 „

“The causes to which the disease was attributed :

Contusion in	20 cases.
Eczema in	3 „
Pregnancy in	7 „
After parturition in	110 „
Fissures, only	2 „
A needle	1 „
Cold	6 „

“In relation to lactation :

In women who have suckled	75
„ „ „ not suckled	4.” (p. 197.)

In the second chapter, the diseases of a benign nature, and which are not dependent upon inflammation, are described ; the author first discusses “ Ecchymosis without contusion, or spontaneous,” and “ Contusion properly so called.” Then, in the second section of this chapter, he refers to “ indolent tumours of a benign nature.”

A table of these tumours, proposed by the author in 1838, is inserted ; it comprehends tumefactions ; chronic intumescence ; all tumours which, left to themselves, are not exposed to cancerous degeneration, or which are very rarely subject to this unfortunate transformation ; hypertrophy ; engorgements ; a certain number of cysts ; and many varieties of tumours. It is as follows :

- “ 1. Hypertrophy { a. Of the gland.
 b. Of the connective tissue.
 c. Of the adipose tissue.
- “ 2. Engorgements { a. Of the connective tissue.
 b. Of the gland tissue.
- “ 3. Cysts { a. Sebaceous.
 b. Gelatinous.
 c. Hæmatic.
 d. Lactic.
- “ 4. Tumours { a. Lipomatous.
 b. Fibrinous.
 c. Butyrous.
 d. Tuberculous.
 e. Osseous.
 f. Granular or nodulated.
 g. Adenoid, called partial hypertrophy.
- “ 5. Imaginary or supposed tumours.” (p. 209.)

In a section headed “ Indolent Engorgements,” M. Velpeau very properly objects to the term in these words :

“It is too vague a denomination, and of too little value, to be retained in precise scientific language. Before pathological anatomy permitted the appreciation and analysis of the changes and alterations induced by disease in the tissues, it was necessary to make use of a word which at least indicated the most evident charac-

teristic; that is to say, increase in the volume of the part diseased. In the present day, however, the various lesions of which the breast is the seat are sufficiently well appreciated to examine them singly, to isolate them, and to give the history of each individual kind. It is, then, only in a collective sense that we can apply the term 'engorgement.' Otherwise understood, in regard to the mamma, it denotes just as much poverty in scientific nomenclature, as that of 'white swelling' applied to the pathology of the joints." (p. 210.)

Under the term "Physiological Engorgement," that affection of the breast is described which is so commonly met with in young single women prior to the catamenial period, in the early months of utero-gestation, or as an accompaniment of some irregularity or depravity of the menstrual function.

The author uses the term physiological, because this affection is in such close relationship with the procreative system, and because it does not strictly deserve the name of disease.

"Simple Engorgement," partial or diffuse, results from contusion and different sorts of irritation,—as the influence of pregnancy, lactation, or depravity of the catamenia.

"Hypostatic Engorgement" expresses a condition of the gland which appears to originate in its own weight, or from its dependent position. There are two forms:—1st. That which occurs in women who have had children, and whose breasts are soft, heavy, and pendulous, and the pathological condition of which when swelled can be only attributed to stasis of the fluids and the pendent state of the organ. 2nd. In women whose breasts are large, without being soft or pendulous, but which are very fat, and in whom engorgement of the axillary region of the organ is often observed. The last form the author attributes to the pressure of badly fitting stays, which press the parts against the axillary region.

The consideration of "Symptomatic or Consecutive Engorgement," and of "Engorgements with a Tumour," concludes this section, in which the author does not appear to us to have kept in view the observations with which it commenced. A perusal of the cases appended to this section will, we think, bear us out in this statement. (p. 230.)

"Tumours, properly so called," form the subject of the next section of this second chapter. M. Velpeau writes:

"The benign tumours of the mamma may be referred to two principal divisions: those which are composed of solid substance or tissues, and others of fluid or pul-taceous matter. There are, in other words, solid tumours or cysts. This last term must not be understood to the letter, however, because certain solid tumours are quite as well surrounded by a sack as the collections of fluid which the cysts specially characterize.

"Solid tumours are subdivided into several kinds: some composed of the natural elements of the economy, more or less modified, exhibit a kind of texture resembling certain tissues; in others, on the contrary, new products are formed, a proof, at least, that the disease has originated in an exudation of matter effused from the vascular system, under the influence of abnormal actions.

"Hypertrophy, lipoma, and neuromata, belong to the natural elements of the breast, and merit, therefore, a serious study." (pp. 230, 231.)

"Hypertrophic Tumours." Under the term hypertrophy, M. Velpeau only describes those intumescences devoid of notable change in the con-texture of the breast:

"In hypertrophy the organ offers a manifest excess of volume; its interlobular spaces (*mailles*), its lamellæ, its cellules, its lobules, in fact all its elements, have undergone an abnormal development, which may be extreme, but without being complicated with disorganization; in such a manner that, when placed by the side of a section of the healthy tissues of the breast, the tumour cannot, at first sight, be distinguished from it. It is, then, in their proportions, not in their constitution, that the elements of a hypertrophied breast have undergone change." (p. 231.)

M. Velpeau insists the more on this characteristic, because he has to return to the subject when describing the adenoid tumours which many modern pathologists describe as hypertrophy of some of the lobes of the mamma, which appears to him very *mal à propos*.

"Hypertrophy exhibits two varieties: diffuse or general, and circumscribed or partial." (p. 231.)

M. Velpeau next describes ordinary cases of hypertrophy of the breasts, without adding to our knowledge of either the pathology or treatment of this fortunately rare disease. He then concludes the subject by the relation of a case which, so far as reported, demonstrates sufficiently clearly that it was not an example of hypertrophy of the mamma at all, but an entirely new growth which he removed. M. Velpeau gives an illustration of a section of this morbid structure, Plate I., which certainly in nowise resembles a section of a healthy mammary gland.

In the case referred to the disease was developed at the age of forty-two, in a prolific, healthy woman. "The *tumour*, formed by the entire breast, and not at the expense of a part of it only, exhibited irregularities of very different consistence." The dissection of the mass removed is thus described:

"The skin was thin, without adhesion, and the subcutaneous fat had completely disappeared from between the tumour and the skin. When the lobes were cut they projected upon the surface of the section. The mass of the tumour was not formed of a homogeneous tissue, but it was divided into several lobes, perfectly separable, and between which a very loose connective tissue was seen. Each of these lobes was composed of two elements: one projecting on the surface, elastic, grey, granular, and forming little nodules of various sizes; the other having a pearly lustre (*des nuances franchement irisées*), but in reflected light only, and of which the fundamental tint was white. The firm tissue, being very elastic, seemed to retract between the greyish-red nodules. The proportion of these two tissues was not the same in all parts of the growth. At the superficial part of the breast the very small lobules seemed to be merely surrounded by an embroidery of white tissue, whilst in the deep part, large and more scattered, they were surrounded by real hands, as it were. At the deeply-seated part of the tumour there was a bursa mucosa distended with serosity.

"The microscope, as well as an examination with the unassisted eye, demonstrated that all the lobules belonged to hypertrophy, and that they all contained the glandular cul-de-sacs." (p. 239.)

We have given the relation of the dissection of this morbid growth *in extenso*, because it demonstrates that, even in the present state of knowledge, how much confusion and difficulty exists in the appreciation of the new growths met with in the breast; and also to show how very deficient in details are several of the cases recorded by M. Velpeau. In this instance, for example, no mention whatever is made of the mammary gland itself—neither whether it was entirely removed with the

growth, nor, if removed, what were its relations to the growth. If the gland had been entirely atrophied, the ducts should have been traced, and then, in all probability, as we have frequently seen, the growth would have been found to be developed behind the atrophied organ. The confusion in the appreciation of the new growth is demonstrated by placing this morbid product in the category of cases of hypertrophy; for M. Velpeau, by so doing, here contradicts himself, after having just before stated that, in hypertrophy, the growth is merely of breast gland-tissue, and differs not, in any way, from the appearance of a section of the normal gland: whilst, in the tumour he describes, the appearance of the section was as far removed from that of a breast as well could be. In fact, this new growth, and "tumour," as M. Velpeau very properly styles it, was really a tumour, and not hypertrophy. It was, doubtless, one of the varieties of mammary glandular tumour, or adenocèle.

Observations similar to the above apply to the subject of "partial hypertrophy," thus described by the author, and in which category he includes "fibro-cystic tumours" and "cysts."

With reference to "Lipoma," M. Velpeau observes:

"It would be wrong to suppose that adipose hypertrophy, before spoken of, can be confused with lipoma—that lipoma is only a variety, a shade of adipose hypertrophy of the breast. The lipoma which I have observed in the mamma exhibited exactly the same characters as lipoma in every other region of the body: they were composed of lobulated masses of fat, of isolated adipose-cells, and were quite distinct from the surrounding tissues. In the persons thus affected, the rest of the natural adipose layer did not exhibit the least appearance of hypertrophy." (p. 246.)

Two cases are related in which tumours of this class were removed during life.

"Tumours dependent upon an Alteration of the Mammary Tissue" are next considered; and the author divides them into "Simple Mammary Induration," and "Neuromatic Tumours and Nodosities." The first heading is subdivided into "Induration with Sub-acute Swelling," and "Chronic Induration." (p. 253.)

The species of mammary induration of which M. Velpeau here writes, differs from the engorgements which he before described

"In the fact, that all the tissues which surround the diseased part of the gland retain their suppleness and the other characters which belong to their normal state. (p. 253.)

"Chronic induration," described by M. Velpeau, in 1838, under the title, '*Induration en Masse*,' "is a disease which was until then confused with tumours of every other kind. This disease, in the diagnosis of which it is, perhaps, difficult to be skilled, even at the bedside, is characterized, like the preceding—which is sometimes its point of departure, or first period—by manifest induration of a part or the whole of the gland. In general, it can only be appreciated and, in reality, determined by a comparison of the healthy breast with that diseased, and by the inequalities in density of various lobes of the gland. Sometimes accompanied with lancinating, heavy, and dull pains, it comes on by slow and insensible degrees; the gland then appears lumpy, having sensibly increased in volume; nothing indicates that it is the seat of the least engorgement or inflammation. It is only by pressing in succession the different lobes of the breast directly against the chest, or by grasping the lobes from side to side, and drawing them from the thorax, that it can be determined that some of them are manifestly more hard and unequal than the others. . . . This affection, which it is quite possible to confound, at first, with scirrhus degeneration, deserves all the attention of surgeons. Left

to itself, it often disperses without leaving any trace of its existence. It has never been demonstrated to terminate in the establishment of a scirrhus or encephaloid tumour." (p. 255-56.)

After describing the usual methods of treatment in these cases, M. Velpeau asks—

"Is it sometimes necessary, or even useful, to extirpate these tumours? It has been shown, when describing their prognosis, that, to all appearance, the benign induration of the mamma has no tendency to degenerate into a malignant tumour. In my opinion, then, an operation with the knife is not indispensable in such cases. It becomes, therefore, a matter of the greatest practical importance not to confuse this kind of induration with scirrhus: the one is nearly always cured without operation, and only very rarely gives rise to serious consequences; whilst the other can only be treated effectively with either cutting instruments or escharotics. In the one case, we may always completely reassure the friends of the patient; in the other, the most serious prognosis can only be pronounced." (p. 257.)

At a subsequent page, Velpeau, under the title of "Neuralgic Pains," refers to the cases, so common in the early and middle periods of life, in which there are severe pains, and sometimes feelings of heat, and yet in which no appreciable change in the tissues of the affected organ is manifest to touch or sight. The affection seems to depend upon a state of hyperæsthesia, and is connected with more or less general derangement of the functions of the procreative organs. The author adds nothing to our present knowledge as regards either the pathology or treatment of this complaint.

Under the heading of "Pain and Imaginary Tumours," the author describes those cases which arise after abscess, contusions, compression, and in irritable, excitable, nervous females, in whom the catamenial function is imperfectly performed, or is associated with leucorrhœa, and a generally disordered state of the nervous system. Several interesting cases are related, but the morbid anatomy of this state of the gland-tissue is not described. It is well known, and has been demonstrated by the use of the microscope, that these lobes of gland-tissue, thus affected, exhibit a very peculiar condition.

The cæcal terminations of the ducts which, in the inactive state of the organ, are usually void of epithelium, or only contain a delicate layer of it, are completely filled with this characteristic structure; and they appear like those of a gland in preparation for the secretion of milk. This fact is not mentioned by M. Velpeau.

"Tumours formed of Excreted (*Exhalées*) or Effused Materials."—Under this title we have described lymphatic or tuberculous tumour; disseminated, multiple, and purulent. Some of these cases appear to be what we should term chronic inflammation, effusion, or abscess; for there is not sufficient evidence in their relation to entitle them to rank as a distinct class of disease.

"I will add," writes M. Velpeau, "that the tumours which seem most peculiarly to merit the title of tuberculous, often finish by dispersing, by becoming inflamed, or by transformation into an abscess, which commonly brings along with it the cure. . . . And lastly, we must admit, that certain tumours of the breast, termed scrofulous, or tuberculous, result rather in a form of chronic inflammation, in lymphatic females, than in any peculiar morbid act." (p. 293.)

"Osteoid; Osseous or Calcareous Tumours," are then described. Old cases only are quoted.

"Tumours formed of Milk, or Galactocoele," are divided into those by infiltration, those circumscribed by a cyst, and those which are solid or concrete.

"In a sac, formed at the expense of the neighbouring tissues, or in a cavity resulting from the dilatation of its proper canals, the effused and accumulated milk would still undergo certain changes, perhaps like blood, when placed under similar circumstances.

"Thus: 1st. It would continue in the state of a slightly painful collection, without notable change, as was the case in a patient under Scarpa, for example, or that of M. Forget.

"2nd. It might decompose, and be replaced by a liquid purely serous, or by a mixture of serum and caseum, as in the case related by Sir A. Cooper; or else, if the serous part is taken up, it may become thick and creamy, like one of the cases of Dupuytren.

"3rd. It may inflame and transform the tumour into a true milk abscess, which, after being a long time indolent, assumes from this moment the progress and characters of an acute attack.

"4th. It may form concretions susceptible of assuming any kind of form and appearance, even to give rise to the idea of milkstones.

"5th. Becoming harder and harder, like the fibrine of blood in hæmatocele, and giving rise to butyrous and caseous tumours, which were seen in some of M. Velpeau's own cases." (pp. 307, 308.)

The differential diagnosis of these tumours formed by milk is next considered, and the author admits that the difficulty in distinguishing between them is great, chiefly owing to the very slight shades of difference which they exhibit.

The subject of the next section is "Cysts in the Mammary Region," and the following varieties are described:—(1) sebaceous cysts; (2) hydatids; (3) serous cysts; (4) sero-sanguineous cysts; (5) sero-mucous cysts.

Of the first variety, a very good illustration is given; but the case exhibits no anatomical difference from the ordinary sebaceous tumour of other parts of the body.

M. Velpeau writes of the second variety:

"Let us first state, that it is not demonstrated that all the tumours described under the title of hydatid were really so constituted. On reading the cases described by authors, serious doubt of the fact arises." (p. 316.)

The present state of knowledge upon this subject fully confirms the suspicions of M. Velpeau, for it must be admitted that many cases described under the title of "hydatid," really belong to the class of sero-cystic disease of the breast.

The true, genuine hydatid cysts, characterized by the presence of *echinococcus hominis*, are developed certainly in the mamma. This fact is demonstrated by recent observation.

The remarks upon the third variety present no novelty requiring a detailed notice; and the fourth and fifth varieties are named in accordance with their contents. The author has not enunciated any new pathological views, nor does he introduce any novel plan of treatment in these cases. In point of fact, his anatomical details are rather behind our present amount of knowledge, and this deficiency, which occurs throughout the volume, must be attributed to the want of careful and minute investigation by means of the scalpel, and by the aid of the microscope.

"Adenoid Tumours" are then described. These, says the author, have

"Generally, in surgical treatises and practice, been confounded with scirrhus

and encephaloid tumours, with occult cancer, and with benign scirrhus, and their history has not yet been sufficiently studied." (p. 350.)

Under the name *Tumeurs Fibrineuses*, M. Velpeau described these tumours in the '*Dict. de Méd.*' tome xix., and in his lectures, more generally, under the names of fibrous, scirrhus, or adenoid.

M. Velpeau seems to take great credit to himself for observing these tumours, and, at the same time, throws in the background the observations of others. The following description was given by M. Velpeau of one of these tumours, in 1824.

"It appeared," he then said, "that it was a degenerescence or transformation of the cellular tissue, and not a new formation. In a word, that it was a fibro-cellular nucleus, indurated by the morbid action; in fact, if not deceived in the utility of distinctions attempted to be established elsewhere, when speaking of carcinomatous tumours, a patient, the subject of this disease, might reckon on a complete cure." (p. 350.)

Astley Cooper, as Velpeau remarks, calls them "chronic mammary tumours;" his observations were published in 1829, under the title, '*Illustrations of the Diseases of the Breast.*' Many years, however, before this period, he had described these tumours in his lectures on surgery, both anatomically and physiologically, and especially as regards their innocence and resemblance to the structure of the organ in which they were developed. M. Lebert states the fact clearly and honestly, when he writes in his most valuable work on '*Cancerous Diseases, and the Curable Affections confounded with Cancer*'—

"The author who has best described these tumours is Astley Cooper; but yet it is clear, after reading his work, that he has not taken for his description of these cases, all the types under which these tumours show themselves, and are developed." (p. 367.)

M. Velpeau states nothing new as regards either the natural history, diagnosis, prognosis, or treatment of these cases. His facts agree with the observations of others, and as regards the anatomy of these new formations, his details are, literally, behind our knowledge of them in this country. This deficiency is solely attributable to the want of personal examination with the aid of the microscope; but he does not fall into the error, as some of his countrymen have done, of regarding these tumours as hypertrophied mammary tissue. These tumours cannot be regarded as hypertrophy, in the strict acceptation of the term, because their structure is, at best, but an imperfect imitation of the mammary gland tissue.

"In my opinion," writes M. Velpeau, "the creation of tumours in general is naturally influenced, often modified, by the neighbouring organic media. I have said for a long time, and I repeat it, that accidental formations have a manifest tendency to assume some of the characters of the organ in which they are located." (p. 357.)

Now, this doctrine was first enunciated, we believe, by Mr. Lawrence in 1832, and may be read in the '*Medico-Chirurgical Transactions*,' volume xvii.

M. Velpeau adduces, at p. 391-92, three cases in proof of the statement that the tumours termed by him "adenoid" can be absorbed or resolved by topical applications.

The following are the opinions of M. Velpeau regarding the treatment of these tumours:

"Although I profess that these tumours remain benign even to the end, that they are not susceptible of undergoing a malignant or cancerous transformation, I nevertheless think that it is better to extirpate them than to abandon them to themselves, or treat them by medical means and simple topical applications.

"This is the rule I have adopted, and for a long time recommended, in similar cases.

"1. If the patient is resigned to her condition, little disposed to trouble herself about a tumour, already of long standing and stationary, I advise her not to think about it, to leave it alone, and only to examine the lump at long intervals of time.

"2. To those who are naturally excitable I attempt to explain that the tumour of itself can give rise to no danger, and that it is not susceptible of assuming a bad character; I advise them, moreover, as much to calm their imagination as in the hope to cure them, to employ one or other of the medicines above described.

"3. I insist on the employment of these medicines when consulted by timorous females, who dread excessively the thought of an operation, immediate or remote.

"4. When the patient is very much occupied by thinking of the tumour, and the dangers which she attributes to it alarm her more than the operation itself, I then advise her to submit to its removal.

"5. Lastly, I tell other patients that there is no danger attending the existence of the tumour, but that if they wish to be cured of it medical means offer little chance of success, besides requiring a considerable amount of time, whilst the operation certainly removes it without exposing them to serious danger; and I add that, once extirpated, this species of tumour is not reproduced." (pp. 401, 2.)

This chapter is profusely illustrated by cases, and at the end a table of 60 is given. Upon looking over them a certain amount of suspicion is felt at the advanced age of many of the patients, but it must be observed that the age of the individual at the time she fell under the notice of M. Velpeau is there recorded, and in another column the length of time the tumour had been growing. Thus we find that in 1853 a tumour of this nature was removed from a woman of 60 years of age, but it had existed 20 years, and was, therefore, primarily developed at the age of 40.

The age at which these tumours are developed is very important as regards their diagnostication, and, therefore, this fact should be always accurately ascertained. This table concludes the first section and more than the half of the whole work.

The second section of the work is devoted to the consideration of diseases of a malignant nature, or of cancers of the mammary region.

M. Velpeau writes: "Cancer of the breast differs neither in its nature nor in its form from cancer of other parts of the body;" and he proceeds in the first chapter to denote the various forms of cancer, to which he applies the following terms:

"Scirrhus:

I. Ligneous scirrhus.

- a. Scirrhus, properly so called, or globular.
- b. Rayed scirrhus, or branching.
- c. Scirrhus "*en cuirasse*," or tegumentary.
- d. Ligneous in mass.
- e. Atrophic scirrhus.
- f. Pustular, or disseminated.
- g. Scirrhus of the lactiferous ducts.

II. Lardaceous scirrhus.

Partial and diffuse.

Encephaloid.

Melanosis.

Chondroid, colloid, and fibro-plastic cancer.

I. Napiform tumours, or fibro-plastic, properly so called.

II. Colloid cancer.

Epithelial cancer, or epithelioma.

Keloid.

Anomalous cancer."

The second chapter of this second section is devoted to the consideration of the differential diagnosis of the varieties of cancer, by their microscopical anatomy, and between cancer and benign tumours.

The third chapter treats of the nature and etiology of cancer, and the origin of this disease as dependent upon external violence, is considered. M. Velpeau thus writes upon this important point:

"Without absolutely denying the influence of external causes in the production of cancers, some authors restrict themselves by saying that a special predisposition is at least required; that without such predisposition, the external cause would not have been followed by such a result.

"It seems, in fact, since we are unable to generate it when we wish, that individuals affected with cancer have in their organism a certain predisposition to contract it; but this predisposition being admitted as a fact, science is scarcely more advanced. All diseases might invoke the same peculiarity. Without the predisposition, phthisis would be only rarely established. The affection called scrofulous, does it not also require an organic predisposition? Are there not certain individuals predisposed to lipoma, tumours, and steatoma? Does not pneumonia itself require, in the greater number of persons it attacks, a special predisposition? Arising from the slightest causes, whilst a similar cause, much more severe, produces nothing of a like nature in an infinity of other persons, it is natural to admit for cancer a special predisposition; but that does not in any way prevent the necessity of an occasional cause, without which it would not manifest itself." p. (532.)

The predisposing causes of cancer are next related; these are age, sex, general health, constitution, regimen, habits of life, &c.; and the consideration of the occasional causes and contagion concludes this chapter.

The prognosis of cancer is described by our author as hopelessly unfavourable.

The remedies employed to combat cancer of the breast, are internal medicines and external applications. The first are bleeding from the arm, or by leeches from the part; purgatives, emetics, preparations of conium, arsenic, alkaline substances, preparations of gold, quinine, sarsaparilla, and iodine. The use of these remedies is not recommended by the author, they are rather mentioned as having been employed, and their inutility demonstrated; and M. Velpeau thus concludes:

"To sum up in a few words, the cancerous nature of the disease being once established, we do not yet possess any remedy, any general or internal medicine which has ever brought about a cure. Besides my own experience, I have, in support of the opinion to which I have just given utterance, the examination of many facts occurring in the practice of others. Every time that I have wished to verify the observation invoked in favour of such a mineral water, or such pretended curative treatment, I have arrived at the conviction that the nature of the tumour was mistaken, or that the pretended cure was not realized." (p. 561.)

The second, external applications, are the same as we employ in this country. In relation to the efficacy of the employment of compression in cases of carcinoma of the breast, M. Velpeau writes:

"Thus I say to practitioners without hesitation, that they can place no reliance on the efficacy of this resource in the treatment of cancers. If it sometimes succeeds, it is only, be assured of it, in cases of simple engorgement, or of tumours not cancerous." (p. 563.)

As exceptional cases, M. Velpeau relates at pages 565 and 6 three very interesting observations, "not with the sole view of proving that scirrhus (*les squirrhés*) may be cured; but with the hope of inducing practitioners not to deny absolutely the possibility of the fact at the commencement of the disease, and under the forms he has indicated."

M. Velpeau concludes a chapter on the appreciation of the curative means in these words:—"If the curative insufficiency of so many different medicines is but too plainly demonstrated, must not the same be said of operative medicine, of surgery properly so called?"

The surgical means to be employed, and especially the question whether cancer itself can be radically cured by operation, are the subjects next discussed, and from clinical facts and observations M. Velpeau concludes:

"1. That no plausible reason has yet been given in favour of cancer being a disease primitively general.

"2. That, on the contrary, one would be inclined to admit its title to being an affection primitively local.

"3. That certain tumours of a benign nature *appear* to undergo, in some cases, a malignant transformation.

"4. That the cause of benign or malignant tumours, adenoid and even cancerous, of the breast may *probably* be traced to a plastic, sanguineous, or secreted exudation into the tissues, either spontaneously, or from external violence.

"5. That the existence, or the non-existence, of the cancer cellule in tumours is not conclusive evidence that the disease will or will not return after operation.

"6. That it would be, consequently, imprudent to decide from the evidence afforded by the microscope alone, whether to operate or not.

"7. That observations and statistics are far from proving that the extirpation of tumours of the breast is always followed by a return of the disease, always useless, or even hurtful.

"8. Finally, that facts sufficiently numerous, and that observations selected from my own practice, demonstrate, without the possibility of contradiction, the existence of radical cures of cancer by the operation." (p. 598.)

After describing the dangers of the operation by means of the knife, and the treatment of the case preparatory to its performance, the value of caustics in general, their advantages and inconveniences, form a subject for consideration.

Upon the practical value of the use of caustics M. Velpeau thus remarks:

"The result of my experience proves that they should not be rejected absolutely as a curative means. That they are preferable to a cutting instrument,—1st. When the cancer is ulcerated in patches, and when more widely spread than deep; 2nd. When, even by the cutting instrument, there is no chance of preserving a part of the integuments attacked by the tumour; 3rd. When the cancer is fungating, exactly limited, and when the patient dreads much more the use of the knife; 4th. Ulcerated scirrhus, irregular or disseminated, can be better attacked by caustics than by operation; 5th. The same may be said of ulcerated cancer adherent to the summit of the axilla, under the clavicle, or in the neighbourhood of bone." (p. 663.)

The effects produced by various kinds of caustics are then described, and their individual advantages examined.

Congelation, by means of the application of pounded ice and salt, has, according to M. Velpeau, certain advantages, which may be regarded as palliative, if not, even in some cases, as a means of cure. And, although the author has little experience in its application, the effects which he has seen produced would induce him to employ the frigorific mixture before entirely rejecting it, especially as a succedaneum to caustics.

"When a true cancer" has been operated upon, there always remains the sad expectation of a recurrence, and therefore M. Velpeau next takes into consideration the means to be adopted in the hope of arresting or preventing this distressing result.

After speaking of the employment of counter irritation, regimen, and syphilization, he thus very justly denounces the last:

"Syphilization, with the intention of curing or preventing cancer, is then one of the thousand chimeras which arise, from time to time, as an exhalation from the brain of certain men.

"In conclusion, science does not yet possess any means which may serve as a preservative from cancer, or prevent the recurrence of this terrible disease." (p. 652.)

The propriety of removing secondary developments forms the subject of the next section, and M. Velpeau is in favour of a second or even third operation, when practicable. He supports this doctrine by the relation of a case entitled "Encephaloid tumour, extirpated three times, and at last radically cured."

A lady, sixty-six years of age, had had a tumour removed from the breast a year before she consulted M. Velpeau. He removed the second development, which was in the lower edge of the cicatrix. The axillary glands were not diseased, nor was there a tumour elsewhere. The growth M. Velpeau removed exhibited "all the characters of encephaloid tissue. Soft, fungous, medullary, red, vascular on its external surface, it was lardaceous, homogeneous, brownish in places, and continuous everywhere with the thick layer of mammary tissue which was removed with it."

A third growth formed in eighteen months, was removed, and exactly resembled the last described. The lady had enjoyed perfectly good health since, and there is no new growth in 1853, ten years having elapsed since the last operation.

If we accept this case as one of encephaloid cancer, it does not prove more than that a patient may be free from the disease for ten years, even after the removal of a third development. It does not prove that she will continue free from cancer the rest of her life, nor that the disease is radically cured, because there are cases on record in which a cancer having been excised, and the patient having continued free from disease in the part primarily involved, has fallen a sacrifice to the development of internal cancer after a longer period than ten years had elapsed.

In the absence, however, of positive evidence, by minute examination of their structure, that these growths were cancer, we cannot accept this case in evidence merely because M. Velpeau states that, in his opinion, the growth was an "encephaloid tumour,"—which it might indeed have been without being a cancer, for many of the forms of mammary glandular

tumour are brain-like, but their minute anatomy at once demonstrates that they are not cancer.

A table of 146 cases concludes the subject of cancer. In an analysis of this table, M. Velpeau begins by stating that in a given year we find a certain proportion of cases reported. But the author's enumeration appears strangely inaccurate. Between the years 1824 and 1852, inclusive, M. Velpeau enumerates 190 cases; whereas the table includes only 146. Of 197 cases, 57 were not operated upon, and the operation, or cauterization, was performed in 140 cases. Of these 140, exactly half, 70, continued well, or were in progress of cure when they were lost sight of. Thus only 70 cases remain which can be of any use to determine the advantage or disadvantage of any operation in the treatment of cancer. Of these 70 cases, in 22 the disease had returned, and 30 patients were dead, leaving 18 concerning whom there is no more history.

Another equally unsatisfactory table of cases of encephaloid disease is given. In this, of 45 cases operated upon 9 died: 3 of erysipelas, 3 of purulent infection, 2 of pleurisy, 1 of hospital gangrene; 20 are said to have left the hospital cured, leaving 16 of which no further result is stated.

These tables appear to us to be valueless for want of accurate and minute investigation of the structure of the growth removed, as well as from the insufficient details relating to the progress and termination of the cases.

The work concludes with some brief details in reference to the diseases which affect the mamma of the male, and the rudimentary organ of the infant.

John Birkett.

REVIEW IV.

Klinik der Geburtshilfe und Gynæko'logie. Von Med. und Chir. Dr. J. CHIARI, Med. und Chir. Dr. CARL BRAUN, und von Med. und Chir. Dr. J. SPAETH.—*Erlangen*, 1853.

Clinical Observations on Obstetric Medicine. By Drs. CHIARI, BRAUN, and SPAETH.

THE book whose title we have given above, the result of the labours of celebrated German physicians, is now on our table, and we propose giving our readers an insight into its contents. There is much in it which has been well known, and equally well described, by writers of our own country; but we are bound to acknowledge that, in the obstetric department of our profession, we are behindhand in works of easy reference and practical utility. Our systematic treatises on the science and art of midwifery are probably the best of their kind in any language, and leave scarcely anything to be desired in their particular sphere; but we possess no dictionary of midwifery, like our dictionaries of medicine and surgery; and the clinical reports published from time to time in our journals do not compensate the busy and anxious general practitioner for the want of such works on practical midwifery as are published by the Germans. We should hail with pleasure the appearance of a work on midwifery in our own language, on the principle of Busch and Moser's '*Handbuch der*

Geburtskunde,' containing a good practical article on each subject connected with obstetrics, written by our best authorities, and arranged in alphabetical order; it would prove a boon to students, and form an acceptable book of reference to such as are engaged in the toilsome and anxious labour of midwifery practice. In the volume before us, numerous subjects are treated of in a very creditable manner, and it is somewhat difficult to make a selection. Hæmorrhage occupies an important part; and as there is one form of it which has rarely been described, and of which four cases are related, we transcribe one of them for the information of our readers.

"N. N., æt. 26 years, was delivered of a female living child of the usual size, after a natural and regular first labour. Soon after the expulsion of the placenta, which took place naturally, the patient complained of pain in the genital organs, which, becoming more severe, led to an examination, and to the discovery of a large, firm swelling in the vagina, supposed, from the manner of its origin, to be a thrombus. In order to prevent its increase, cold applications were made use of; but notwithstanding this, it had increased so much the next day, as to fill the cavity of the pelvis, the right labium being swelled to the size of a fist, and the swelling extending beyond the perineum backwards into the right buttock. The uterus was pushed upwards, and the pressure upon the bladder was sufficient to require the use of the catheter. The following day the swelling became of a violet colour, and on the third day it burst, about an inch behind the entrance into the vagina, and a good deal of coagulated blood was removed by the fingers. Meanwhile, all the symptoms of a severe metro-phlebitis appeared, and the large irregular cavity of the thrombus soon secreted very offensive matter. The patient died on the seventh day. The treatment of the thrombus consisted in cold applications during its formation, and frequently-repeated warm injections into its cavity for the removal of a foul discharge, after it had opened. Dissection exhibited the usual results of *endometritis septica* and metro-phlebitis; and between the vagina and pelvis, the irregular cavity of the thrombus, extending along the psoas upwards towards the right kidney. The surrounding parts, and even a portion of the mesentery, were stained of a violet colour by the sanguineous effusion."

This disease, or rather this accidental complication of labour, has rarely been noticed by authors, and must be of very infrequent occurrence. It is said to originate in the rupture of large vessels, but whether arteries or veins is a matter of doubt, and perhaps of no great practical importance to be known. The period at which it has been observed to take place is either during or after labour; and most frequently in first labours, when the head has been forced down quickly, and the vessels have not had time to accommodate themselves to the increased quantity of blood they have to contain, and therefore give way. It is remarked, however, that individuals subject to varicose veins around the vagina are not thereby predisposed to this accident. The thrombus generally makes its appearance in some part nearest the genitals; indeed, most frequently in the cellular membrane between the vagina and pelvis, near the rectum, labia, and perineum, and is discovered about the period when the afterbirth is expelled; for it requires some little time to develop itself. The extravasation sometimes extends outwards over the back and thighs, and internally to the peritoneum, and along the psoas muscle towards the kidney, but seldom into the texture of the uterus. The diagnostic signs are, the sudden appearance of a swelling during labour, more or less firm to the touch, and increasing in size during the pains; the pain belonging to the disease itself, if any, being obscured by the pains of labour. The bluish

colour of the skin does not appear until the effusion has made its way nearly to the surface.

When the extravasation is of slight extent, it may be absorbed; or the disease may result in the gradual escape of the extravasated blood by a spontaneous or artificial opening, with subsequent suppuration, terminating either in a cure, or pyæmia, and death. The treatment consists in endeavouring, in the first place, to prevent the increase of the tumour, as soon as it has been discovered. Compression of the swelling with cloth dipped in cold water has been recommended, but unfortunately can seldom be carried into effect, from the peculiar situation of the thrombus; and if pressure be applied externally without closing the bleeding vessels, matters may be made worse, by forcing the blood internally under the peritoneum. When cold and pressure cannot be applied directly upon the bleeding point, our treatment has to be limited to cold lotions, injections, and the introduction of pieces of ice into the vagina, in order to promote the coagulation of the extravasated blood. If a thrombus occurs during labour, and causes obstruction to the passage of the child, we are advised to hasten delivery, and if necessary, to open the tumour. If the tumour does not soon give way spontaneously, we are recommended to persevere with the cold applications for several days, and not open the cavity before the vessels are closed by coagula; but afterwards it may be opened, some of the coagulated blood removed, and a healthy suppuration promoted by warm injections and fomentations. Should the cavity be slow in filling up, weak astringent injections may be employed.

The volume before us also contains an elaborate article on the nature and treatment of convulsions occurring in the generative period of women, either during pregnancy, in the various stages of labour, or after delivery, by Dr. Carl Braun. The results of his own experience, in the observation of 52 cases of convulsion occurring in 24,132 labours, have led him to differ somewhat from former prevailing opinions. He arranges the cases as follows, according to their causes:

“A. Convulsions occurring without the co-existence of morbus Brightii.

1. Hysterical convulsions.
2. Epileptic ditto.
3. Convulsions from cerebral diseases—as hyperæmia, apoplexy, meningitis, encephalitis.
4. — from the inhalation of irrespirable gases, or from the effects of poisonous substances.

“B. Convulsions which are connected with Bright’s disease—as uræmic intoxication, under the name of eclampsia.

“C. This division includes those cases where, notwithstanding the presence of Bright’s disease, the act of labour occurs normally, and, with the exception of certain disorders of the senses during pregnancy—as amaurosis, amblyopia, hemeralopia, deafness, &c.—no other functional disturbance is produced.

“D. Natural parturition during the existence of Bright’s disease, *without convulsions.*”

Several writers in this and other countries have remarked upon the presence of albuminuria in connexion with puerperal convulsions, but the subject is still in considerable obscurity, and is of sufficient importance to demand further investigation. Dr. Carl Braun has thrown new light

upon the influence of uræmic intoxication in the production of eclampsia; and although it must be admitted that some of his propositions require further testing before they can be fully accepted, we think it worth while to place before our readers such an analysis of his interesting paper as will induce them to devote their attention to the practical questions involved. To be enabled to distinguish clearly between the various kinds of puerperal convulsions, and at once decide upon the befitting treatment, is a most desirable qualification for an obstetric practitioner; for a mistake in the application of remedies in a disease of such severity would often, probably, be attended with fatal consequences; and when we consider that, with a train of external phenomena very similar in appearance, one case may demand venesection to as great an extent, perhaps, as almost any other disease whatever, whilst another requires an exactly opposite method of treatment; when, too, the nature of the symptoms is such, that delay in determining upon the proper management of the patient may lead to the most serious consequences, we cannot but hail with pleasure every philosophical or scientific attempt to elucidate the causes, diagnosis, and prognosis, of the different kinds of puerperal convulsions.

It has generally been supposed that the presence of albumen in the urine of pregnant women has been due to congestion of the kidneys, from the pressure of the enlarged uterus; or to a general congestion of the system during the pregnant state, often producing anasarcaous swellings, or effusions into serous cavities. Cases of this description are familiar to us all; but it will be found that, in some cases, disease of a more serious character is at the bottom of it all, and that in such our prognosis, as concerns the safety both of mother and child, must necessarily be of a much more unfavourable character. We now proceed to describe the symptoms of eclampsia, or uræmic convulsions, according to our author.

Symptoms.—Eclampsia, or puerperal or uræmic convulsions, occur suddenly at some period of the generative process, and consist of a more or less regular succession of phenomena, amongst which loss of consciousness is the most prominent. During the attack, the head and neck appear swollen, red, and livid; the eyes move rapidly in various directions, or remain fixed in their orbits with a vacant stare; the conjunctivæ are generally injected, the tongue protruded, and frequently wounded by the teeth, so that bloody froth escapes from the mouth; it is sometimes, also, considerably swelled. The muscles of the face are convulsed, and the extremities are affected with rapid movements of flexion and extension, alternating with each other. The whole trunk is also thrown backwards and forwards, or else immovably and stiffly bent backwards, or to one side. The carotids pulsate strongly; the jugular veins become distended; the respiratory muscles, especially the diaphragm, are in a state of spasm, threatening suffocation. Generally, there is vomiting, and the urine and feces are passed involuntarily. The skin is covered with perspiration, and its temperature altered. Reflex sensibility is arrested during the attacks. The pulse may be either frequent or slow.

After these symptoms, an interval of variable duration follows, in which the patient lies in a soporose condition, with extended, stiffened limbs, difficult, frequent, and stertorous breathing, and continued loss of sensation and consciousness. The duration of the attack may be a quarter

or half an hour, or a whole day, the coma remaining uninterruptedly, so as to simulate a severe attack of apoplexy.

Should the first attack not terminate fatally, a remission ensues; the breathing becomes less impeded, the muscular rigidity disappears, consciousness sometimes returns, but is often imperfect, and the frequency of the pulse diminishes. The abdomen seems tender to the touch, and reflex sensibility, during the intervals, is generally exalted. The patient mostly complains of confused, dull headache, and great exhaustion; and this continues until a degree of restlessness, stretching of the limbs, convulsive movements of the eyelids, and turgescence of the features, announce a fresh attack. In this manner, the paroxysms are repeated often in the day, sometimes from ten to fifty times, a state of unconsciousness remaining during the intervals. The outbreak of the paroxysm itself is occasionally preceded by precursory symptoms, such as headache, vertigo, heat of skin, hallucinations, scintillations, imperfect vision, ringing in the ears, difficult articulation, mental irritability, pain in the præcordial region, nausea, vomiting, irregular pulse, and considerable lassitude without any apparent cause.

The paroxysms generally occur either in the latter months of pregnancy, or during or soon after labour; they are commonly preceded, sometimes for several weeks, by œdematous swellings in various parts of the body, although this is not always the case. The œdema occurs less frequently in the upper half of the body than in the ankles and labia; but in the latter months it is sometimes diffused over the whole body. With œdema of the face there is generally more fever, suffusion of the eyes, and redness of the cheeks, so that the countenance assumes a bloated appearance. These dropsical swellings are very uncertain, sometimes diminishing, or even entirely disappearing towards the completion of labour; whilst the quantity of albumen in the urine, and the disease of the kidneys themselves, may be on the increase. The skin of those parts of the body which are not œdematous is dry, and frequently assumes a chlorotic appearance, its temperature being at the same time reduced. The œdema which is connected with albumen and fibrinous cylinders in the urine stands in intimate relation with eclampsia. Anasarca of the lower limbs and hydramnios are frequently met with without a trace of albumen or casts of tubes in the urine, and are then never accompanied with eclampsia during the several periods of pregnancy or labour.

In eclampsia the urine is generally acid, always deposits, with nitric acid and heat, a large quantity of albumen, and exhibits under the microscope fibrinous cylinders, and often also blood corpuscles. The quantity of albumen corresponds with the extent and duration of Bright's disease; and increases in the latter months of pregnancy, and with the attacks of eclampsia.

The normal proportion of the constituents of the urine undergoes the following alterations, viz. :

The uric acid is diminished.

Urea diminished or almost wanting.

Chlorides but little altered.

Sulphates and phosphates either diminished or increased.

Uroxanthin increased.

The specific gravity of the urine varies from 1010 to 1030. If the sediment of the urine be examined in the first 24 hours, blood and mucous corpuscles and epithelial scales will be found, but these disappear when decomposition ensues. The more acute the disease of the kidney, the more cloudy the urine, and the greater the number of blood-corpuscles. There is frequently also increased sensibility of one or both kidneys under pressure externally, and sometimes various affections of the digestive organs. Eclampsia often occurs in first labours, but seldom recurs in future ones.

As neither time nor space will allow us to enter fully into the consideration of each division of Dr. Braun's interesting article, we shall, on the present occasion, content ourselves with presenting to our readers a recapitulation only of the views he entertains upon the subject.

1. Convulsions in females during the generative period, depend either upon hysteria, epilepsy, diseases of the brain, mineral or vegetable poisoning, or upon uræmic intoxication.

2. The most frequent cause of eclampsia is uræmia and Bright's disease.

3. Chronic hysteria, and epilepsy, exert no injurious influence upon pregnancy and labour, or upon the life of the foetus; they are never connected with Bright's disease, nor are fibrinous cylinders or a large quantity of albumen found in the urine.

4. Primary diseases of the brain, as apoplexy, meningitis, &c., are rarely causes of convulsions; and when they occur simultaneously with Bright's disease, they are the consequence, not the cause, of the convulsions.

5. All forms of convulsion, with their different causes, may occur without pregnancy.

6. They may also occur in males from any of the above-mentioned causes, except hysteria.

7. Amongst the more important causes of Bright's disease in the pregnant, may be considered venous congestion of the kidneys from the pressure of the enlarged uterus and distended abdomen, as well as the sanguineous diathesis often connected with pregnancy.

8. In *nephritis diffusa*, urea becomes detained in the blood, is converted into carbonate of ammonia, and then excites convulsions.

9. In cases of Bright's disease during pregnancy, if carbonate of ammonia be found in the blood, a speedy outbreak of convulsions may be expected.

10. No convulsions, however, occur in Bright's disease in the pregnant, provided but a small quantity of urea accumulates in the blood, or does not become converted into carbonate of ammonia.

11. The strong action of the uterus during labour is not the cause of this conversion, or of the uræmic convulsions; for these occur nearly as frequently without labour, during pregnancy and in the puerperal state; and sometimes in those who are not pregnant.

12. The abortions which frequently occur in uræmic convulsions are the effect of the uræmia, not the cause of the convulsions.

13. Eclampsia—i. e., uræmic convulsions—has no immediate connexion with the pains or process of labour.

14. The albuminuria cannot be the result of congestion occasioned by the convulsions, for it precedes the uræmic convulsions for days or weeks,

and does not, as a rule, occur under other forms of convulsion, as from hysteria, epilepsy, &c.

15. Attacks of eclampsia *during pregnancy* may cease, and the albuminuria continue.

16. But if the attacks cease *after labour*, and there be present only a slight degree of Bright's disease, the albuminuria disappears after a few days; but continues for a longer time if the disease of the kidney be in a more advanced stage.

17. The diminution in the size of the uterus from the completion of labour contributes very much to the disappearance of albuminuria, when there is neither fatty degeneration or atrophy of the kidneys.

18. Bright's disease (without convulsions) never gets well during pregnancy; after labour it mostly disappears within a much shorter time.

19. Albuminuria will be found in all cases of eclampsia not dependent upon hysteria, epilepsy, primary cerebral diseases, or poisoning.

20. Epilepsy may take place simultaneously with Bright's disease and uræmic convulsions; and Bright's urine may be found in an individual affected with habitual epilepsy.

21. With a frequent repetition of uræmic convulsions the foetus dies, through the injurious influence of blood impregnated with carbonate of ammonia. The life of the foetus is not compromised by the mechanical effects of convulsions which depend upon hysteria, epilepsy, or diseases of the brain.

22. Examination after death from uræmic convulsions always discovers Bright's disease of the kidneys; more frequently in the primary than in the latter stages (of fatty degeneration and atrophy).

23. Edema and anæmia of the brain are as frequently found after death from eclampsia, as hyperæmia and consecutive apoplexy.

24. Reflex sensibility is entirely abolished during each uræmic attack, but during the intervals loss of consciousness generally increases.

25. The injurious effects of venesection generally in eclampsia have been observed by Kiwisch, Litzmann, Sedgwick, Blot, and King, and the uncertainty of the practice has been confirmed by our own experience. In cerebral eclampsia, however, venesection is a proper remedy.

26. Chloroform inhalations are the best means of mitigating and bringing to an end uræmic convulsions, either during pregnancy, labour, or in the puerperal period.

27. The diuretics most to be relied upon for the relief of uræmia and Bright's disease are the benzoic, citric, and tartaric acids.

28. Bright's disease during pregnancy admits of relief only, not of cure.

29. Lessening the size of the uterus, and removing the child, are the most effectual means of curing the affection of the kidneys and uræmic intoxication.

30. Exciting and hastening labour by artificial means diminishes the danger both to mother and child arising from uræmic convulsions.

31. Artificial premature delivery is not, as a rule, to be resorted to in Bright's disease; but is to be recommended in actual uræmic convulsions.

32. The most appropriate method of producing premature labour, and

hastening delivery in cases of uræmic eclampsia, is by forcibly dilating the vagina by means of a caoutchouc apparatus (*Colpeuryse*).*

We trust we have thus introduced to the profession sufficiently, though imperfectly, the more recent views entertained by our German *confrères*. We shall, perhaps, return to the subject at some future time, as well as offer a few observations upon other articles contained in the volume before us. Meanwhile, we would strongly urge upon our brethren the more accurate study of puerperal convulsions, aided as they now will be by the glimpse we have afforded them of what is most novel in the investigation.

E. Copeman.

REVIEW V.

Beiträge zur Vergleichenden Pathologischen Anatomie der Gelenkkrankheiten. Von Dr. E. GURLT, Prakt.-Artze und Assistenz-Artze der Chirurgisch-Augenartzlichen Klinik der Universität zu Berlin. 1853.
Contributions to the Comparative Pathological Anatomy of Diseases of the Joints. By Dr. E. GURLT.

(Continued from No. 26, p. 359.)

WE proceed with the review of this well-compiled and highly instructive work, confining our remarks on the present occasion to the pathological anatomy of the joints of the superior extremity. The information, however, collected and arranged by Dr. Gurlt is in so condensed a form, that the perusal of the work itself can alone do justice to the author: and we recommend it most strongly to those familiar with the German language.

Diseases of the Clavicular Articulations.

If it were true that constant motion, the support of a weight, and exposure to cold, were sufficient to determine in joints the relative frequency of disease, we should expect to find its traces nowhere more often than in the acromio and the sterno-clavicular articulations; whereas, the opposite is the case. Indeed, the sternal joint is, according to Gurlt (p. 233), the most rarely affected of any articulation in the body. Guy's Hospital Museum contains a specimen (No. 1092) of complete bony ankylosis of the clavicle with the sternum, history unknown; but there is none such in the museum of St. Bartholomew's; and the cases of resection upon record, either resemble that of M. Davie, who operated to relieve the œsophagus from pressure caused by displacement of the clavicle consequent upon distortion of the vertebral column; or that of Dr. V. Mott, of New York, who took away the entire bone for osteo-sarcoma (i.e., osteo-cartilaginous growth). We have before remarked upon the importance of the extent and relations of the synovial membrane; and upon the favourable influence exerted as regards the production of disease, by an interarticular fibro-cartilage, which subdivides the joint, and affords

* "Of the various means of producing labour artificially in cases of uræmic convulsions before dilatation of the parts has taken place, we prefer an invention of our own consisting of a tent (*tamponade*), made with a bladder of caoutchouc, by means of which strong pains are excited, and the wished-for dilatation of the orifice is effected entirely without danger." (p. 347.)

under all circumstances a soft and well-fitting pad, against which the bones may press easily in every movement.

Chronic Inflammation of the Acromio-clavicular Articulation occurs usually, but not always, associated with similar disease in the shoulder-joint. The acromial end of the clavicle is looser than natural, and rises from its connexion with the scapula, as if it were a case of partial traumatic luxation. The articulating surfaces are deprived of cartilage, broad and flattened, and surrounded by osteophytes. The capsule is roomy and loose; the fibrous tissue is thicker than natural, and fibro-cartilaginous growths often project from its inner surface. Gurlt refers to cases related by Hamilton Lebatt, R. Adams, and E. Canton. He quotes the particulars, also, of two preparations; one from the Museum of the Royal College of Surgeons of England, the other from the Anatomical Museum of the University of Berlin. In the former, the corresponding articular surfaces of the clavicle and scapula are enlarged and rough, and perforated by numerous small round openings; the margin of the joint is surrounded by new bony growths, in the form of hard, knotty masses. The opposite acromio-clavicular articulation was similarly diseased. In the latter, a case of chronic inflammation of the shoulder joint, the clavicular end of the acromion is thickened; bony knots surround the articulation; the cartilage is absorbed, and the opposed osseous surfaces are polished; the capsule is in its normal state. We have dwelt upon this subject because we have, from examinations in the dissecting room, ascertained the frequency of the disease, and the great tendency among those who give the parts but a cursory examination to refer the morbid appearances to external injury. Simple dislocation of the acromial end of the clavicle is not very common, but is easily diagnosed, and, as far as we have observed, admits of ready cure under proper treatment, leaving no deformity of the bones.

The Shoulder Joint.

When the diseases of the shoulder joint are considered generally, it is found that the different varieties of inflammation are relatively rare; but that the consequences of external injuries are more frequent than in other articulations of similar size. Synovitis, according to Gurlt, is the most uncommon; then follows chronic inflammation, with ulceration; then gouty deposits, which are found only when other joints are similarly affected; cancerous growths are excessively rare; hydatids may be regarded as a curiosity. But there are many kinds of congenital luxation, and frequent specimens of traumatic luxation; although it is necessary to distinguish between the effects of chronic disease and cases in which, after accident, the bones have not been replaced during life. Gurlt thinks it doubtful whether the long tendon of the biceps can be displaced from the inter-tubercular groove solely as a result of accident, in which opinion we agree. Fractures of the upper extremity of the humerus are common; those of the scapula are most rare. We shall proceed to examine, in the first place, the different inflammatory affections.

Inflammation of the synovial membrane usually ensues as a consequence either of rheumatism or of general purulent infection. Bonnet mentions

the particulars of two cases of the former variety: 1. *Chronic inflammation of the scapulo-humeral articulation; absorption of the cartilages; ulceration of the articular surfaces.* 2. *Chronic rheumatism of the shoulder, with absorption of the cartilages.* The effects of acute rheumatic inflammation in the shoulder-joint are precisely similar to those witnessed in similar structures in other parts of the body: the synovial membrane acquires a red hue from the congestion, in its entire extent, of its bloodvessels, which abruptly cease at the margin of the articular cartilages. The cavity is filled with a whitish homogeneous pus, and similar changes take place in the contiguous bursæ. We believe that this is, at least, one form, and perhaps the principal, of that disease, which ultimately involves every articular structure, and which Gurlt (op. cit.) describes under a distinct head; for, as the symptoms proceed, the fibrous capsule becomes thickened, and perhaps contracted and perforated, by fistulous openings; the subcutaneous tissue is infiltrated by a gelatino-lardaceous substance; the surrounding muscles waste, and the integument, red and shining, presents numerous ulcerated openings, leading through the capsule to denuded bone. The spongy texture of the humerus, exposed by being deprived of its cartilage, is filled with a fibrinous or purulent exudation, and frequently there is absorption or destruction of the opposed articular surfaces, producing irregularities, depressions, and vacuities. It is obvious that these appearances ensue from inflammation extending to the bone, for purulent infiltration has been noticed by Löbl* in the coracoid process, and adjacent parts of the scapula. In the head of the humerus, cavities have been found, filled by pus,† or by pus and necrosed bone;‡ and osteophytes, of different forms, become developed around the whole circumference of the affected joint, generally limited to the neck of the scapula and of the humerus, but occasionally extending to the coracoid process, and the acromial end of the clavicle.

Collections of matter within the joint follow the course of the long tendon of the biceps, or the tendon of the subscapularis, both of which are invested by a prolongation of the synovial membrane. In the first case, the pus presents at the anterior border of the deltoid muscle; in the second, behind and below the shoulder, between the muscle and the subscapular fossa. In other cases, the matter bursts into the axilla, or runs along the course of the upper arm, governed by peculiarities in the case. The neighbouring bursæ become similarly affected, but Gurlt has met with no instance of consecutive dislocation of the humerus.

We repeat our belief that, from the history and symptoms characterizing these cases, the disease usually commences in the synovial membrane, and spreads thence, in its onward progress, to other structures—namely, the fibrous membrane and the bone. Cases are upon record where, in other regions of the body—the knee or ankle—inflammation has spread from the bone towards the interior of the joint, but, in such instances, there is usually an absence of that steady progress from bad to worse, which marks the disease of the shoulders. A painful condition of the limb near the joint, accompanied by swelling of the bone, is fol-

* Zeitschrift der K. K. Gesell. der Aertze zu Wien, 1844, Band i. § 161.

† Bonnet: *Maladies des Articul.*, tom. ii. p. 579.

‡ Meinel: *Prager Vierteljahrsschrift der prak. Heilk.*, 1852, Band iii. § 68.

lowed, at some uncertain period, by rapid extension of disease to the articular cavity, into which one or more pieces of hardened and dead bone are thrown from the inflamed cancellous texture. The immediate symptoms are, pain of the most agonizing character, severe constitutional disturbance, under which the patient might sink, were relief not afforded by amputation. The joint, when opened, exhibits destruction of the cartilage, thickening of the synovial membrane, and softening of the ligament; and the cavity contains fragments of bone, soaked in pus, often foetid, and sometimes discoloured. Such is not the usual history preceding the cases of disease of the shoulder-joint. The progress of the symptoms indicates distension of the synovial membrane, thickening and tenderness of the capsule, and, subsequently, ulceration of the cartilage, and the exposure of the inflamed bone, when abscesses form and burst externally, leaving fistulous passages leading to the morbid parts.

The principles of treatment laid down by Bonnet* in acute arthritis, or rheumatic synovitis, are such as are usually pursued: loss of blood when the congestion is active; narcotics when the pains are severe. In general, one commences by the employment of leeches, emollients, and narcotics; in a more advanced stage, mercurial frictions and different kinds of poultices are employed, according to the case; finally, blisters, or the pomade of nitrate of silver, are necessary. When the acute inflammation shows a tendency to pass into a chronic state, gradual and passive motion should be substituted for perfect immobility, and, subsequently, compression; the douches, mercury, colchicum, and iodine, may be administered according to the symptoms.

But should the joint pass into that state last described, when the inflamed and denuded bones rub against one another in every movement of the arm, to the great distress of the patient, to whom such a limb is useless, the question arises, whether, by operation, the articulating surface of the humerus, or of the scapula, or of both, may not be advantageously excised. This joint, both as regards the conformation of the bones and the relations of surrounding parts, is favourably circumstanced for the operation of resection, and we should not hesitate to recommend it, although, perhaps, the results of cases are not quite so flattering as some authors have maintained. In thin subjects, the head of the bone may be reached by a simple incision; but in more muscular persons, the formation of a flap is necessary. Moreau made it quadrilateral, the attached part, or the base, being downwards; Manne, quadrilateral, the base being superior; Sabatier, triangular, the base superior—he even removed it altogether; Morel, semilunar, the base superior; Syme makes first a longitudinal incision, of three to four inches, through the middle of the deltoid; then, from the lower extremity, he carries another to the posterior part of the axilla. The head of the humerus can be easily raised, and removed with a saw; the articular surface of the scapula can be also taken away when necessary; and certainly it is important to act as much as possible upon the diseased structures, for, should aught remain to impede the fibrous union of the deeper parts, and the closure of the external wound, the joint becomes the seat of frequent inflammations, abscesses form and burst, leaving fistulous passages, the skin assumes a

* *Traité Thérapeutique des Articulations*, p. 154.

red hue, the subjacent parts are œdematous, and the limb is perfectly useless.

We do not, however, purpose, in the present review, entering upon the question of treatment; our limits confine us to the point of pathological changes.

Chronic Inflammation of the Shoulder Joint.—Until recently this disease has been confounded with partial luxation of the humerus; it is now ascertained that the number of instances of the latter condition is much fewer than formerly supposed, and to two of our countrymen—namely, Mr. R. Adams* and Mr. Edwin Canton,†—is due the merit of having put this subject in its proper light. This disease, probably, does not commence in the synovial membrane. The principal changes are those which the osseous parts of the joint undergo. The upper arm becomes enlarged, but not equally so, inasmuch as the tubercles and the articulating surfaces show increase of substance: the latter are flattened, and become extended, so as to involve the former as well as the upper part of the inter-tubercular, or the bicipital groove. Upon examining the head of the bone we may find it covered partially or entirely by cartilage, which has undergone fibrous degeneration; converted into an irregular knotty eminence, or perfectly smooth, denuded of cartilage, and eburnated. All these conditions may be met with in the same specimen; the polished surface being confined to those parts where constant friction has gone on during life. Numerous bony growths, osteophytes, are developed around the articulation, where they tend to limit and restrain the free movements of the limb. When the head of the bone rests partly in the normal cavity, and partly in a new one below, it is marked by a transverse groove, formed by the pressure of the lower margin of the glenoid cavity.‡ The glenoid cavity is usually enlarged; it acquires twice its normal extent; loses its oval form, becoming circular, and the glenoid ligament disappears mostly at the inferior border.

In another class of cases, the articular surface of the scapula becomes extended upwards and inwards; the superior surface, separated from the lower by the remains of the normal margin of the glenoid cavity, extends as high as the acromion and the coraco-acromial ligament; and fibrous bands pass between the contiguous structures.

Smooth surfaces are constantly found on the coracoid process, the acromial end of the clavicle, and the acromion, which in some cases is hypertrophied, and divided along the line of its epiphysis into two, thus resembling a fracture. These are the morbid changes which precede the displacement of the head of the humerus, generally upwards under the acromion, or inwards under the coracoid process. More rarely, the displacement is backwards, on the costa of the scapula, where the new cavity is then formed, or downwards into the axilla. It can be understood from this description how easily such cases might be mistaken for non-reduced luxations; and the idea would receive further support from the examination of the interior of the joint. The long tendon of the biceps is rarely

* Todd's Cyclopædia of Anat. and Phys., Art. Shoulder Joint.

† London Medical Gazette, vol. iv., 1848, p. 410.

‡ Bonn: Commentatio de Humero Luxato, Lugd. Bat. et Amstelod, 1782. Hargrave: Edin. Med. and Surg. Journal, vol. xlviii. 1837. Sandifoot: Mus. Anat., Tab. 99, fig. 1—4, p. 222.

in its integrity; when whole, it is often pushed from the bicipital groove, flattened and spread out, and separated into three or more strings. But generally, this part of the tendon is absorbed, and the corresponding belly of the biceps muscle is atrophied. The fibrous capsule is thickened and hypertrophied (in some cases it is thin), being usually more capacious than natural, having acquired attachments corresponding with the increased extent of the articular cavity. The synovial membrane is converted into a dark-red, or reddish-brown substance, having a villous appearance, and containing bloodvessels and fat. In it we often find those fibro-cartilaginous bodies which become ultimately loose in the joint. The bursa mucosa subdeltoidea is often morbidly distended,* and occasionally communicates† with the synovial cavity. The surrounding muscles waste, their tendons becoming blended with the fibrous capsule. The tendons of the supra spinatus, infra spinatus, teres minor, and subscapularis, may separate from their tubercles; and with these changes, not uncommonly simultaneously affecting both the right and the left sides, the acromioclavicular articulations present those changes mentioned in a previous section.

The determination as to whether a case be one of chronic inflammation of the shoulder joint, or of non-reduced dislocation, is settled‡ by the examination of the head of the humerus. If we use the information deduced from a consideration of what occurs in the hip joint, where, from the shape of the acetabulum, no confusion can occur, it will be found that the head of the dislocated femur becomes in time atrophied and smaller, not enlarged and flattened; and hence the consolidation of the head of the humerus, its increase in size, the eburnation of its surface, and the development of osteophytes, give evidence, conclusive to the minds of most, of chronic inflammatory action. The opinion is further confirmed, when it is seen that similar changes have occurred symmetrically in both shoulders; for experience teaches us, that double traumatic luxation is of very rare occurrence, and not necessarily followed by changes in the bone. Scattered through the journals are notices of such accidents, but they are quite exceptional.§

The Dislocation of the long Tendon of the Biceps from the inter-tubercular groove, as described by Soden, is referred by Gurlt to the changes consequent upon chronic inflammation. Upon this point further observations are requisite. The rarity of fracture of the neck of the scapula is properly insisted upon. The fractures of the head of the humerus he arranges in the following table:

“A. FRACTURE THROUGH THE ANATOMICAL NECK. (INTRA-CAPSULAR FRACTURE.)

a. *Fracture without impaction.*

α. Simple fracture through the anatomical neck.

β. Fracture through the anatomical neck, with luxation of the upper arm.

* Adams: op. cit., p. 595.

† Gregory Smith: Lond. Med. Gaz., vol. xiv., 1834.

‡ Gurlt, p. 275.

§ Coote: Hospital Reports in Lancet and Med. Times and Gaz., 1854.

b. Fracture with impaction.

- α.* Simple fracture through the anatomical neck, with impaction.
- β.* Fracture through the anatomical neck, with impaction and separation of the tubercles.
- γ.* Fracture through the anatomical neck, with *inversion* of the upper fragment, and impaction.

"B. SEPARATION OF THE GREATER TUBERCLE.

"C. SEPARATION OF THE LESSER TUBERCLE.

"D. SEPARATION OF THE EPIPHYSIS FROM THE DIAPHYSIS (IN CHILDREN), OR FRACTURE THROUGH THE FORMER LINE OF SEPARATION (IN ADULTS). EXTRA-CAPSULAR FRACTURE.

- a.* Separation of the epiphysis.
- b.* Fracture through the ossified line of union.

"E. FRACTURE THROUGH THE SURGICAL NECK.

a. Fracture without impaction.

- a.* Simple fracture through the surgical neck.
- β.* Fracture through the surgical neck, with dislocation.

b. Fracture with impaction, the lower portion being driven into the upper.

Gurtt remarks, that in the intra-capsular form of fracture, *without impaction*, osseous union of the fragments is not to be expected; basing his view on the observation of Mr. Wilkinson King,* that in intra-capsular fracture the nutrient artery of the head of the bone is separated from the part which it is destined to supply. In three of the cases, however, which he has himself recorded, osseous union had taken place: in one only was the fracture united by a fibro-cartilaginous substance. In the other cases there appears to have been no attempt at repair. Whether impaction takes place or no, it is the lower fragment, without doubt, that undertakes especially the office of producing callus. On the whole subject, however, of fracture *with impaction*, special observations are still wanting.

A single case of fracture of the anatomical neck of the humerus with impaction, and separation of the greater tuberosity, is recorded by R. W. Smith.†

From the same author are quoted two other cases‡ of a very remarkable variety of fracture; where the head of the humerus, after complete separation from the shaft, has become completely *inverted*, so that the broken end came to lie in the cavity of the joint, and the articular surface on the fractured extremity of the shaft. In this position, the head of the humerus seems to have become impacted in the spongy portion of the bone on which it rested, and to have occasioned a new fracture, breaking off both tubercles and a portion of the bicipital groove. However, the nature of the second of the cases recorded seems to be in a great measure conjectural.

Separation of the greater tubercle (a very rare form of fracture) is necessarily attended by luxation; the fragment itself being forcibly drawn into the articular cavity. In an interesting case recorded by Mr. Hilton,§ the head of the humerus had escaped from the capsule, and lay

* Guy's Hospital Reports, vol. ii., 1844, p. 350.

† Smith on Fractures and Dislocations, p. 191.

‡ Loc. cit., p. 193.

§ Guy's Hospital Reports, vol. v., 1847, p. 93.

external to it, in a smooth cavity, hollowed in the free surface of the separated tubercle, with which, therefore, it articulated. The surfaces of this new joint are said to have been all invested with a synovial membrane.

A single and uninteresting case of separation of the lesser tuberosity is given by Thudicum.*

The separation of the epiphysis from the diaphysis in children, as well as fracture through the line of former separation† in adults, offer no points for special consideration; nor do the author's remarks on fractures of the surgical neck of the humerus present any features either of novelty or interest. When such fractures are attended by impaction, it is the *lower*, not the *upper*, fragment which is driven into the other; a circumstance which, according to the author, exercises no little influence on the subsequent osseous reunion of the bone.

Of fracture of the anatomical neck of the humerus with luxation, but without impaction, seven cases are enumerated. The head seems usually to be dislocated into the subscapular fossa; in a preparation in the Museum of St. Bartholomew's Hospital,‡ it had formed for itself a new articular cavity in this situation, in a mass of bone which had been deposited around it. In one case,§ (Lenoir's) the luxation took place on to the third rib. The shaft of the bone is ordinarily forced upwards towards the glenoid cavity, with which it forms more or less intimate connexions. In one of Sir A. Cooper's cases,|| the fractured extremity was wedged immovably into the articular cavity; in two others¶ it was so rounded off and polished as to form a sort of articulating surface, retained within the cavity by newly formed fibrous bands.

Of simple fracture through the anatomical neck of the humerus with impaction, Gurlt contributes no information beyond that afforded by English surgical literature. He quotes (p. 289) Smith's** case of a female, aged 52, in the shaft of whose humerus the articular head was so deeply imbedded between the tubercles, that it was below the level of the tuberculum majus. This latter process was broken off, dislocated outwards, and formed an obtuse angle with the external surface of the diaphysis. All the lines of fracture were united by bony substance. To this author we should refer our readers for information upon this point, as well as upon "Fracture of the anatomical neck with twisting of the upper fragment with impaction;" fracture and separation of the tuberculum majus and minus. Of separation of the epiphysis from the diaphysis a case is recorded by Reichel.††

Of fracture between the epiphysis and diaphysis (i.e., of the anatomical neck), of fracture of the surgical neck, with separation of the fragments, or with impaction, we need say but little. Upon these points literature is chiefly indebted to the labours of British and Irish surgeons, among whom the name of Sir Astley Cooper still stands prominent. It is true that we may not be able to diagnose these different forms of accident during life; that we may include them for practical purposes under the one head of "fracture of the humerus near the shoulder." But a proper

* Loc. cit., § 22.

† Sir A. Cooper: Guy's Hospital Reports, vol. iv., 1839, p. 277.

‡ Series III., Subseries C, No. 103.

§ Gaz. des Hôpitaux, 1851, No. 72.

|| Mus. Coll. Surg., No. 875.

¶ Guy's Hospital Reports, vol. iv., 1839, pp. 273—279.

** R. W. Smith on Fractures.

†† Diss. de Epiphys. ab Ossium Diaphysi Diductione, 1759.

knowledge of what may occur never can be otherwise than valuable in doubtful cases, while the fact of the possibility of impaction would deter the surgeon from making such violent and determined efforts to detect crepitus as were at one time considered necessary, though productive of intense pain.

The Elbow Joint.

The elbow joint is subject to the same diseases as other articulations. Simple synovitis is less common than that variety of inflammation, often of chronic character, which passes into ulceration of the articular cartilages. Hydatids have been observed by Gurlt; but cancer, he says, he has not witnessed here any more than in the shoulder. Gouty deposits occur, but in no very marked degree. Congenital irregularities of position affect the radius. There are occasional malformations; one instance of synostosis of the radius and humerus.* Luxations occur in great variety; and there are not a few specimens of united fractures. Finally, changes of considerable importance take place from long inactivity of the limb; namely, atrophy of the parts subjected to pressure, and of those from which pressure has been removed. In illustration of these points two cases are quoted from the 'Dublin Journal of Medical Science.'† In the first case, both elbow joints were in permanent flexion; the rotatory movement of the radius had been lost for many years. In the second, the changes occurred in an elbow, fixed in the extended position, following amputation of the fore-arm, performed many years previously. Here, too, there was no power of rotation, and the head of the radius was larger than the corresponding surface of the humerus.

Inflammation of the Synovial Membrane of the Elbow is pronounced by Gurlt a comparatively rare form of disease, because in many cases, where the joints were generally affected, this articulation escaped. He quotes, however, in illustration of his subject, the particulars of a preparation in the Museum of St. Bartholomew's Hospital (Ser. II., No. 12). "Chronic inflammation of the synovial membrane, of fifteen months' duration, in a man aged 60. The synovial membrane is converted into a light-coloured brown substance of firm texture, half to three-quarters of an inch thick, with white lines, which radiate in various directions: and it has a white smooth surface. The morbid change ceases at the border of the articular cartilage. Around the head of the radius, the thickened synovial membrane forms a fold, which projects into the joint; both cartilages and bones are healthy." We can assure the author that this disease is by no means uncommon in this country, especially among the young of the poorer classes. It is less common in adults, but even here it occurs under circumstances calculated to excite a subacute form of vascular disturbance.

In the investigation of an inflamed elbow joint in its advanced stages, there are found under the skin, ulcerated by fistulous openings, a lardaceous gelatinous infiltration of the subcutaneous tissues; generally the swelling is upon the posterior part of the limb, and presents a contrast with the structures anteriorly. The fore-arm is mostly in the half-flexed and prone

* Bulletin de la Soc. Anatom., 1836, tom. ii. p. 82.

† B. Adams: Dublin Journal of Medical Science, vol. xvii., 1840, p. 506.

position. There are frequently numerous abscesses in the proximity. The articular extremities of the bones are diseased; the cartilage is absorbed; the bones themselves ulcerated; and examination of the cancellous texture shows that the inflammation commenced there, and spread towards the surface. In such cases there are generally necrosed pieces attached to a bone, at the end of a fistulous passage, or loose in the articulation; in others, osteophytes grow from the bones. When the disease is confined to the radial side of the arm, the infiltration is limited to this region; the ligaments become softened and elongated, or even destroyed, and the head of the radius itself may protrude through an ulcerated opening of the skin.* The same process may take place on the opposite side of the limb with the internal condyle of the humerus. When the greater sigmoid cavity becomes enlarged by ulceration, the end of the humerus appears to project forwards, the radius and ulna being drawn in an opposite direction by the triceps. Pus in the joints usually collects in the posterior part, where the synovial membrane is loose, and it bursts either on the outer or inner side of the triceps, generally at this former spot. When it collects in the anterior part, it makes its way under the brachialis anticus along the upper arm, and bursts, as in the preceding case, on the outer or inner side of the limb, so that from a consideration of the situation of the fistulous opening, it is not always easy to determine the course which the matter has taken. Repair may take place by bony ankylosis; complete, by the firm union of all the bones; or partial, as, for example, union between the olecranon and the humerus,† or the ulna and the humerus. Or the ends of the bone may suffer great loss of substance, and unite by their flat surfaces at a right angle. Upon the diseased bones of the elbow, the operation of resection has been performed with great success, chiefly by Mr. Fergusson, in cases where nature seemed unable to complete the process of osseous repair.

A Chronic Form of Inflammation leads to removal of the articular cartilages, to eburnation of the bones, and the formation of deep lines or grooves in its substance, corresponding with the direction of movement. Fibrous growths and osteophytes abound; and new ligamentous bands are developed, passing from bone to bone. The synovial membrane is thickened and vascular; the fibrous capsule is thickened. In many cases there are loose bodies in great numbers, of all sizes, from that of a pea to a walnut. Some yet retain an attachment by a narrow pedicle. The largest are usually found attached to the coronoid process of the ulna, where they are enclosed in a process of synovial membrane.‡ Some are quite bony, others fibrous or fibro-cartilaginous. The bursa mucosa olecrani may be morbidly enlarged. No treatment can restore the healthy condition of a joint thus affected, nor are the symptoms during life such as to demand surgical interference.

Luxations of the bones composing the elbow-joint are common, especially among the young; but considerable difference of opinion still exists as to their varieties and number. The following table, arranged by Gurlt, will be perused with interest by most surgeons.

* Lobstein. *Traité d'Anat. Pathol.*

† Sandifort: *Mus. Anat.*, vol. iv., Tab. 160.

‡ Blazina: *Prag. Viertel der prakt. Heilk.*, 1844; and St. Thomas's Hospital Museum.

Luxation of both radius and ulna,	{	1. backwards,	{ a. complete. b. incomplete.
		2. forwards,	{ a. with fracture of the olecranon, b. without fracture of the olecranon.
		3. outwards,	{ a. complete. b. incomplete.
		4. inwards,	{ a. complete. b. incomplete.
		5. Simultaneous luxation of both bones in opposite directions.	
Luxation of the ulna,		backwards.	
Luxation of the radius,	{	1. backwards,	
		2. forwards.	
		3. outwards.	
		4. incomplete.	

We have not space in the present review to do more than direct attention to those varieties which are least common. Great praise, however, is due to Gurlt for the care and accuracy with which he has arranged a vast amount of information, collected from different sources.

Complete Luxation of both Bones of the Fore-arm forward without Fracture.—The coronoid process and the head of the radius were found upon the anterior surface of the humerus, above the articular surface; the extremity of the olecranon rested upon the lower border of the posterior supra-trochlea fossa, where it had made for itself a special cavity.*

Two cases of incomplete luxation backwards are mentioned by Gurlt; one related by Gély,† a second by Girdlestone,‡ who describes a specimen taken from the dissecting-room of St. Bartholomew's Hospital, by Mr. Holmes Coote. These appear to be the only two yet published, and they agree singularly in particulars; in both, the coronoid process was placed on the middle or the under part of the trochlea, and the head of the radius was close under the extremity of the humerus: new articulations had been formed around.

The Museum of the Royal College of Surgeons of England contains a specimen (No. 3278), in which the radius is dislocated forwards, and the ulna outwards instead of backwards, as is the more common.

Luxations of the head of the radius forwards or backwards are very common. One case of luxation outwards has been described by Nelaton:§ the annular ligament was of course ruptured.

Fractures about the elbow joint are arranged as follows:

“ 1. Fracture of the humerus, just above the condyles, or through them into the joint.

“ 2. Fracture of the external condyle.

* Anat. Mus. of the University of Berlin, No. 2253.

† Journal de Chirurgie, par Malgaigne, May, 1844, p. 139.

‡ London Medical Gazette, vol. xi. p. 138, 1850.

§ Ein von Nelaton beobachteter Sectionsbefund, in der Concours-These, von Hugnier, 1842.

- " 3. Fracture of the trochlea or internal condyle.
- " 4. Fracture of both bones of the fore-arm close to the elbow joint.
- " 5. Fracture of the olecranon.
- " 6. Fracture of the coronoid process.
- " 7. Comminuted fractures."

Fractures of the olecranon usually unite by ligament, but occasionally also by bone, as proved by a specimen in the Musée Dupuytren (No. 105). The head of the radius and the ulna are here dislocated forwards. The fracture, which was from above downwards, from before backwards, and from within outwards, had divided the ulna into two fragments. Instances are upon record in which the olecranon has not united, or the fracture has been but partially repaired. In such cases, the power of extending the fore-arm is much impaired, or even lost, and the limb is weak and comparatively useless.

Diseases of the Hand and Wrist.

In this important part of the upper extremity, where many small bones contribute to form the articulation, we find inflammatory affections passing into ulceration; chronic inflammation, which, however, more commonly attacks the carpo-metacarpal, or the metacarpo-phalangeal articulations, than the wrist. Gouty deposits are very common; cancer is rare. An instance of synostosis between the ossa semilunare and cuneiform is preserved in the Anatomical Museum of Berlin (No. 7363). A case of club-hand, from congenital deficiency of the radius, is described by Roger and Honel.*

Congenital luxation of the carpus has been noticed on the volar surface of the fore-arm, by Cruveilhier,† and both on the volar and dorsal surfaces, by R. W. Smith.‡

Of traumatic luxation there is no specimen, nor any well-authenticated case yet published.

When we consider how numerous and how complicated are the joints composing the wrist, how constantly, to the last hour of existence, they are in movement, to what violence they are exposed, we may wonder that disease is not of more common occurrence. Although Gurlt states that inflammation of the carpal bones is very frequent, yet he confesses to the lack of anatomical material whence to draw his description, a statement we should hardly expect, were morbid changes as abundant as he infers. As far as our observations extend, inflammatory disease of the wrist occurs chiefly (1) in persons of strumous habit, being of chronic character, and curable with difficulty, from the great tendency to relapse; (2) in persons subject to rheumatism or gout. We have seen it as a consequence of gonorrhœa, in a case where it produced much thickening of surrounding tissues, and great impairment of motion.

Redfern§ has accurately described the morbid appearances resulting from inflammation of the hand, the carpal, and the metacarpal joints in a man, aged 19. All the tissues around the carpus were infiltrated by a gelatinous fluid; fistulous passages extended in various directions; the

* L'Union Médicale, 1851, No. 140.

† Anat. Pathol., Livrais 9.

‡ Fractures in the Vicinity of Joints, p. 121.

§ Abnormal Nutrition in the Human Artic. Cartilages, p. 18.

bones were so firmly united, that they broke when forcibly separated, fragments remaining adherent to the remains of the articular cartilages. Where there was no cartilage, the bones were rough. The opposed surfaces of the radius and ulna were in the same condition; but the inferior extremity of the latter still retained its articular cartilage. The bones were tolerably firm. In the joint between the rows of carpal bones there was scarcely any cartilage left. Those composing the upper row separated easily one from another, also those of the lower, and between them was a white fibrinous mass. The carpal end of the fifth os metacarpi and the lower part of the os cuneiforme, were wanting, a mass of granulation occupying the space. The Museum of St. Bartholomew's Hospital contains several specimens in illustration of this subject. The carpal bones (Ser. I., No. 40) may be nearly destroyed by deep ulceration, osteophytes may form around, the radius may become necrosed at its lower extremity (Ser. II., No. 13), and the surrounding tissues hardened; but further examination of pathological collections proves, that in the wrist there is a power of repair sufficiently great to induce the surgeon to delay as long as possible any operation by which the patient would be deprived of a part so useful as the hand and fore-arm. In the Anatomical Museum of the University of Berlin, there is a preparation (No. 6853) of complete ankylosis of the hand and carpus. The os metacarpi pollicis alone is movable. A similar preparation exists in the Museum of the Royal College of Surgeons of England (No. 3350). The same fact is illustrated by specimens in the Museum of Guy's Hospital, of the Surgical Klinik of the University of Berlin, and by a case recorded by Sandifort.*

Chronic Inflammation occurs under various forms in the wrist. The Museum of the Royal College of Surgeons of England contains two specimens (Nos. 627 and 627 A), in which bony processes have been developed around the inferior articular surfaces of the radius and ulna; and St. Bartholomew's Museum possesses one in which the inferior articular surfaces between the radius and ulna are hard, polished, and grooved, and new bone has been formed around (Sub-ser. II. B, No. 37). But the most common disease is that dependent upon gout, which more especially attacks the smaller joints, but yet occasionally leaves its deposits in both carpus and metacarpus. The concretion has been found also both in the spongy texture of the bones and along the course of the tendons.

The only case of cancer affecting the wrist, recorded by the author, is that related by Mr. R. Adams.†

Congenital Luxations at the wrist constitute one of the varieties of club-hand. There is usually no trace of the carpal articulating extremity of the radius, but a new joint is formed, either upon the dorsal or volar surface, with which the atrophied carpal bones articulate, at an angle. The ulna, generally longer than the radius, preserves more of its normal form. Cruveilhier‡ has recorded a case of luxation upon the volar surface of the fore-arm, in an adult female, the hand being united to the radius at a right angle in flexion; the radius was short and deformed, but a strong process connected it with a cavity in the ulna. Mr. Smith, in his excel-

* Mus. Anat., vol. iv. Tab. 156; and vol. iii. p. 246.

† Dublin Journal of Medical Science, vol. xvii. p. 490.

‡ Anat. Path., avec Planches, Livrais 9, Pl. 2.

lent work, so often quoted,* gives an instance of double congenital luxation, one on the volar, the other on the dorsal surface, with an accurate account of the condition of the carpal bones, and the relations of the surrounding tendons; but of traumatic luxation, Gurlt, in common with other authors who write either from personal experience, or with a careful analysis of the evidence which they bring forward, can furnish no example. The "dislocated wrist," so commonly talked of, not twenty years hence, as demanding sudden and violent extension, proves either a simple sprain, or fracture of the lower extremity of the radius. The twisting of the hand from cicatrices, of which Cruveilhier speaks; the sub-luxation among particular classes of workpeople, mentioned by Dupuytren, cannot properly be included under this head. The only instance of which we have heard (the particulars are not published), where the possibility of such an accident can be admitted—and we must confess that the evidence is doubtful—occurred to one of the workmen employed in raising a large and heavy piece of stone, during some works in St. Bartholomew's Hospital. The stone suddenly slipped from the machine a short distance, against the hands extended and pressing upon the piece, which was of large size and enormous weight. It was affirmed by the gentleman who saw the case, and immediately did what was necessary, that he felt the extremity of the carpus lying upon the dorsal surface of the fore-arm: reduction was easy, and the displacement did not recur. In most cases of doubtful injury to the wrist, the radius is broken close to the joint, the lower fragment being drawn backwards. When the accident is properly treated, union takes place readily by bone; but when the surgeon, labouring under the mistake that the bones are merely dislocated, subjects his patient to frequent attempts at reduction, the surrounding tissues inflame, the tendons become agglutinated, and the movements of the hand and fingers are permanently impaired. In Ireland, this is called "Colles' fracture."

The metacarpal and phalangeal articulations exhibit, in small, the same changes which go on in the larger articulations of the wrist or elbow. The synovial membrane may suffer from either acute or chronic inflammation; gouty deposits are especially common; necrosis of the phalanges ensues, both as a consequence of acute inflammation, as whitlow, or in strumous subjects, where there has been no active disease. In all, we would impress upon the surgeon the fact that the parts possess great powers of repair, and that amputation should not be hastily recommended.

Upon the development of enchondroma, which is particularly common in the long bones of the hand, we must refer to works upon tumours of bone; no remedy exists save the removal of the limb.

Holmes Coote.

(To be continued.)

* *Fractures in the Vicinity of Joints*, p. 121.

REVIEW VI.

1. *De la Mort et de ses Caractères.* Par le Dr. JOSAT.—*Paris*, 1854. 8vo, pp. 375.
2. *Du Signe Certain de la Mort.* Par MICHEL-HYACINTHE DESCHAMPS.—*Paris*, 1854. 8vo, pp. 240.
3. *Traité des Signes de la Mort.* Par E. BOUCHUT.—*Paris*, 1849. 8vo, pp. 407.
4. *The Medical Aspects of Death.* By JAMES BOWER HARRISON.—*London*, 1852. Small 8vo, pp. 165.

THE subject of the treatises above named has, it is evident, attracted much greater attention on the Continent than in our islands. We have here three elaborate works recently published in France, each of which has its independently distinctive features; while of the solitary essay that has emanated from the British press, it cannot be said that its author has done more than reproduce, in a popular form, what has been already written by others. In an admirable article which appeared in the ‘Cyclopædia of Anatomy and Physiology,’ twenty years ago, the author, Dr. Symonds, quoted from two British, and at least a dozen foreign, medico-legal writers. In the bibliography appended to one of the treatises now before us, we count only four English names among the number of forty-seven authors who have treated of the signs of death. It cannot, however, be denied, that much of the material so abundantly produced has consisted of a repetition of the same facts, and, often enough, of the same falsehoods; for in some of these authors there is apparent a credulity equal to every story of live-death that ever was invented by the most incorrigible horror-monger. The object, however, of the essays now under consideration, has not been to perpetuate the incredible fabrications of excited imaginations, but philosophically to recognise undeniable accidents, and then scientifically to look for the means of their prevention.

We may, in the first place, clear our way by disengaging from our subsequent remarks Mr. Harrison’s brochure. We do this in all courtesy and respect for its author, who will be ready to admit that his book was not written to assist professional research, but rather to present, in a small compass and in a readable shape, a topic, painful, but useful to be thought upon by non-professional readers: nor with the intention of unfolding any new scientific views, or of suggesting any improved practice as regards interment, but partly to satisfy his own “natural curiosity about death,” and partly “to fill up the intervals left by more arduous professional studies.” We concede that he has, in thus addressing the public, manifested judgment and discretion in the execution of his object. The topics discussed by Mr. Harrison are premature burials, signs of death, putrefaction, modes of death, &c. The scope of the work required the illustration of these several topics by sundry quotations from the wide field of general literature—e.g., among others, ‘The History of the Tragedy at the Black Hole in Calcutta;’ ‘The Account of the Opening of the

Coffin containing the Remains of Charles I.;' 'Conversations of Barry O'Meara with Napoleon, upon the Easiest Mode of Dying;' 'The Narrative of the Death of Dr. Arnold, of Rugby;' *cum multis aliis*. The whole constituting, as we have said, a readable, popular exposition of a very solemn subject.

In making our readers acquainted with the contents of the three French treatises, we shall pass over the enumeration of the signs of death to be found in each, as these may be met with in every systematic work upon medical jurisprudence; confining our attention to those particular signs which are dwelt upon by each author as having the closest connexion with the great practical objects of all these investigations,—viz., the recognition of the state of apparent death, and the prevention of premature interment.

The treatise of M. Josat is, he informs his readers, the work of upwards of ten years' patient and conscientious research. His labours originated in the impression made upon the public mind in France by the frequent recitals of premature interments recorded in the newspapers. The government of Louis Philippe, in order to ascertain the truth or falsehood of these rumours, and with a view to their future prevention, instituted an official investigation, preparatory to a revision of the laws regarding interment. The burial of the dead had been already placed under wiser regulations in Germany, and to obtain a more intimate acquaintance with these the author was entrusted with a mission to the German States. The results of his inquiries are embodied in this work, of which M. Rayer, in his report thereon to the Academy of Sciences, observed,—“M. Josat's is not a work merely made up from other books, but it is a long and important work, which, for its own merits, deserves an attentive examination.”

M. Josat, besides treating of the ordinarily received signs of death, which he seriatim examines and criticises, gives a full exposition of the legislative enactments regarding interment at present in force in France and Germany. The author concludes from his researches that there is no single sign of death which can be regarded as satisfactory, except putrefaction. Having arrived at this conclusion, Dr. Josat enters upon the consideration of the subject of apparent death, and the means of preventing premature interment. These we shall presently more fully bring under the notice of our readers.

We proceed in this place to compare the views of Dr. Josat with those of M. Deschamps, who also examines and rejects all the signs of death, except that of green discoloration of the abdomen, which he asserts is an invariable and infallible proof that life is totally extinct. Dr. Josat sees the early evidences of decomposition, at uncertain periods after death, in the changes of colour presented by the integuments of the abdomen about the inguinal regions, which he says become of a dull, white, whitish-grey, blue, green, or black colour, and spreading over the abdomen as the tints deepen; and, at the same time, that a peculiar cadaveric odour becomes perceptible. The occurrence of the phenomena is much influenced by the nature of the disease which caused death, as well as other internal conditions of the corpse, and external circumstances by which it is surrounded. It is not, however,

pretended by Dr. Josat that these postmortem changes should be waited for in every case, as in the majority of instances of death no room for doubt exists. The views of M. Deschamps, although seemingly similar to those of Dr. Josat, are not precisely the same. M. Deschamps asserts, with the fullest confidence, that the single phenomenon of green discoloration of the abdomen is a sufficiently certain sign of death. It is, therefore, not required to wait for further putrefaction, as this green discoloration, this veritable stamp of death, precedes other putrefactive changes in the structure of the abdominal parietes, and is unattended with fetid odour. The same discoloration perceived on the limbs, the author says, is not an equally sure sign of death, since these may, at any time, during life, be removed without destroying the body. The author points out that the period at which this sign may make itself seen will vary. Generally, it will not be present until the warmth of the body has passed away. It coincides most frequently with the occurrence of cadaveric rigidity. It is absent, also, so long as the muscles retain their susceptibility to galvanic stimulus. We must here remark, that in reading Dr. Josat's work we have met with an exception to this rule, in the case of a body the arms of which were susceptible to galvanic excitation although perfectly cold, and had passed through the state of cadaveric rigidity, having returned to that of cadaveric pliancy, and being even partially decomposed. M. Deschamps remarks, that the appearance of the green discoloration may be retarded from twelve to fifteen days by an exposure to a freezing temperature, while on the occurrence of a thaw putrefaction will rapidly supervene. A corpse exposed during one day, in a chamber at a temperature of from 65° to 75° Fahr., will often exhibit this sign of death by the evening. At higher temperatures,—e.g., from 130° to 140° Fahr.—the corpse is dried, and putrefaction is arrested. In death from acute inflammations and effusions, in puerperal fever, in dropsies, in infancy, the green patch occurs quickly. It appears, also, earlier in females than in males; in the young than in the old. It is stated by M. Deschamps that the foetus that has died in utero does not present a green, but a red discoloration of the abdominal integuments. The appearance of this sign is retarded by submersion in water, but it still appears first on the abdomen. A range of from a few hours to twenty days has been noticed by M. Deschamps with regard to the date of its appearance;—the average would seem to be about three days. Its occurrence is accelerated by all those conditions which preserve the warmth of the body, as well as those which accelerate or promote putrefaction; it is retarded by very low or very high temperatures, and by the use of antiseptics. As the phenomenon in question is caused by air, warmth, and moisture, so its appearance can be accelerated by the exposure of the corpse to these conditions in a chamber; its advent can there be watched, rendering it unnecessary to expose survivors to the risk of the noxious exhalations of putrefaction. The seat of the discoloration is subepidermic. Under the microscope, it is seen to be composed of green granules, resembling the minute structure of vegetable mould. No other discolorations caused by disease, the author asserts, can be mistaken for this, his infallible sign of death.

We may here insert the statistics on the probable duration of apparent

death, which M. Josat, casting aside the marvellous improbabilities that have been written, gives from his own observation in 162 instances. The state of apparent death lasted in—

	7	from 36 to 42	hours.
20	„	20 to 36	„
47	„	15 to 20	„
58	„	8 to 15	„
30	„	2 to 8	„

The order of frequency of diseases in which these occurred was as follows: asphyxia, including still-births; syncope; hysteria; apoplexy; narcotism; concussion of the brain. The apparent death lasted longest in hysteria, and shortest in concussion.

We have, in the next place, to direct attention to the leading characteristic of M. Bouchut's essay, which, allowing all other signs of death to be fallacious, claims the merit of certainty for the absence of the sounds of the heart when prolonged beyond one or two minutes. This opinion is founded upon the physiological axiom which regards the heart as the *primum vivens, ultimum moriens*, and has been strengthened by the author's observations and researches in cases of syncope, lethargy, poisoning by narcotics, &c. Notwithstanding, however, that M. Bouchut's researches have been extensive, and scientifically conducted, his conclusion is not so trustworthy as he himself, and the Commission of the Academy of Sciences, were willing to believe. M. Josat has recorded several instances wherein newly-born children have been most carefully examined during several minutes, without the detection of the slightest cardiac sound or movement, and yet these have rallied and lived. M. Depaul has collected ten similar instances. M. Brachet has recorded* an instance of a man in whom neither sound nor movement of the heart could be heard for eight minutes, and who nevertheless survived. Another adult case is mentioned by Dr. Josat, as having been witnessed by M. Girbal, of Montpellier.

The results of experiments that have been instituted by physiologists, to ascertain the actions of various poisons, have also tended to conclusions adverse to those of M. Bouchut, and to the axiom with which he starts. The heart has been seen beating in the exposed thorax of an animal several minutes after life has been extinct, and even after the brain and spinal cord have been destroyed. Sir B. Brodie and others have described children born without hearts. The circulation is maintained at one period of human life without the aid of the heart. It is, besides, quite consistent with the facts observed in hysterical and other morbid conditions of the nervous system, that the action of the heart, like that of other muscles, should be so extremely feeble as not to be cognisable by any sound or impulse, and yet it may have sufficient movement slowly to move the blood through a system whose every function and endowment is suspended, and all but annihilated. In cases of catalepsy, and of authentic instances of apparent death, the respiratory muscles have not been seen to move, yet inspiration and expiration, however slowly and

* Bulletin Thérap. Méd., tom. xxvii. p. 371.

imperceptibly, must have taken place. So may it be with the heart. If so, then M. Bouchut's test loses its character of infallibility.

Having in the preceding observations dwelt upon some of the most prominent features of these works, we conclude with a brief abstract of the results of Dr. Josat's investigation on the modes of preventing premature interment, which he has found in force in the German states, as well of the provisions existing in France at the present time, with the author's proposed reforms.

The earliest movements in the direction of means for the prevention of premature interments originated with Winslow, in France, to whom followed other known French medical writers upon signs of death. It was Madame Necker, however, who embodied their suggestions in a practicable form as submitted to the National Assembly in 1792 by Count Berchshold. In the ninth year of the former French Republic a project was entertained for the erection of six "*temples funeraires*" in Paris, but came to no good end, as attendant evils preponderated. To Germany belongs the credit of having executed these designs in such wise that they should not prove the positive sources of more danger to the living than could be counterbalanced by the occasional preservation of an individual from the risk of premature interment. A belief in this same risk having been prodigiously diminished since the establishment of institutions for the reception of cases where doubt of the reality of death has existed, Hufeland, in Weimar, devised the plan that Frankfort-on-the-Maine incorporated with its reform in sepulture and establishment of extra-mural cemeteries in 1823.

Hufeland's plans have subsequently been adopted and carried out in many other German states. The arrangements described by Dr. Josat, as witnessed at Frankfort, represent those adopted in other parts of Germany. They are deserving of attention, both from their author's celebrity and from their intrinsic ingenuity.

The description of these establishments in Dr. Josat's work is accompanied by engravings, which facilitate the comprehension of their structural details. We can only lay before our readers their broad features. Attached to each of the public cemeteries, which are constructed on the heights surrounding the city, is on one side of a quadrangle, a series of buildings, comprising chapel, directors', and servants' apartments. On the other side is a *salle de veille*, or *watch-room*. In the whole length of this chamber is a series of glazed sashes, each corresponding to a separate cell, and above each window a loud bell, termed the *alarm-bell*. This ominous alarm-bell is struck by a hammer which communicates with the interior of the cell by a simple mechanism in contact with the body placed therein as one of reputed apparent death—and only under these circumstances.

Each of these *mortuary cells* is so built so to insure ventilation and temperature at the will of the director. In each is placed a stand, upon which is laid the bier conveying the body. The limbs of the body are so arranged and brought into connexion with the mechanism belonging to the alarm-bell, that it is impossible that the slightest movement can take place in the body without an alarm being given, and an attendant summoned. A medical officer, with competently educated assistants, are

constantly upon the premises, furnished with all the means and appliances for fostering any indication of returning life.

A body deposited in these institutions is detained until decomposition has commenced its operations, when the friends are informed thereof, and interment takes place in the cemetery in as quiet a manner as possible.

Dr. Josat detects several objections to various parts of the arrangements he has described. The most serious of these is that in certain cases of syncope and hysterical trance the first indications of life are to be traced in a slight change of complexion, or a scarcely perceptible movement of the eyelids, of the thoracic muscles, or of the heart, to all of which the apparatus would be unavailable.

In Mayence, Dr. Josat states that all bodies are exposed, until decomposition takes place, in large chambers constructed for the purpose. There bodies are often left so long that these places become charnel-houses, distributing far greater danger than the risk of premature burial. In Berlin also, and many other Prussian towns, bodies are exposed until decomposition occurs—but the bodies are here carefully watched and attended with the greatest care, by skilful officers, and interment takes place on the earliest appearance of putrefaction. We learn from a recent return that in Berlin, out of a population of 430,000, only 48 corpses were brought to dead-houses. Not a single instance has occurred of resuscitation.

In Austria, bodies are received into dead-houses on the request of the friends. In any case interment does not take place until sixty-two hours after death, as certified by a medical man.

In France, twenty-four hours is the term fixed by law for interment. Restoration from apparent death has frequently taken place later than twenty-four hours; consequently the law favours premature interment, and offers opportunities for the concealment of crime. An official medical attestation of death is required within the twenty-four hours, but it is obvious that this offers no protection or assistance to cases of apparent death lasting shorter periods, before the arrival of the attesting medical officer.

As a remedy for these dangers, the author proposes that no corpse should be interred until it presents signs of decomposition, which, he observes, will usually be apparent within seventy-two hours; that chambers should be attached to every cemetery for watching every reputed death, without regard to rank or condition of the individual; that a circular arrangement of cells constructed for the purpose would afford sufficient accommodation, according to the population or average mortality of each place. The author describes minutely the arrangements he suggests for the recognition of signs of life in the bodies thus exposed; these are modifications or improvements upon those of Frankfort.

As a sanitary measure, the separation of the dead from the living, especially from among the crowded poor, would be, apart from the not less important point of the verification of death, an incalculable benefit to France—to Paris: also to England—to London.

The histories of premature interments are in all cases to be received with hesitation. During epidemic visitations the most alarming accounts are greedily believed and magnified, all the while being totally devoid of truth. It is, however, not to be denied that such things have happened,

and may therefore happen again. It has been urged that, even admitting the possibility of a person being buried alive, it is not possible that he could wake to consciousness in so small a quantity of air as is enclosed in a coffin under ground; that there being little more than enough to fill the chest, it would become so quickly vitiated that real death by asphyxia would be almost instantaneous. The statement is plausible, but we fear is not enough to divest of its unutterable horror the very thought of such a predicament. The truth is, that facts are stubborn things, and will not do obeisance to all theories. It behoves us in this matter to learn another lesson from our neighbours, and to take measures to prevent the occurrence of catastrophes too fearfully horrible to contemplate in thought—too dreadful for the most vivid or the most morbid imagination to realize. Science can hold out no token by which to recognise the certainty of death. Sanitary police, at least in England, is indifferent about the risk of a few burials alive, and thinks it superfluous to prevent their occurrence.

W. B. Kesteven.

REVIEW VII.

1. *Cure and Prevention of Scarlet Fever.* By SAMUEL HAHNEMANN. (Lesser Writings of the Author, collected and translated by R. E. DUDGEON, M.D.)
2. *Travaux Thérapeutiques sur la Belladone.* Publiée par A. L. J. BAYLE. (Tome Seconde de 'Bibliothèque de Thérapeutique.')—Paris, 1830.
3. *Homœopathy: its Tenets and Tendencies.* By Professor SIMPSON.—Edinburgh, 1853.
4. *Homœopathy fairly represented.* By Professor HENDERSON.—Edinburgh, 1853.

SINCE the immortal discovery of Jenner, whereby one of the most frightful and most fatal diseases from which the human race has ever suffered was deprived alike of its terrors and its victims, the cultivators of medicine have been justly animated by the hope that their science might be caused to yield other services of a kindred nature to mankind.* Nor has there been any want of real and earnest activity in a work which, since the introduction of vaccination, all must have had more or less at heart. For whether or not we concede to belladonna the prophylactic virtues in scarlatina which not a few have claimed for it, we are at all events called upon to acknowledge, that from very many the subject has, at various times during the five-and-fifty years it has been under discussion, received all that attention and patient investigation which every right-thinking man will readily and heartily admit to be its due.† That a disbelief in

* "I believe," says Dr. Simpson, "medicine will yet most probably discover prophylactic measures against scarlet-fever, measles, &c."—*Homœopathy*, p. 230, note at foot of page.

† We may remind our readers that very many other prophylactics have been recommended and actually employed in scarlatina besides the exhibition of belladonna. In regard to such we find Joseph Frank writing, "Ad scarlatinam præpediendam commendata fuere: errhina et collutoria ex ammonia cum sufficiente quantitate aquæ; acida mineralia diluta tum interne, tum externe sub forma gargarismatis: subfumigia vel ope acidi muriatici sive simplicis sive oxygenati, vel ope acidi nitrici; minimæ doses succi inspissati herbæ atropæ belladonnæ; et ipsa

been attributed to Castelliz, of Vienna,* there is no doubt that the idea originally occurred to the mind of Hahnemann. When the subject was first introduced to him it happened when he was residing in Vienna; and thereafter he published a treatise on the Scharlachfieber,† from which the present edition of the lesser works of Hahnemann is derived, in the manner in which his

daughter, at the commencement of July, 1799,‡ when she was only ten years of age, had got a new counterpane made (she was ignorant of the former) had in her small room caught scarlet fever. The first-mentioned woman on seeing her daughter thus affected, in order to ascertain whether it might not be necessary to hang it in the open air; but as she was of the sort, she laid it beside her on the pillow of the bed. After she lay down for her afternoon's nap. She had no other visitors (for the family had no other near or remote connections), imbibed this miasm. A week subsequently she was attacked with quinsy, with the characteristic shooting pains in the throat, which could only be subdued after four days of threatening symptoms. After her daughter, ten years of age, infected most probably by inhalations of the mother, or by the emanations from the counterpane, was attacked in the evening by severe pressive pain in the abdomen, with rigour on the body and head, and rigour over the head and arms, and with stiffness of the joints. She slept very restlessly during the night, with delirious dreams, and perspiration all over the body excepting the head. I found her in the morning with pressive headache, dimness of vision, slimy tongue, some swelling of the submaxillary glands hard, swollen, painful to the touch, shooting pains in the throat on swallowing and at other times. She had not the slightest thirst, her pulse was quick and small, breathing hurried and anxious; though she was very pale she felt hot to the touch, yet complained of horripilation over the face and hairy scalp; she sat leaning somewhat forwards, in order to avoid the shooting in the abdomen, which she felt most acutely when stretching or bending back the body; she complained of a paralytic stiffness of the limbs with an air of the most dejected pusillanimity, and shunned all conversation. 'She felt,' she said, 'as if she could only speak in a whisper.' Her look was dull and yet staring, the eyelids inordinately wide open, the face pale, features sunk.

"Now I knew only too well that the ordinary favourite remedies, as in many other cases, so also in scarlatina, in the most favourable cases leave everything unchanged; and, therefore, I resolved in this case of scarlet fever just in the act of breaking out, not to act as usual in reference to individual symptoms, but if possible (in accordance with my new synthetical principle) to obtain a remedy whose peculiar mode of action was calculated to produce in the healthy body most of the morbid symptoms which I observed *combined* in this disease. My memory and my written collection of the peculiar effects of some medicines, furnished me with no remedy so capable of producing a counterpart of the symptoms here present as *belladonna*.

"It alone could fulfil most of the indications of this disease, seeing that in its primary action it has, according to my observations, a tendency to excite even in

* See Lectures on Materia Medica and Therapeutics, by G. G. Sigmond, M.D., Lecture xiii. *Lancet*, vol. ii. 1836, 37.

† Originally published at Gotha, in 1801.

‡ The 14th of May, 1796, was, as Dr. Watson happily terms it, "the birthday of vaccination;" It is not unlikely that during the period from May, 1796, to July, 1799, the mind of Hahnemann had been strongly directed to the subject of the prevention of contagious diseases.

healthy persons great dejected pusillanimity, dull staring (stupid) look, with inordinately opened eyelids, obscuration of vision, coldness and paleness of the face, want of thirst, excessively small rapid pulse, paralytic immobility of the limbs, obstructed swallowing, with shooting pains in the parotid gland, pressive headache, constrictive pains in the abdomen, which become intolerable in any other posture of the body besides bending forwards, rigour and heat of certain parts to the exclusion of others—e. g., of the head alone, of the arms alone, &c. If, thought I, this was a case of approaching scarlet fever, as I considered was most probable, the subsequent effects peculiar to this plant,—its power to produce synochus, with erysipelatous spots on the skin, sopor, swollen hot face, &c.,—could not fail to be extremely appropriate to the symptoms of fully developed scarlatina.

“I, therefore, gave this girl, ten years of age, who was already affected by the first symptoms of scarlet fever, a dose of this medicine ($\frac{1}{432000}$ th part of a grain of the extract, (which, according to my subsequent experience, is rather too large a dose.) She remained quietly seated all day, without lying down; the heat of her body became but little observable; she drank but little; none of her other symptoms increased that day, and no new ones occurred. She slept pretty quietly during the night, and the following morning, twenty hours after taking the medicine, most of the symptoms had disappeared without any crisis; the sore throat alone persisted, but with diminished severity, until evening, when it too went off. The following day she was lively, eat and played again, and complained of nothing. I now gave her another dose, and she remained perfectly well, whilst two other children of the family fell ill of bad scarlet fever without my knowledge, whom I could only treat according to my general plan detailed above. I gave my convalescent a smaller dose of belladonna every three or four days, and she remained in perfect health. I now earnestly desired to be able, if possible, to preserve the other five children of the family perfectly free from infection. Their removal was impossible, and would have been too late. I reasoned thus: a remedy that is capable of quickly checking a disease in its onset, must be its best preventive; and the following occurrence strengthened me in the correctness of this conclusion. Some weeks previously, three children of another family lay ill of a very bad scarlet fever; the eldest daughter alone, who, up to that period, had been taking belladonna internally for an external affection on the joints of her fingers, to my great astonishment, did not catch the fever, although during the prevalence of other epidemics she had always been the first to take them.

“This circumstance completely confirmed my idea. I now hesitated not to administer to the other five children of this numerous family this divine remedy as a preservative, in very small doses, and as the peculiar action of this plant does not last above three days, I repeated the dose every seventy-two hours, and they all remained perfectly well, without the slightest symptoms throughout the whole course of the epidemic, and amid the most virulent scarlatina emanations from their sisters who lay ill with the disease. In the meantime I was called to attend another family, where the eldest son was ill of scarlet fever. I found him in the height of the fever, and with the eruption on the chest and arms. He was seriously ill, and the time was consequently past to give him the specific prophylactic treatment. But I wished to keep the other three children free from this malignant disease. One of them was nine months, another two years, and the third four years of age. The parents did what I ordered, gave each of the children the requisite quantity of belladonna every three days, and had the happiness to preserve these three children free from the pestilential disease, free from all its symptoms, although they had unrestricted intercourse with their sick brother. And a number of other opportunities presented themselves to me where this specific remedy never failed.” (p. 434.)

Such is Hahnemann's account of the mode in which the efficacy of belladonna was first suggested to his own mind. We shall anon revert to the passage we have quoted at such length; meantime, let it be

observed, that over and above the prophylactic virtue in scarlatina which Hahnemann claims for belladonna, he also asserts its potency as a specific remedy in the disease itself, modifying its symptoms, removing its "after sufferings," or consequences, "often worse than the disease itself;" and capable, too, of suppressing the fever "in its first germs," when its invasion has already occurred. Further, that, *so far as the prophylaxis of belladonna* is concerned, Hahnemann makes no restriction of the cases of true scarlatina in which the drug may either be inadmissible, or may, in his own experience, have proved useless.* On the contrary, we are led to suppose that, in his experience, no such cases occurred. And this view of his own opinion is rendered more than probably correct, when we find him speaking in his greater work thus:†—"Et qu'en prenant une dose de belladone aussi faible que possible, on se garantit de la fièvre scarlatine."

One of the earliest notices, if not the first mention of the alleged virtues of belladonna, which appeared after the publication of Hahnemann's own pamphlet, and corroborative of his views, was in Hufeland's Journal, for May, 1812, from the pen of Dr. Schenck, having reference to an epidemic which occurred in the department of Hilchenbach, in the grand duchy of Berg. It and the other testimonies which follow, both in favour of and against belladonna, are detailed, for the most part, with much precision in the learned work of Bayle.‡

In 1812, when Schenck witnessed the effects of belladonna at Hilchenbach, the epidemic had, before his arrival, lasted for three weeks. Eight persons had already died, two of whom were previously healthy and robust young men, and two young women in like condition. Twenty-two were then affected; almost all were children, or young persons below the age of twenty. Of 525 persons who used the belladonna, 522 were unattacked by the disease. The three persons who suffered were a mother and her two children, who were, it is said, peculiarly exposed to the contagion, and had only taken the drug four times. The manner of making and administering the preparation of belladonna adopted by Schenck, to whom it was suggested by Hahnemann himself, is thus recorded, and as this is important, we shall quote the French of Bayle:

"M. Hahnemann eut la bonté de me faire parvenir trois grains d'extrait de belladone qu'il avait préparés lui-même, attendu qu'on le confectionne pas dans toutes les pharmacies avec assez de soin pour qu'on puisse compter sur son effets. Il m'envoya en même temps l'instruction suivante: On triture ces trois grains dans un petit mortier, avec une once d'eau distillée qu'on y ajoute peu-à-peu, de manière à ce qu'ils soient exactement dissous. On ajoute à cette solution un autre composé d'une once d'eau distillée et d'une once d'alcool purifiée; on agit le tout, et on laisse déposer. On met une seule goutte de cette liqueur bien claire, dans une bouteille contenant trois onces d'eau distillée et une once d'alcool

* Indeed the *only* restriction made mention of is "in some particular cases, where the original disease has been very violent, and advice has been sought for the *after sufferings* too late, . . . that belladonna is no longer of service;" but in this restriction we recognise a very great amount of speciousness; what is it but to say that whenever and wherever the disease baffles the belladonna, it is not to be laid to its charge, but to the mistake of a too-late advice or consultation.

† Organon: Nouvelle Traduction, par Jourdan, p. 85.

‡ The title of Schenck's paper, as published in Hufeland's Journal, is "Versuche mit dem Hahnemann'schen Präservatif gegen das Scharlachfieber, von Hrn. Hofrath Schenck."—It is

rectifié : on agite bien le tout. C'est cette liqueur qui sert de préservatif. On en donne aux enfans au-dessous de neuf ans une seule goutte, et aux personnes au-dessus, deux gouttes sur du sucre, tous les quatre jours, de manière à ce qu'on reste deux jours pleins sans en donner. . . . M. Hahnemann me conseilla en même temps de recommander qu'on préservât les enfans de toute commotion vive, ainsi que de lésions externes : mais de ne rien changer d'ailleurs à leur genre de vie. . . . Le 7 février l'on commença l'usage des gouttes, et on les continua pendant quatre semaines." (p. 391.)

In this experience of Schenck, let it be noticed that three individuals who had taken the belladonna four times were attacked, and let the possibility of the epidemic having approached its termination before his observation of it began, not be lost sight of. To M. Schenck, M. Rhodius writes as follows :

"Altenkirchen, ce 15 Juillet, 1809.

"L'application de la belladone, comme préservatif de la fièvre scarlatine, a eu ici un grand succès. Lorsque je reçus ce moyen, cette dernière régnait déjà fréquemment dans la ville. Les trois enfans de M. l'architecte de Trott étaient dangereusement malades dans la maison de M. le gouverneur de Poelnitz, dont les deux enfans habitaient l'étage au-dessous. On donna aussitôt le préservatif à ceux-ci, et ils ne furent pas atteints. L'enfant de M. Furchel, qui demeurait dans le voisinage, fut préservé par le même moyen. La bonne d'enfant de M. Hertel était très-dangereusement malade ; on donna le préservatif aux deux enfans, et ils n'eurent pas la maladie. Une de mes trois domestiques avait également la fièvre scarlatine : les deux autres, quoique habitant la même chambre que la malade, furent garanties de la contagion par le préservatif. Je pourrais ajouter plusieurs autres faits à ceux que je viens de rapporter : mais je regarde cette énumération comme superflue, et crois en dire assez, en affirmant que tous ceux qui ont fait usage du préservatif ont échappé à la contagion.

"Signé,

RHODIUS."

MM. Himly* and Hufeland each add a note to M. Schenck's communication : both speak favourably of belladonna as a prophylactic ; the former confirms Schenck's observations, but adds no new ones.

The experience of the observers just named, whether contained in Schenck's original paper in Hufeland's Journal,† or as quoted by Bayle, appears to us as scarcely warranting the language which the latter employs in regard to it, and which Dr. Black‡ transcribes. Bayle, let it be observed, gives, in the first place, numerous details of individual experience, and then, as is usual with him, adds a condensed view of the evidence in the form of a report. Now, to say the least, the deductions made by Bayle do, in some instances, scarcely tally with the evidence in detail. We have found it the best way to compare the two, and when possible, to refer to the original paper from which the French physician quotes. The experience of Hufeland and Rhodius is thus given in the report of Bayle,—“gave perfect immunity to all the individuals to whom

from no desire to find fault, but, on the contrary, with great reluctance, that we must, at the outset of our references to Bayle, express our extreme astonishment and disapprobation of the course Dr. Henderson has adopted. He writes at p. 112 of his work—“Before adverting to the experiments made in Edinburgh, I shall adduce from an article by M. Bayle,” &c &c. Now, it is quite clear that Dr. Henderson has never had recourse to Bayle, but only to Dr. Black's very inaccurate representation of what Bayle has written ; for he even copies Black so literally as to transcribe his errors—one of which, miserable as it is, we must beg Dr. Henderson to correct. He follows Dr. Black in referring to the *Bibliothèque Thérapeutique*, tom. ii. p. 583, *et seq.*, being unaware that there are only 532 pages in the volume.

* Himly, who was professor of medicine at Göttingen, was joint editor, with Hufeland, from 1809 to 1814, of the celebrated German journal which bears the name of the latter.

† Mai, 1812.

‡ Principles and Practice of Homœopathy, p. 36.

they had administered this substance in several very violent epidemics.”* We leave our readers to judge whether or not the statement of Rhodius, in the letter already quoted, authorizes the employment of such terms as “gave perfect immunity,” and “several very violent epidemics.”

M. Masius, Professor of Medicine at Rostock, furnished a paper to Hufeland's Journal in 1813. His belief in the efficacy of belladonna is founded on his own immunity from scarlet fever, when occupied during two years at Schwerin, along with M. Sachse, in treating cases of a malignant type. He took half a grain of the extract every day on which he visited scarlet fever patients, in four doses,—“Et je fus préservé.” At another time when, during winter, scarlatina was prevalent at Rostock, both Masius and his children were preserved by attending to the same precautions. We are rather amused at the manner in which M. Masius is prepared to meet any objections which may be offered to his very paltry evidence. “J'aime beaucoup,” he says, “un scepticisme raisonnable, mais je déteste l'aveugle incrédulité de notre siècle.” We shall have more to say by-and-by of the “hazard” to which M. Masius is aware that some at least may be inclined to ascribe his preservation, and this, evidently, because the narrative favours the author's own purpose.

Gumpert, a physician at Posen, commences a contribution quoted in Hufeland's journal for July, 1818, in very much the same way as some medical men have written during the last few years: he did not, and they have not, given belladonna before, because they wanted “faith,” or “confidence,” in the discovery of Hahnemann. Gumpert, who was happy in the possession of four children, of the respective ages of thirteen, eleven, seven, and two years, administered belladonna to each during a period of three months, when scarlet fever prevailed as an epidemic in Posen. At one period the disease existed in the same building as his family lived in, on the floor below his own house, and when in every house in the same street there were persons affected with the disease. The elder children attended a public school. The younger and elder children were alike preserved. Gumpert, at the same period, employed belladonna in upwards of twenty families which he attended, and always with success. The preservation of his patients, even in the hands of this most sanguine doctor, was not, however, universal. One person took the disease during the first week of prophylactic treatment, and another, a child, after taking the belladonna for two weeks. We are left to conclude that these were the only two who contracted the disease after taking the belladonna; but we are directly informed that Gumpert never had a case of scarlatina in which the specific had been employed for more than two weeks. We are, moreover, told, that in one family, consisting of six, to which the second exceptional case belonged, one took the disease, and two a few days thereafter became affected with sore throats and slight fever, without having eruption or desquamation.

In his synopsis of Gumpert's report, just as in that of Himly already referred to, Bayle does not adhere to the strict letter of the observer. This is perhaps pardonable in Bayle, because within the four corners of his book the statement of Gumpert is given *in extenso*; but what are we to say of Dr. Black, who has evidently never read the statement of

Gumpert, either in Hufeland's Journal, or *in extenso*, as given in Bayle;* or if he has read either, has contrived to ignore both.†

Gumpert père appears to have been the only one in the same district as his son who employed the belladonna. The latter records his father's success, during some years and in several epidemics, as well as the fact of the confidence of the inhabitants of the district in which he resides being so firm in the belladonna, that the druggists dispensed it without the form of a medical prescription: "et qu'il y a la même confiance qu'en la vaccine."

Gumpert père further mentions, that in no case in which the belladonna has been administered, at the proper time and in the approved manner, has scarlatina declared itself; and that those few cases of the disease which have occurred owing to the belladonna not having been administered during a sufficiently long period, have invariably been of a very mild type. This is no doubt the evidence of Gumpert père; but we profess ourselves entirely at a loss to discover how Bayle, from it, is able to assert that Gumpert, by the timely and judicious use of belladonna, prevented the introduction of scarlatina "*into several villages.*" In this statement Dr. Black of course follows. After this our readers will scarcely require our advice as to the necessity of reference to the original quarter for information regarding the experience of German physicians.

M. Berndt‡ observed an epidemic which occurred at Cüstrin in 1817, 1818, and 1819. The following are the results of his observations:

1. Of 195 children daily exposed to contagion, and to whom I administered the belladonna, there were only 14 who, notwithstanding the remedy, contracted the disease, whilst the other 181 were preserved.

2. The same experiments, made with a solution of three grains of the extract of belladonna, upon a large number of individuals, equally exposed to the influence of contagion, resulted in the preservation of the whole number.

3. The 14 who did suffer had the disease less severely than those who had not been similarly subjected to the influence of belladonna.§

Muhrbeck,|| Dusterberg, Behr, and Meglin are all cited by Bayle as confirming in their own experience the peculiar virtue of belladonna. He quotes on this occasion from Martini's paper in the 'Revue Médicale' for 1824. Muhrbeck speaks in the highest terms of its efficacy, having employed it for about seven years, and always with success. In regard to its action he makes the following remark—that vaccination and belladonna differ in the preservation effected by the former being lasting, that of the latter temporary merely. The experience of Dusterberg is important; we shall, therefore, quote it at length from Bayle.

"Pendant trois épidémies consécutives de scarlatine, j'ai employé la belladone

* Taken from Marc's translation in the Biblioth. Méd., tom. lxxv. p. 114.

† It is not Gumpert who says he preserved eighty individuals, it is Bayle who supposes most gratuitously that each of the twenty families contained four individuals.

‡ Berndt's paper in Hufeland's Journal for 1820 is entitled, "Bestätigende Erfahrungen über die Schutzkraft der Belladonna gegen die Ansteckung des Scharlachfiebers, von Dr. Berndt."

§ We shall shortly have occasion to refer to the strength of the dose of the remedy employed by Berndt and others.

|| The title of Muhrbeck's paper is, "Die Schutzkraft der Belladonna gegen das Scharlachfieber." The same paper, with the author's name changed into Muhskbeck, is rendered into French in the Nouveau Journal de Médecine, tom. xii.

avec un succès tel, que je regarde ce remède prophylactique comme aussi efficace que l'inoculation de la vaccine. En effet lorsqu' en 1820 la fièvre scarlatine menaçait la population de la ville Warbourg, je me décidai à vérifier les expériences connues jusqu' alors sur la vertu prophylactique de la belladone. A cet effet je fis prendre aux enfans confiés à mes soins 10, 15, ou 20 gouttes, suivant l'âge, d'une solution faite avec trois grains d'extrait de belladone et trois gros d'eau de cannelle. Cette solution ainsi administrée deux fois par jour, et durant plus d'une semaine, eut pour effet que tous les enfans ayant fait usage du préservatif furent préservés de la contagion, malgré leur contact intime avec les individus atteints de la fièvre scarlatine. Pour mieux faire ressortir l'effet de la belladone et en écarter celui du hasard, j'ai choisi dans chaque famille un enfant, lequel fut excepté de ce mode de traitement. Or, tous les enfans auxquels l'usage du préservatif était demeuré interdit, furent atteints de la contagion. Plusieurs enfans, à la vérité, n'ayant usé du préservatif que pendant quatre ou cinq jours, furent atteints également de la scarlatine; cependant, presque chez tous, la maladie fut si peu grave, que l'on ne s'aperçut de sa présence que lors de la desquamation." (p. 404.)

The following is the experience of Behr, at Bernbourg, during an epidemic which prevailed in that town in 1820, and which, though at first not of a formidable character, speedily acquired a more fatal aspect. Among forty seven individuals, including children and adults, to whom the belladonna was given, only six were attacked by the disease, *and in nearly all the six* the disease was of a benign character.* After concluding his account of the experience of Behr, M. Bayle refers to that of Méglin,† at Colmar, who found, during an epidemic which continued during the autumn and winter of 1820, and the following spring, and which at times (*assez souvent*) assumed a severe and fatal character, that all those who, before the invasion of the epidemic, had taken the specific, were preserved. M. Meglin administered the root of the belladonna in powder, with a little sugar, according to the following prescription: *R. Pulveris radiciis belladonnæ, gr. ij.; sacchari albi, ʒij. Misce: et divide in 60 partes equales.* From one to five doses to be taken, according to the age of the patient, and to be repeated four times daily.

M. Koehler, physician of Cercle, records the following. A child, one of seven, was attacked with scarlet fever well marked; the other six took a very small dose of belladonna, and were preserved, though remaining in the same apartment as the sick child.

* The paper of Behr is one of the most interesting, if not the most so, of all those published in Hufeland's Journal upon this subject; it contains a table, giving the name, age, date of the commencement of the disease, &c., in 47 cases.—Hufeland's Journal, Stück ii., Aug. 1828.

In Dr. Black's account of this physician's experience, he says, the six alluded to above "were attacked in an almost insensible manner." This is certainly not Behr's own account, as our readers may satisfy themselves, by referring to the paper of Martini in the *Revue Médicale* for 1824. We confess to feeling a very strong dislike to the frequent discrepancies which we find between the different writers' own accounts—which surely are the accurate ones—and those furnished by Dr. Black; and as Dr. Henderson has rested satisfied by always referring to the latter, and has, in a foot-note to page 115 of his own work, recommended the English reader to the same source, we take this other opportunity of directing him from so unworthy a quarter. It may be, and in most instances is, very true, that the important facts in regard to the question at issue, as given by the German writers, are fairly enough rendered in both Dr. Black's and Dr. Henderson's pages; but we have a right to expect more than that; and from those who ask us to believe experiences in which they put faith, we require that these experiences should be by them truthfully and accurately presented to us, otherwise let them furnish their readers with a simple reference to the authorities, to which, it appears to us, Dr. Black has never once turned for himself.

† See *Nouveau Journal de Médecine*, &c., Paris, for November, 1821, under the head *Variétés*, the passage which M. Bayle quotes, and which we have rendered above.

Dr. Beeke, among other experiments in favour of the peculiar virtues possessed by belladonna, mentions that the physician of the district, Wolf, in Silesia, encountered an epidemic of scarlatina in the village of Staedtel; 120 persons were already affected; the specific was administered, and thereafter there occurred 39 mild cases. In two other villages, where 132 individuals made use of the same extract, only 6 were attacked. In 1820, at Siegen, the son of a merchant was attacked with scarlet fever. His aunt, who had paid him a hurried visit, was also seized. She was the mother of three young children; they took the belladonna, and, though they were always beside their mother, they were preserved. Dr. Bénédict employed belladonna with success against the contagion of a malignant fever in the island of Rugen. His paper, a short and interesting one, follows Behr's in 'Hufeland's Journal' for August, 1823: and after it come two notices, one by Dr. Wesener, of Dülmen, in Westphalia, the other by Dr. Zeuch, practising in the Tyrol. The former appears to have thought little of the power of belladonna till he administered it to his own children, and finding them preserved from the contagion of prevailing scarlatina, he changed his views. The latter, in the military hospital for children, had the following experience. Twenty-three children out of 84 became affected; to the remaining 61 belladonna was administered during 20 consecutive days; only 1 of the 61 took scarlatina, although the disease continued to prevail in the neighbourhood of the hospital. Dr. Suttinger reports that before belladonna was administered several persons had died during an epidemic of scarlatina which occurred at Miaskowo, but that after recourse was had to belladonna no other case happened.

Hufeland, the learned editor, commences the November number of his journal for the year 1825 thus:

"Es ist mir grosse Freude, die schützende Kraft der Belladonna gegen das Scharlachfieber durch neue Erfahrungen zu bestätigen. Es sind nun fünf (dreizehn?) Jahre vergangen, dass in diesem Journal die erste Aufforderung zu der Anwendung dieses Schutzmittels erging, und jedes Jahr hat seitdem eine Menge günstige Erfahrungen geliefert. . . . Ich selbst habe das Mittel mehrmals in meiner Praxis angewendet, und nie gesehen, dass eines von denen, welche dasselbe gehörig gebraucht hatten, angesteckt worden wäre."*

Having passed this panegyric on belladonna, the observations made in the Frederick Institution at Berlin, to which he is physician, by Kunzmann (whom Bayle calls Kunstmann, and of course Black does so also,) are detailed. He had remained doubtful as to the efficacy of the remedy, till, in January, 1825, he became, from his experience in the institution already referred to, satisfied as to the protective virtue of belladonna. In it there were about 70 children of both sexes, from 4 to 14 years of age. On the 25th December, 1824, scarlatina manifested itself in the person of the director's son, and three days later two young girls, one of 4, the other of 7 years, became affected. The

* "It is to me a great pleasure to be able to confirm, by new observations, the prophylactic power of belladonna in scarlet fever. It is now five (thirteen?) years since, in this journal, the first mention was made of the employment of this preventive, and each year since that time has brought with it a large number of corroborative facts. . . . In my own practice, I have on several occasions used this remedy, and I have never seen one of those who used it in the proper manner affected by the disease."

sick children were separated, but, adds Kunzmann, it was impossible to cause a complete isolation. The sound children then received a mixture, composed of two grains of the extract of belladonna in an ounce of distilled cinnamon water, of which each child took as many drops twice daily as he or she had years. From that time to the 23rd of January, a period of four weeks, no case presented itself, but on that day a little boy of ten became affected, but only very slightly, proving that the contagion still existed in the house. A second son, however, of the director of the institution, who had not taken the mixture, suffered a severe attack of the disease. During six weeks the remedy was persevered in, and no cases occurred. The table furnished by Gelnecki, of Stettin (with whose name also Bayle, and Black after him, take great liberties, manufacturing it into Geneki), succeeds Kunzmann's report, and is a remarkably interesting one. His experience was obtained in Glasow. There were in all 94 children. Of these 76 appeared to be preserved from the contagion by the use of the belladonna, while 15, who had not employed the remedy, became affected with the disease, 3 who had employed the belladonna took scarlet fever, and 2 of the 3 died. Of the 15 who took the disease without having made use of the prophylactic, 4 died.*

Maizier,† district physician of Burg, made use of belladonna in the village of Nigripp, and not one of the 170 children to whom he administered it became affected with scarlatina. The treatment was continued for 14 days, and then the epidemic disappeared, though in the neighbouring village of Detershagen, where no belladonna had been employed, it continued to prevail, and some children died. This physician had previously obtained similar results with belladonna in 1821: an epidemic of a fatal character prevailed at the village of Grabow, and its cessation followed the use of the prophylactic. In the districts of Riesel and Ziegelsdorf, where some children had been already seized, the belladonna was employed, and no other case occurred. Also in Burg, the place of his own residence, among from 60 to 70 children, there were only 3 or 4 who became affected with scarlet fever (when epidemic) after the use of the specific. Hufeland mentions Dr. Wiedemann, of Wolmirstedt, as bearing like testimony.

Dr. Raudhan,‡ in the Orphan Hospital at Langendorf, on the occurrence of 2 cases of scarlatina, gave the belladonna to the 160 remaining, from February (when the two cases presented themselves), so long as the contagion lasted. On the 21st of April, the disease had attacked none of the other orphans, not even two who shared the same apartment with the two previously sick children. Velsen,§ physician at Clèves, reports, that of 247 persons who used the belladonna, 13 only contracted the disease, of whom 4 were children who had taken the remedy during several weeks,

* The inexcusable blunder which both Bayle and Dr. Black, in copying him, have committed, in quoting the testimony of Gelnecki, is also observable in the table which Bayle has prepared, but which, from the inaccuracy we allude to, is rendered useless. Dr. Black, in a foot-note to page 39 of his book, says, "there is an error here as to the number, also in the tabular list;" but he had not the ingenuity to correct the mistake into which Bayle had fallen, although in Bayle's own work the opportunity for so doing was afforded him.

† Hufeland's Journal, Nov. 1825. For his account of Maizier's experience, Bayle (and it is singularly inaccurate) quotes from a French journal. Journ. des Prog., tome i. p. 242.

‡ Hufeland's Journal. 1825.

§ Journ. Complémentaire du Dict. des Sciences Méd., tome xxviii. p. 370.

but not with regularity, 1 child who had taken it regularly during fourteen days, another during eight days, and the rest during forty-eight hours. In all the cases the disease was mild, milder than with those who had not taken the medicine. Among the facts mentioned by Velsen is the following: A man, the father of four children, who had visited but only for a few seconds a friend labouring under scarlet fever, was seized, some days thereafter, with the same disease, and in a violent manner; his wife and children, the youngest of whom was only three weeks, and the oldest four years, took with great regularity the extract of belladonna, and, although day and night were passed with the sick husband and father, and in a small and badly-ventilated chamber, none took the disease. M. Velsen adds: "Est ce là l'effet du hasard, ou le résultat de l'emploi de la belladone?"*

Such are some—indeed, nearly all—of the testimonies borne by foreign—and more particularly by German physicians—to the prophylactic virtue of belladonna. We now proceed to consider the facts which have been advanced in this country; here we find the evidence neither so extensive nor on so large a scale. The following account is given by Messrs. Taynton and Williams, gentlemen practising at Bromley, in Kent, in 1829:†

"During the months of April and May, the scarlet fever was very prevalent in this town and neighbourhood, and in many cases it proved fatal. Our attention was called by a friend to a notice in the 'Lancet'‡ of the 2nd of May, 'On the prophylactic powers of belladonna against scarlet fever, by M. Hufeland.' We were at that time attending in a boarding-school where the disease had attacked 12 of the boys, many of whom had been most dangerously ill, but none had died. There still remained several boys (perhaps 20) who had not taken the infection; also 4 young children of the master's, and several servants. We immediately commenced the use of the belladonna, in the exact manner and dose advised by Hufeland. Only 6 or 7 persons in the house took the disease afterwards, and in every instance it assumed the mildest form.

"In another school, we were called to visit a child about two years old, who had been attacked the evening before. The disease was of the most malignant character, and the child died on the following morning, the third day from the attack. The house is a very small one. There were in it 3 other young gentlemen and 5 boarders, and a servant-girl. The belladonna was faithfully administered, and not one individual took the disease. We will not offer any conjecture on the *modus operandi* of the belladonna, or whether it did or did not prevent the other members of these families from taking the disease. The facts are stated exactly as they

* Did our space permit, we might have quoted Wagner's report of the epidemic at Schlieben, of Dr. Peter's at Leopoldshagen, of Dr. Reuscher at Stendal, and Dr. Cohen. For these we beg to refer the reader to Hufeland's Journal, 1825, also to the Gazette de Santé for the same year, for the statement of M. Lemer cier. These are all alike favourable to the theory of the prophylactic power of belladonna.

† The London Medical Gazette, vol. iv. p. 297.

‡ The following are the conclusions of M. Hufeland, contained in the paper which Messrs. Taynton and Williams refer to:

I. The proper use of belladonna has, in most cases, prevented infection, even in those instances where, by the continual intercourse with patients labouring under scarlet fever, the predisposition towards it was greatly increased.

II. Numerous observations have shown that, by the general use of belladonna, epidemics of scarlet fever have actually been arrested.

III. In those few instances where the use of belladonna was insufficient to prevent infection, the disease has been invariably slight.

IV. There are exceptions to the above three points, but their number is extremely small.—Lancet, May 2, 1829.

occurred, and we entreat our professional brethren to make trial of the belladonna whenever a favourable opportunity occurs."

The following is the result of Dr. Black's* experience :

"Belladonna was administered to 11 children who never had scarlet fever, and who were living in a house with 2 cases of scarlet fever, the one of them attended with sloughing sore throat, and in intercourse with these cases: all escaped, even one who was sleeping in the same bed with one of the patients. In another instance, we gave belladonna to 4 children, none of whom had the fever, and were directly exposed to the contagion; 3 escaped; 1 took the fever, but so slightly, that we were inclined to regard the symptoms as those of belladonna. In another instance, we administered the remedy to 4 children and an adult, who were living in the same house with 2 cases of scarlet fever. The adult and 2 children were seized with the fever; 2 had only taken the remedy for two days, and 1 for three days; the other 2 children escaped. The 3 cases were much milder than the 2 cases in which no belladonna had been given as a preservative. Out of the 20 cases, we observed the remedy produce headache, with increase of pulse, in 1 child; in another, there was slight redness of the skin, which lasted for eight hours, and unattended with fever."

Dr. Patrick Newbigging† writes as follows:

"Scarlet fever having prevailed in John Watson's Institution to so considerable an extent, and the cases having occurred in close succession, notwithstanding a system of separation as complete as was possible amongst inmates residing under the same roof, I felt desirous to try the effect of belladonna as a prophylactic against the disease. It was an opportunity such as rarely occurs for the investigation of the alleged virtue of this drug on a large scale. Having ascertained the number of children unaffected with scarlet fever, or who were uncertain as to ever having had it—making, in all, 69—I directed that belladonna should be administered to them, in the proportion of one-sixth to one-fourth of a grain twice a-day, according to the age of each child; the first dose being given before breakfast, and the last dose at bedtime. This plan was adopted on the 16th of October. Three new cases occurred between that and the 20th. After that date no child was affected, nor has there been any instance of scarlet fever since that period in the institution. . . . I should now consider it my duty to lose no time in making use of this medicine on the first appearance of this disease, and I would strongly recommend the same plan of practice to those of the profession who are connected with similar educational institutions, with the view, not merely of attempting to ward off a malady so uncertain in its progress, and occasionally so fatal in its termination, but also with the object of accumulating information on a point of such paramount importance to the public health. The opinion I have adopted on this point has been greatly strengthened by a similarly beneficial result produced some time afterwards in another case. I was requested to visit a young gentleman at a large educational seminary. I found him labouring under scarlet fever, with profuse eruption, an aphthous and very painful condition of the throat, accompanied by all the usual symptoms exhibited in the acute stage of a smart attack of this disease. I caused my patient to be removed, a few hours after first seeing him, to the house of a relative, and placed his brother, who continued to reside in the seminary, upon belladonna. This treatment was adopted on the other members of the family, consisting of 19, who had not previously been affected with scarlet fever. No other case occurred."

We might easily multiply the quotation of experiences such as the three now adduced. We believe such a procedure, however, to be unnecessary, the facts in favour of the employment of belladonna being as

* *British Journal of Homœopathy*, vol. i.

† *Monthly Journal of Medical Science*, Sept. 1849.

strongly elicited in these three as in any other recent accounts we have met with.

Our readers, after having followed us in the production of these various facts and opinions of authors in favour of the prophylactic action of belladonna, will naturally expect us to advance the facts and opinions of a contrary bearing. And if we now limit ourselves to the quotation of a few of the former, and to a mere glance at the general nature of the latter, it must not be supposed either that the facts are wanting or are even limited in number, or that silence has prevailed over the expression of opposite views. Such is certainly not the case; there exist, if not so many facts as in favour of the prophylactic action of belladonna, at least stronger, and altogether more reliable ones, on the inefficiency of its employment; while the expression of opinion in regard to its inefficiency—not always formed on the justest grounds, we allow—have undoubtedly been neither few nor uncertainly declared. Among German writers who have adopted this view is Lehmann, the staff physician of the garrison at Torgau. Dr. Black makes it appear as if Bayle objected to the evidence of Lehmann, on the score of its being “supported by no facts.” Such is not the case; Bayle never could have made such a mistake with the paper of Lehmann before him, and when he writes,—“*Nous ne pouvons apprécier à leur juste valeur l'opinion de ces auteurs, parce qu'elle n'est appuyée d'aucun fait, et que la maladie n'est point décrite,*” Bayle means this to apply to the opinions advanced by Raminski* and Teuffel,† as quoted by Barth. Any reader, however, of either Black or Henderson, will come to the conclusion, that by Bayle the evidence of Lehmann was held in the same estimation as that of the two other observers just named,—affording another proof of the danger of trusting to second-hand reading, and of the propriety of consulting in all cases, where possible, the original statements of every author. Had Dr. Black not rested satisfied by quoting the mere *résumé* of Bayle, he would not have fallen into this error; for at page 417 of his same volume, Bayle devotes a paragraph of nearly half a page in length to Lehmann's observations, entitling them, ‘*Observations du Docteur Lehmann:‡ Epidémie de Scarlatine dans laquelle la Belladone ne prévint pas la Maladie.*’ The title of the paper itself, in ‘*Rust's Magazin,*’ is different; it is given below.§ What Bayle says of Lehmann's experience is in every respect fair, and when he expresses his opinion in the following words, “*Il n'a jamais pu parvenir à empêcher la contagion chez eux qui y étaient disposés, ni à modérer la gravité de la maladie chez eux qui déjà en étaient atteints,*” (p. 417,) he says *no more than* Lehmann's accurately observed and precisely stated facts required.

Now this paper of Lehmann's is both a very interesting and a very important one. His experience was large; his attention to the mode of preparation and the manner of administering the belladonna were alike

* Raminski is mentioned by Barth to have lost his own son, and to have afforded many proofs of the augmentation of the disease after the employment of belladonna.

† Teuffel's observations, says Bayle, are to the like effect.

‡ *Magazin für die gesammte Heilkunde, Herausgegeben Von Dr. Johann. Nep. Rust,* vol. xxi. 1826 (at p. 42).

§ *Die Unwirksamkeit der Belladonna als Schutzmittel gegen das Scharlachfieber, nebst einem Impfungsversuche dieser Krankheit. Von Dr. Lehmann.*

exact—(“en le donnant,” Bayle himself says, “suivant toutes les règles indiquées par ceux qui ont préconisé ce moyen,”)—and lastly, his memoir has the advantage of almost all others which we have perused, while it is inferior to none in exhibiting the precision of its author’s observations. We shall quote four of these:—

1. In a family consisting of three boys, the eldest was attacked with scarlet fever. The two others were immediately removed from the sick boy, and were confined to the floor of the house below that on which his room was. They got, at the same time, every morning and evening, the belladonna solution. After this boy’s recovery, and at the end of one month from the first appearance of the disease, he was restored to the society of his two brothers. Four months later the youngest brother was seized with the disease in a severe form; he recovered, and then the third (in respect of years, the second) brother, who remained on this occasion in proximity to the patient, but at the same time took the belladonna regularly, contracted the disease on the tenth day, and fell a victim to it.

2. In a family consisting of five brothers and sisters, a boy of five years was first attacked with scarlet fever. To the other four the belladonna was immediately given. After eight days a little girl of four years old was seized, and on the third day of the disease died. The following day a sister of three years of age took the fever mildly, and recovered; another sister, of eleven years, was almost immediately afterwards affected, and on the fourth day of her illness died. The eldest brother, long a sufferer from bad health, and particularly from a chronic affection of the heart, remained free from the disease. It is of importance to know that the four patients together occupied a small and extremely damp room, on the ground floor; and this, indeed, was accepted as the probable cause of the early deaths.

3. A boy of five years, an only son, contracted scarlet fever after having uninterruptedly, during several months, taken belladonna. The fever assumed a cerebral character, and on the fourth day the little patient died.

4. In a family of four children, the eldest (who was five years) became affected with scarlet fever. The remaining three were immediately put on the belladonna; two of these, on the twenty-first day of the employment of the drug, became affected with the disease in a severer form than the first child, who had taken no belladonna.

Along with other facts of a like nature, Lehmann mentions that, in his own experience, whole families (one in particular, consisting of seven children,) altogether escaped the disease, though epidemic in the place where they resided.

At Stralsund, writes Barth (quoted by Bayle, p. 419), Dr. Mierendorf observed that the children to whom belladonna was administered became more seriously affected, and died in much greater proportion than those for whom the drug was not prescribed. Dr. Schmidt, writes the same authority, lost two children who had taken the so-called prophylactic. Of 100 children so treated, fifteen became affected with scarlet fever, and one died.

Dr. Ramiński, who lost his own son, had so many proofs of the exacer-

bation of the disease during the employment of the belladonna, as to make him altogether doubt its efficacy as a remedy.

Mr. Benjamin Bell, in the course of an article on "Scarlet Fever as it appeared in George Watson's Hospital in the Spring of 1851,"* writes as follows:

"Conceiving that no means for arresting the disease ought to be neglected, and that a favourable opportunity now offered itself for testing the alleged prophylactic virtue of belladonna, I determined to give it a full and fair trial.

"Accordingly, on the 21st of February, upon the appearance of a second case of scarlet fever, the fifth part of a grain of the extract was given, morning and evening, to each of the boys. The dose was found, in a few days, to be too large, from the dilated state of the pupil and impaired vision which it occasioned in several instances. It was accordingly diminished, and then administered without interruption, to all the boys, who continued well until the 7th day of June, a full month after the last case of scarlet fever had occurred. It is important to remark, that the second case already referred to had been in the sick-room, separated from the rest of the boys, for more than a week before the symptoms of scarlet fever appeared, and that no additional case occurred until the 21st of March, an entire month after the belladonna had been regularly administered. There was thus ample time for the manifestation of its virtue as a prophylactic; but the subsequent occurrence of so many cases seems to throw considerable dubiety over the existence of any such power. No experience of a merely negative character can be regarded as of much weight, when contrasted with this positive experience now detailed. It is by no means unusual to meet with only two or three cases of scarlet fever in a large assemblage of children, without the belladonna having been used at all; and therefore we are not called upon to give it the credit of securing a similar exemption in cases where it has been administered; but surely the occurrence of 23 cases out of 54 boys, who might be legitimately reckoned liable to the disease, is an overwhelming evidence on the opposite side."

In reference to the prophylactic action of belladonna, we find Dr. Elb, a homœopathic practitioner at Dresden, writing as follows:†

"I must add, that in general I did not find the prophylactic power of belladonna by any means so generally borne out; although cases have come before me, in which I gave belladonna as a preventative, and the children to whom I administered it remained free from scarlet fever. But just as often have I found that children have been attacked by it, notwithstanding the use of belladonna for several weeks, and that this long previous use of the belladonna had not even the power of diminishing the violence of the disease."

The interesting experiments of Dr. Balfour, conducted at the Royal Military Asylum at Chelsea, are thus alluded to by Dr. West:‡

"I cannot do better than relate the experiment in the words in which Dr. Balfour was good enough to communicate it to me. Scarlet fever having broken out in the visitation, Dr. Balfour determined to try the virtues of belladonna. 'There were,' he says, '151 boys, of whom I had tolerably satisfactory evidence that they had not had scarlatina. I divided them into two sections, taking them alternately from the list, to prevent the imputation of selection. To the first section (76) I gave belladonna; to the second (75) I gave none; the result was, that *two* in each section were attacked by the disease. The numbers are too small to justify deductions as to the prophylactic power of belladonna; but the observation is good, because it shows how apt we are to be misled by imperfect observation. Had I given the remedy to all the boys, I should probably have attributed to it

* Monthly Journal, Aug. 1851.

† See British Journal of Homœopathy. 1849. Vol. vii. p. 33.

‡ Lectures on the Diseases of Infancy and Childhood. Third edition, 1854.

the cessation of the epidemic.' To these remarks," continues Dr. West, "I need add nothing. They convey a most important lesson, but one which, I fear, we are all too apt to forget in the study and in the practice of medicine." (p. 600.)

Dr. Andrew Wood's experience in Heriot's Hospital is thus mentioned by Dr. Simpson:

"In Heriot's Hospital my friend, Dr. Andrew Wood, placed half of the boys in each ward or sleeping division on belladonna, and left the other half without any such protection. The disease did not spread much; but at least as many of those using the belladonna as of those not using it were attacked; and the only fatal case out of 40 which occurred during that epidemic, was that of a boy who had been using belladonna in doses of one-eighth of a grain twice a-day for three weeks previously to his being attacked."*

In drawing this article to a close, we have to consider, first, whether or not the prophylactic action of belladonna against scarlatina, as claimed, promulgated, and practised by Hahnemann and his followers, has stood the test of experience, and is now to be regarded as a reality; and second, whether, in regard to the employment of belladonna for a like purpose, in larger doses than those recommended by the former, experience and observation lead us to embrace the practice as a real boon to humanity, or to abandon it as inefficient and absurd.

At the outset, let us exactly understand what Hahnemann did, and his followers do now, claim for belladonna as a prophylactic: and what was the manner of its exhibition which he advised and required. This inquiry is not unnecessary, when we consider that in the numerous instances of failure of the belladonna reported to Hahnemann himself, he invariably attributed the want of success to the prophylactic having been employed in cases of a fever different from scarlet fever;† or the *Miliare pourprée*, which was, according to Barth, imported from Holland, in the

* Through the kindness of Dr. Wood and of Dr. Simpson, we are enabled to give the experience of the former a little more in detail. The plan Dr. Wood adopted was an excellent one, and the result of his experiments, taken in connexion with Dr. Balfour's and Mr. Bell's (somewhat differently performed), to our mind appears conclusive: "The plan that I proposed to myself," says Dr. Wood, "was this—viz.: whenever scarlatina appeared in any particular ward, and not till then, I immediately made inquiry, and having ascertained the boys who had previously had the fever, these I left out of the question. I then divided the remainder into two nearly equal sections: to one I gave one-eighth of a grain of belladonna, twice a day; to the other, no belladonna was given. This experiment was continued for several weeks, and the reason why it was then discontinued was simply this—that a fatal case occurred in the person of a boy (J. B.) who had been taking the belladonna for nearly four weeks. Taking alarm, I resolved to discontinue the experiment." The following is a brief analysis of the trial:

First ward—containing 11 boys. Case occurred April 17th: 8 already had scarlatina; 5 boys got belladonna; 2 got no belladonna. One of the 5 took scarlatina June 2, and died on the 7th. No other case.

Eighth ward—containing 20 boys. Case occurred April 25th: 7 already had scarlatina; 5 got belladonna; 3 got no belladonna. No subsequent case.

Fourth ward—containing 25 boys. Case occurred May 9th: 4 already had scarlatina; belladonna given to 10; no belladonna to 10. On 19th May, J. G., who had accidentally slept in the same room as a boy who had scarlet fever, and had been taking the belladonna since the 28th April, became affected with the disease in a moderately severe form: he recovered. On 4th June, a boy, who had taken no belladonna, contracted the disease in a very mild form. No subsequent case.

Fifth ward—containing 18 boys. Case occurred May 23rd: had had the disease, 4; took belladonna, 6; took no belladonna, 7. No subsequent case of fever.

Seventh ward—containing 36 boys. Case occurred May 28th: had had scarlet fever, 6; took belladonna, 18; took no belladonna, 11. No subsequent case.

† Hahnemann also speaks of the introduction of this *Fièvre Miliare Pourprée*—(or, in German, *Rothe Friesel*, *Purpurfriesel*, *Roodvonk*)—purple rash—as having been introduced from Belgium in 1801. See *Reine Arzneimittellehre*. Von S. Hahnemann. Vol. i. p. 15.

month of January, 1801. It appears to us that in all probability the limitation of the use of the drug to the cases of scarlet fever, as described by Sydenham* and Plenciz,† was an after thought, and that, notwithstanding the allusion made by Hahnemann to the similarity borne by the epidemic of scarlatina at Königsbutter to the disease described by Plenciz; for, unquestionably, in the whole length of the article 'On Scarlatina,' as translated by Dr. Dudgeon, there does not occur a single expression whereby we are to understand that his proposal of belladonna as a prophylactic, or as a remedy strictly so called, was to be confined to cases of the same nature as those which occurred in that epidemic.‡ Were any further proof of this (than the absence of any restriction) required, we think it supplied in the fact of Hahnemann, in his first publication, expressing his belief "that a similar employment of belladonna would also preserve from measles."§ Now, no one will pretend to urge that a closer resemblance subsists between measles and scarlatina, than between that disease and the *miliare pourprée*, which, if it were really not a variety of scarlatina, must have very closely approached it in character, before so many observant physicians could have been deceived. In our opinion, then, the plea of want of success on the ground of dissimilar diseases being treated under the belief that they were alike examples of scarlatina epidemics, does not hold good; for, first, there is no proof of the disease treated by Raminski, Teuffel, and other physicians, not having been a true scarlatina; and second, Hahnemann himself did not confine the virtues of belladonna to scarlatina, but extended them to a disease whose characters are by a still longer way removed from it than the *miliare pourprée*—namely, measles.

It has been contended, and this argument is referred to by Professor Henderson, that in instances of failure, another drug than belladonna, dulcamara for example, may have been used. Now, we beg to submit that if this idea is allowed to have any weight, it must be permitted, in

* Processus Integri. (Sydenham Edition of Works.) Vol ii. p. 242.

† Tractatus de Scarlatina. Autore Marco Antonio Plenciz. Sectio ii. Vienna, 1772.

‡ Dr. Henderson argues that Hahnemann has the advantage of Jenner, in not claiming universality of exemption from scarlatina after the use of belladonna, as he says Jenner did after vaccination, from small pox. To us it appears, that if Hahnemann had adhered to his original opinion—from which he at the time allowed no exception—he would have been both more honest and more entitled to our attention. We can, however, see no parallelism either between the discoveries of Jenner and Hahnemann, or between their subsequent histories.

§ Hahnemann, in adverting to the subject of the treatment of scarlet fever as recommended in the works of various authors, makes this singular admission: "Here we often see the *ne plus ultra* of the grossest empiricism: for each single symptom a particular remedy in the motley, mixed, and repeated prescriptions; a sight that cannot fail to inspire the unprejudiced observer with feelings at once of pity and indignation." We think "the cap fits" most exactly here, and even pinches, though Hahnemann, with strange perversion of observation, does not appear to feel it. The proposal and employment of belladonna in scarlet fever is as apt an illustration as could be imagined of the fitting remedy to symptom. Belladonna produces a scarlet rash; therefore, concludes Hahnemann, it will cure scarlatina, or is homœopathic to it. But *scarlet rash* is not scarlet fever, it is only a symptom of it; and if we were to give belladonna as often and as long, and in whatever doses we chose, we might kill our patients, but we never could contrive to give them scarlet fever. The question of the power of belladonna to produce the rash which is so universally believed to follow its continual administration, itself requires revision. For our own part, we can say that, after giving belladonna for a long time, in more cases than one, we have failed, though careful and repeated in our examination, ever to discern it. We do not mean to doubt the production of what may be called *spots* in some cases, but we altogether disbelieve the fact of even these following in any large number of instances. Schultz has justly remarked, that similarity of symptoms, not of diseases, lies at the base of all the therapeutic proceedings of Hahnemann and his followers—a pity they do not see it.

all justice, to affect both sides of the question; for we know no reason, and no experience, to justify such reason,—why Hahnemann and his followers should have always hit upon the proper plant, and those who opposed his views have seldom or never done so. If Dr. Henderson insists upon this point, we are quite ready to allow that some physicians, who employed dulcamara, or some other member of the Solanaceæ, instead of belladonna, have failed (though not in consequence) to protect their patients from scarlet fever; but then, we must contend that certain other physicians, who administered these drugs instead of belladonna, have succeeded in the desire to have their patients preserved from the disease though not in consequence. Let it, however, be remembered that belladonna possesses a singular property—a property almost peculiar to itself—and then we think it will be granted, that any physician entering on a careful investigation into its properties, by means of a given portion of its extract, will first establish the power of that individual specimen to dilate the pupil in the peculiarly marked manner which belladonna does.

Some singular discrepancies exist in regard to the frequency of the administration of belladonna, as recommended by Hahnemann: for example, Barth, as cited by Bayle, says that “tous les six à sept jours” was the interval at which he advised the dose to be administered. Jahr* also says, as expressing the views of Hahnemann, “To this effect the smallest dose of belladonna ought to be given every six or seven days.” On the other hand, in his communication to Dr. Schenck, already quoted, Hahnemann says distinctly, on every fourth day the belladonna solution should be taken; and in his own pamphlet, as translated by Dr. Dudgeon,† he condescends to a greater particularity, and orders a dose to be given every seventy-two hours. It is not for us to reconcile these discrepancies, believing, as we do, that it certainly makes very little difference whether the $\frac{1}{437500}$ th part of a grain of belladonna§ be taken every seventy-two hours or every seven days. Some of his followers, moreover, take what we should have thought to be unpardonable liberties with Hahnemann’s directions; of these we need only cite Dr. Black, who has the effrontery to double the Hahnemannic dose (making it thus $\frac{1}{218750}$ th part of a grain), and to make the interval of its exhibition from ten to fourteen days.|| Surely when Hahnemann’s own followers, acting on their own responsibility, double the strength of his remedial measures, and fix their own time for their administration, Dr. Henderson¶ need not be so very indignant at Mr. Bell likewise choosing the amount of his dose, and for himself determining when and how often to employ it.

But leaving the adherents of Hahnemann’s system to reconcile these differences, we come to a point in the argument concerning both him and them, which we shall take the liberty of settling for ourselves. Hahnemann distinctly says (as we have already quoted) that the peculiar action of belladonna *does not last above three days*, and the repetition of the dose

* Jahr’s New Manual of Homœopathic Practice. Edited by A. G. Hull, M.D. Article, Belladonna, in Symptomatology, p. 161.

† Die von mir gefundene Schutzkraft der Belladonne in der Kleinsten Gabe aller, 6, 7, Tage gereicht, &c. &c., are Hahnemann’s own words.

‡ Op. cit., p. 438.

§ And this (for a preventative object) as a dose for a child of ten years, is, according to Hahnemann’s own experience, too large.

|| Dr. Black in the Homœopathic Journal, vol. i. p. 138.

¶ I am persuaded, says Dr. Henderson, Mr. Bell will pardon me for asking if he made him-

of the prophylactic at the end of every seventy-two hours is, therefore, strongly insisted upon, and (though he has mentioned longer intervals) appears always to have been acted upon. He never allows seventy-two hours to pass without the administration of a dose, though, if the epidemic of the disease be very violent, he counsels the safety, if children could bear it, of giving the second dose twenty-four hours after the first, the third dose thirty-six hours after the second, and the fourth forty-eight hours after the third; thereafter to let the subsequent doses be taken every seventy-two hours until the end, in order that the system may not at first be taken by surprise by the miasm.* Now, if we inquire the reason of the period of interval of the doses being at all events limited to seventy-two hours—never allowed to exceed that period—we are met by the (homœopathically speaking) very sensible answer, that “the peculiar action of this plant does not last above three days.” Now, if that was Hahnemann’s opinion—and these are his *ipsissima verba*—we should like to know what believer in the homœopathic action of medicines has any right to dispute it? besides, Hahnemann first proposed belladonna as a prophylactic in scarlatina, and—to use the argument adopted by Professor Henderson, in addressing himself to Mr. Bell’s experience—therefore try his way of it, and adopt his theory regarding it, else leave it altogether alone. But we do not require to do this; all the followers of Hahnemann acknowledge that the period of the duration of the effects of the “divine” remedy never exceed seventy-two hours. “Vis per 56 horas ad minimum, per 72 horas ut plurimum durat,” says the founder of the homœopathic school; and it makes very little matter if Hahnemann’s ignoble editor, Dr. Quin, daring to attempt to improve upon what he has said and done, adds, in a note, “Aliquando belladonna ad diem vigesimum primum et ultra (most convenient) vires retinet;†” or that Jahr still further improves upon both by saying that the duration of the action of belladonna extends “from one day to eighteen months.”‡ If, then, Hahnemann’s idea be correct, that the action of belladonna as a prophylactic against scarlatina is exerted only, at the farthest, for a period short of seventy-two hours, we are fairly entitled to conclude, that all these instances of preservation from the disease in which the drug was exhibited at intervals exceeding that space of time, and which have been attributed to its prophylactic action, are just as likely due to any one of the many other causes which may be presumed to have acted beneficially in contributing to the exemption, and to some of which we shall presently allude. We shall, assuredly, not insult our readers, nor these pages, by inquiring if the exhibition, according to Hahnemann’s direction, of the $\frac{1}{432000}$ th of a grain of belladonna, at intervals not exceeding seventy-two hours, can, or ever has, preserved those exposed to the contagion of scarlet fever; we can unhesitatingly answer the question we put to ourselves in

self acquainted, before he began his researches, with Hahnemann’s instructions as to the proper dose, and the interval that should elapse between the successive repetitions of it? If he did not, why try the medicine at all; since there was no other discoverer of the alleged preventative power of belladonna than that same Hahnemann, who also says, that the dose ought to be very small, and ought not to be repeated above once in two or three days? We should prefer Dr. Black’s answering this question.

* See Dr. Dudgeon’s Translation, p. 439.

† Fragmenta de Viribus Medicamentorum positivis sive in sano corpore humano observatis, a Samuele Hahnemann, M.D. Edidit F. F. Quin, M.D., p. 21.

‡ Hull’s Jahr, p. 161.

the negative. And now we pass on to the second. It may, perhaps, have occurred to our readers, that if we proposed to ourselves the settlement of this question by reference to the numbers of the facts and illustrations we advanced, that we were not doing ourselves justice, but that, on the contrary, we were assigning to the believers in the prophylaxis of belladonna an easy triumph. We beg to remind our readers of our expressed determination to give our opponents fair play; and further, of our own acknowledgment that we have been content to adduce a few of the many instances of failure of the drug in the hands of experienced and competent observers. Now, we are ready to acknowledge that, in attempting to determine for ourselves the right of the question we have proposed, we escape from none of those difficulties which all are ready to acknowledge perplex the path of him who, by a reference to the experience of several or of many, endeavours to determine a question in therapeutics. We do most entirely agree with Dr. Alison, who, in the course of an article in this journal,* replete with valuable information and the soundest reasoning, writes:

“We do not mean to deny that questions occur in therapeutics, likewise, as to which large numbers of cases may be compared with advantage, and the ‘numerical method’ applied, . . . but we think it is reasonable and right for practitioners to build their opinions, as to the powers of a remedy, on observations of very different kinds, besides the mere enumeration and statement of ultimate results of the cases in which it is given; or, as it is shortly and justly expressed by a practical author, that, in order to make up our minds as to any such question, it is better, in general, to watch than to count.”

Now, the great objection we have to the evidence which has all along been adduced in favour of the employment of belladonna in scarlatina is precisely, that “counting” has taken the place of “watching.” Numerous circumstances in the particular epidemics, and in the particular cases occurring in these epidemics, have either been altogether passed over, or, at least, have not been awarded the importance they deserve. If 100 children have been exposed to the infection of scarlatina (the degree of exposure is seldom noticed, or, at all events, is very inadequately described), and belladonna has been administered; and of the 100, if either all or a very large proportion have remained free from the disease, the *post hoc ergo propter hoc* has been, in every instance, adopted; and, apparently, as if there could be no objection offered to its adoption; because belladonna was taken and the 100 children preserved—*therefore* the belladonna preserved them. Again, and irrespective of the necessity of attention being paid, in such investigations, to the minutest particulars, to which we shall presently refer, let it be held in remembrance, that the great majority of facts which have been advanced in favour of belladonna are entirely of a negative character, while those we have brought forward in opposition to it are positive.

“I conceive,” writes the lamented Dr. Pereira on this point, “twenty cases of failure are more conclusive against the opinion than one thousand of non-occurrence are in favour of it. . . . The cases which I am acquainted with are decidedly against the efficacy of the remedy.”†

Let us very shortly glance at some of the foreign cases, and then more particularly consider what Dr. Henderson calls the “Edinburgh experi-

* January, 1854.

† Elements of Materia Medica, 2nd edit., vol. ii. p. 1238.

ments." Now as regards the former, there is not one favourable to the belladonna theory which in the least degree approaches to the "experimentum crucis," and not one of *any weight* at all; if, perhaps, we except the experiment of Dr. Dusterberg at Warbourg, the principal features of which have been already detailed. The choice of an individual in each family to whom the drug was not administered, and the subsequent infection of the *whole* so excepted, is, we acknowledge, a startling circumstance. *Still* we are not satisfied, even supposing the statements of Dr. Dusterberg to be perfectly accurate, that the mode of experiment was a good one. The exemption from the belladonna treatment of one half of each family would have been much fairer; the exemption of only one in each family would undoubtedly serve as predisposing the ones so exempted to contagion in another and very marked manner, which is not even alluded to by the experimenter: the mental influence exerted over the exempted child of each family, we hesitate not to say, would be very decided, and all in favour of his or her contracting the disease. But apart from this consideration, there are points in the narrative of Dr. Dusterberg, which make us very sceptical as to the reality of his experiments. Unlike most authors, he speaks of a "*contact intime*" between those who had taken the belladonna and those who were affected by the disease. And further, he asserts, that in most instances of those subjected to the belladonna treatment, at the end of some days there appeared "*a general eruption resembling that of measles*," and that all who presented this appearance remained free from the disease. We take leave to doubt the reality of the above altogether; indeed, as we have previously stated, it is *exceedingly doubtful* if any eruption over the skin of any kind whatever follows the internal employment of belladonna. Some of the other experiments, whose results appear in favour of the prophylactic action of belladonna, seem, as far as numbers are concerned, to tell well. Of 195, 14 attacked, 181 preserved (Berndt's experience). Of 525, 522 preserved, only 3 attacked (Schenck's experience). Out of 20 families, 2 attacked (Gumpert's experience). But apart altogether from any favourable circumstances, which, it is not unreasonable to suppose, may have existed in these cases, and of which the exemption of so large a number as 181 out of 195, and of 522 out of 525, renders all the more likely, are not facts of this kind allowed every day to pass under our eyes, and to attract our attention, while they only, and very properly too, elicit the remark, that these are unusual, or, at the most, remarkable coincidences? The truth is, in regard to scarlet fever, as well as many other infectious diseases, that an amount of capriciousness so evidently attends their progress, indeed, if we might so speak, regulates their progress, as to make it a very difficult matter to decide if, at any time, or in any degree, their occurrence is at all affected or moderated by external circumstances; and if this be true, as undoubtedly it is, how far more difficult must it be to decide if the exhibition of any prophylactic means does good?

Vaccination in its effects made itself at once recognised, and the contrast between the ravages of small-pox at the commencement of this century, and the almost entire immunity from that disease in an epidemic form, which now prevails, are facts so plainly recognisable, and so appreciable, as in the instance of that disease entirely to remove the difficulty referred to. It is altogether otherwise with *scarlatina*; notwithstanding

the introduction of belladonna, and its extensive employment, both in this country and abroad, as a prophylactic against scarlet fever, we are not aware that the mortality in either has been reduced; a circumstance which in itself militates very strongly both against the prophylactic and the remedial efficacy of belladonna.* But let facts like the following be, moreover, taken into consideration.

During the prevalence of scarlatina in Edinburgh and its vicinity, the writer of this article was requested to visit a young gentleman of twelve years of age, a boarder in an educational establishment at a little distance to the west of the city.† He was found to be labouring under well-marked scarlet fever, the characteristic eruption of which had made its appearance the day previously. In the room in which this boy lay there were other eight boys, only two of whom had suffered from the disease; and of sixty-five boys who lived under the same roof, there were thirty-eight who were in similar circumstances. The sick boy was immediately removed to the hospital at a little distance, the room he had occupied was well aired, and the bed-clothes removed from his bed; but, with these exceptions, no other means were adopted; the eight boys continued to tenant the same room, and no other case of the disease occurred.

A few months before this occurrence, the writer visited a young lady, one of a family of eight, whose ages were from six to twenty-four, occupying a comfortable, but neither very large nor very well-ventilated house, in the new town of Edinburgh. She passed through a severe attack of scarlet fever, having in particular very severe cynanche, and afterwards very profuse desquamation. None of her sisters or brothers, nor her mother, who nursed her, nor any of the domestics, contracted the disease. In the same street—not a very large one—there were, at the same time, at least two other houses in which the disease existed. These are not singular instances, nor are they recorded here in that belief; on the contrary, we know that the experience of every practitioner could amplify such a catalogue. But from such cases we do learn not a little; and this in particular, that the disease we have to deal with is a very fickle one, and that at times, in circumstances in which we should feel disposed to look with certainty to its diffusion and spreading, it will, why or wherefore we cannot tell, pleasingly disappoint us. Had belladonna been employed in either of these two instances, or in any of the numerous similar ones which have occurred, we do not doubt the exemption of the thirty-eight boys in the former, and of the eight individuals (exclusive of domestics, who were comparatively little exposed) in the latter example, would have been attributed to its prophylactic action.

As regards the “Edinburgh experiments,” we attend to them here for this reason particularly, in order to notice a remark of Dr. J. D. Gillespie,

* The total number of deaths in England and Wales from scarlatina alone, during 1847, was 19,816; and in London, during 1848, out of a total mortality from all causes of 57,628, there died 4756 of this disease. It may be said that belladonna was only very limited in its employment; but so far as we have been able to learn, there are very few medical men who have not, at one time or other, employed it in their practice; it follows, that they have abandoned it on account of its inutility, or, as is the case with several, on account of its injurious consequences.

† At the same time, cases of scarlet fever had been known to have occurred in one of the houses nearest the place of this boy's residence; and but a few days before the attack we are presently to notice, the writer had been consulted in regard to the adoption of any measures which it might be expedient to put in force, seeing the disease, for which all directors or governors of such institutions stand in great awe, was then visibly within a few hundred yards.

who observed and described an epidemic of scarlatina which prevailed in Donaldson's Hospital.* Dr. Gillespie did not employ belladonna, because "had belladonna been administered, the experiment would not have been decisive without allowing the healthy children to mingle freely with the infected." This Dr. Gillespie did not deem warrantable, as very great facilities were afforded for keeping the children separated. Fifty-two children of a hundred who had not previously had the disease, took scarlet fever. Isolation of the sound from the sick, and removal of the sick from the part of the hospital occupied by the sound children, was, under Dr. Gillespie's judicious management, effective, to the extent of preserving 48 of the 100 children.

In criticising the accounts of the Edinburgh experiments, and contrasting, in particular, the experience of Dr. Gillespie in Donaldson's Hospital, with that of Mr. Bell in George Watson's, Dr. Henderson, while assuming that some of the boys in the latter institution were protected by the belladonna, speciously endeavours to account for (what appears to us) the entire failure of the drug in Mr. Bell's hands, by urging the greater liability of the boys to contract the disease on account of the amount of belladonna taken. This argument, though ingenious, is most fallacious. We shall not go back to the Hahnemannian view of the action of belladonna in scarlatina, further than to point out this fact, and it is a very striking one, that though Mr. Bell's care and attention, and evident determination to let the experiments in his hands have fair play, led him, "*in a few days*," owing to the dilatation of the pupil and impaired vision, to lessen the amount of belladonna the boys took; yet he never in any instance for months noticed either the *sore throat*, or the *rash over the skin of the body*, which Hahnemann described, and which he asserts led him to recognise in belladonna at once the prophylactic against, and the remedy in, scarlatina. Will it be contended that the small dose produces these symptoms, and the larger those which Mr. Bell has so faithfully described? If so, we can only add, that after repeated attempts we have failed to produce any rash by the employment of very small doses of belladonna.

Dr. Henderson makes no objection to the experience of Dr. Newbigging, in John Watson's Hospital, yet his boys received *larger* doses than Mr. Bell's, for he gave the extract in the proportion of *one-sixth to one-fourth* of a grain twice a-day, and *never* diminished it, continuing its use for five weeks. Mr. Bell began with *a fifth*, and finding, in a few days, that dose to be too large, he diminished it. Seeing that the injurious effects which led to the diminution of the dose by Mr. Bell were discernible in a few days, and that Dr. Newbigging continued the employment of the belladonna in some cases, in even larger doses than Mr. Bell had ever administered, we do think that a fairer statement of the case may be put than the one by Dr. Henderson. If *large* doses of the belladonna are to be regarded at once as exposing to the contagion of scarlatina, and as freeing from that contagion, there must be an end to all argument, for such a proposition tends in no small degree to the *reductio ad absurdum*.

Now, we do not mean to say, that Dr. Henderson wishes this to be believed as his opinion, yet his words undoubtedly admit of this interpretation: for when he consigns Mr. Bell's cases to the ready action of the scarlatina poison, owing to the *largeness* of the doses of belladonna

which they have consumed, and attributes the freedom from infection which Dr. Newbigging's enjoyed to their having had the belladonna administered—he in reality says nothing less—for *during five weeks* Dr. Newbigging continued to dose the children at John Watson's Hospital with *a fourth, a fifth, and a sixth* part of a grain, while the second of these was found by Mr. Bell, in the course of *a few days*, to be too large. It will not do to say that the continuance of the drug in Dr. Newbigging's experience for a few more weeks might have caused the children to take the disease; for, most assuredly, if such effects as Mr. Bell has described were produced with smaller doses than Dr. Newbigging for the most part employed, in the course of a few days, it is only reasonable to conclude that their continuance for a period far short of five weeks, would have produced all those effects upon the boys which Dr. Henderson imagines caused Mr. Bell's boys to fall an easy prey to the contagion. Dr. Henderson, determined to leave no stone unturned, having already, in regard to other experiments, suggested that some other drug than belladonna was used, conjectures that the extract of belladonna used by Dr. Newbigging was not so strong as that used by Mr. Bell. We venture, however, to remark that just on account of the variation in the strength of the extracts of belladonna, both gentlemen would satisfy themselves of the potency of the specimens they obtained.

The very accuracy which attended Mr. Bell's experiments, the evident care and attention he paid to all the particulars in connexion with them, makes his experience one of peculiar value; and we have little hesitation in saying that his "excellent" paper will continue to be regarded alike an authority condemnatory of the so-called prophylactic action of belladonna, and on the general treatment of the disease. It is our opinion that experience has altogether failed to recommend the employment of belladonna, and that now we should be prepared to abandon the practice, as not only insufficient but absurd.

We sum up our disbelief in the prophylactic action of belladonna on account of the following reasons:

1. Numerous facts attest its want of success.
2. All those facts which apparently testify in its favour admit of other and ready explanations.
3. These explanations are, in themselves, perfectly satisfactory and philosophical.

In conclusion: We have thus seen that it is impossible to accept the facts which have been advanced (with as strict a regard to impartiality as possible) as establishing the prophylactic action of belladonna; for though, at first sight, not a few of them seem to give countenance to that view, these do not so in reality, and very many directly oppose it. It may be that a prophylactic against scarlatina exists, but, assuredly, it yet remains to be discovered; meantime, our knowledge of what affords the best protection against that disease cannot be said to have advanced far beyond what was known to Frank, in whose words, equally truthful now as when written, we shall not inappropriately close: "*Salus igitur in sola fuga contagii quæri debet, cui scopo regulæ adversus febres contagiosas jam traditæ, præcipue vero cura severa scholarum et ambulacrorum publicorum infantilium inserviunt.*"

J. Warburton Begbie.

REVIEW VIII.

1. *Lehrbuch der Physiologischen Chemie.* Von Professor Dr. C. G. LEHMANN. Dritter Band, 1852.
- A *Compendium of Physiological Chemistry.* By Professor LEHMANN. *The Chapter on Digestion.*
2. *Handbuch der Physiologischen Chemie.* Von Professor Dr. C. G. LEHMANN.—*Leipzig*, 1854. 8vo, pp. 334.
- A *Manual of Physiological Chemistry.* By Professor LEHMANN.
3. *The Bakerian Lecture, on Osmotic Force: delivered at the Royal Society.* By Professor GRAHAM, F.R.S., Corresponding Member of the Institute of France. ('*Medical Times and Gazette.*')

IN the number of this Journal for July, 1853, we gave a succinct account, drawn from the most recent sources, of the digestive fluids in their physiologico chemical relations; and more lately (p. 144) we have brought before our readers all that is contained, on the subject of respiration, in the admirable work of Professor Lehmann, which is at present being translated into half the languages of Europe. We purpose now to redeem a promise made on the former of these occasions, and to consider the important subject of absorption. Before entering upon it, however, we may briefly allude to the '*Handbuch der Physiologischen Chemie*,' just published by the same distinguished chemist.

In this, which is, with some unimportant changes in the sub-arrangement, an abstract of his larger work, the author has, he informs us, endeavoured to put together, as concisely as possible, the positive facts which may, for the present, be considered as the sure possession of physiological chemistry, and to bring forward only those conclusions which, according to our present physical views, carry the stamp of relative truth. Professor Lehmann laments that, as yet, but few facts have been so fully established as to be undisputed, and that there are few points with which more or less of doubt is not connected. This, he feels, must increase the difficulty, always great, of giving a full account of a subject in a limited space, and where it is an object to avoid discussions, and the weighing of evidence. The author has, however, succeeded in overcoming this difficulty, for we must express our unhesitating opinion that he has, in the volume in question, produced a most excellent and useful abridgment of his great system of physiological chemistry; it is unnecessary to add, that an abridgment compiled by the author of a work must be both more trustworthy and more valuable than if drawn up by other hands.

It is right that we should, also, in this place, give a short account of Professor Graham's views on the osmotic force, for although we cannot at present see how far his discoveries will affect the subject of absorption, there is no doubt they must eventually do so, to a great extent. We abstract the following from the lecture, the title of which we have given above:

"The term 'osmotic force' is applied to the power by which liquids are impelled through moist membrane and other porous septa, in experiments of endosmose and exosmose. Diffusion and capillarity are shown to be insufficient to account for it.

“The nature and *modus operandi* of the chemical action producing osmose remains still very obscure. Salts and other substances, capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances, and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action, or are wholly inert. The active substances are also, relatively, most efficient in small proportions. The chemical action must be different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view, which accords best with the phenomenon, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane at the inner surface, and as an acid action on the albumen at the outer surface.

“The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt—such as carbonate, or phosphate of soda—in the osmometer, and water outside, the flow is inwards; but with an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube.”

A table is given exhibiting the osmose of substances of all classes.

“It may appear to some, that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to be incompatible with vital conditions, and that osmotic movements must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or, at all events, premature. All parts of living structures are allowed to be in a state of incessant change of decomposition and renewal.

“The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly, therefore, be of a reparable kind.

“In other respects, chemical osmose appears to be an agency particularly adapted to take part in the animal economy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems, therefore, almost inevitable. In osmose there is, further, a remarkably direct substitution of one of the great forces of nature, by its equivalent in another force,—the conversion, as it may be said, of chemical affinity into mechanical power. Now, what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements, being entirely dependent upon extent of surface, may attain the highest conceivable velocity.

“May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition?”

We shall now proceed to the consideration of the more immediate subject of this article, and in doing so, will bring before our readers a full abstract of Professor Lehmann's chapter on the function of absorption. This chapter, it can scarcely be necessary to observe, was written before the promulgation of the views put forward in the lecture by Professor Graham, and just now quoted. As the ‘Handbuch’ bears a considerably later date than the chapter in question, we shall, in passing through the latter, note any new views or changes of opinion which may be expressed in the former.

Professor Lehmann considers that, notwithstanding the labours of the most distinguished investigators, as Poisson, Magnus, Brücke, Liebig, Jolly, Ludwig, &c., we are still without any satisfactory theory of endosmose. The establishment of such a theory is the problem, to the solution of which our attention should be next directed, for we shall be unable properly to comprehend the mechanism of absorption until we have more accurately ascertained the physical conditions of endosmotic effects, and compared them with the circumstances which obtain in the living body. To enable us to do this, it will be necessary to make out the laws of the diffusion of fluids in the manner proposed by Graham, and thoroughly to investigate the influence of the various kinds of porous septa or diffused fluids—i. e., the relation of diffusion to endosmose, and all the circumstances influencing the latter. Then only will it be possible to prove or disprove the co-operation of vital powers in absorption.

In the 'Handbuch,' the author having pointed out, that in analogous bodies the degree of capability of being absorbed will always coincide with certain closely allied physical and chemical properties, as solubility, the density of the solution, the diffusibility or endosmotic equivalent, observes, in speaking of the absorption of matters in the intestine, that "*absorption is nothing but a function of the mechanical conditions just now mentioned.*" (p. 260.) He also shows that absorption through the blood-vessels is favoured by the following circumstances: first, that the solutions of matters existing in the intestine are extremely dilute, while the blood is the most concentrated fluid of the body; hence the endosmose of these very dilute solutions is greatly promoted. This state of things is soon disturbed by the injection of large quantities of water, unless the blood be kept at its due degree of concentration by the abundant removal of the water from the blood, through the lungs, skin, or kidneys.

A second circumstance which favours the transudation from the intestine into the blood, is the constant motion of the latter in the capillaries, by which the portion which has been diluted by the watery fluid of the intestines is immediately replaced by concentrated blood.

A third is the normal acid reaction of great part of the contents of the intestine, while the fluids destined to take up the intestinal solution have a strongly alkaline reaction. "But we know," he adds, "that free acid promotes exosmose, while free alkali impedes it." (p. 262.)

The author divides the objects of digestion into, first, those which are brought into the alimentary canal in a state of solution, and immediately becoming diffused, enter into the general mass of the fluids of the body; secondly, those which are rendered soluble by the action of the digestive fluids, and so become likewise more or less diffusible; and finally, those which, dissolved or undissolved, must first be changed by certain digestive agents, and then, even though soluble, do not yield to a simple diffusion, but are introduced into the blood and the rest of the body in peculiar modes, being, before their arrival in the blood, subjected to some, though it may be trifling, changes.

The substances which undergo no essential change in the intestinal canal from the action of the digestive fluids, are particularly adapted for immediate absorption by the bloodvessels, yet it is not clearly ascertained what relation exists between these two qualities. It is not the saline

nature alone which makes the alkaline salts so easy of absorption, for many other salts are not taken up by the capillary bloodvessels; and, on the other hand, urea, alcohol, and certain poisons pass as easily, and perhaps more quickly, into the fluids of the body than many of those salts; neither is it merely the degree of solubility of a substance, but it is the union of many qualities, which confers both a capability of being absorbed, and a power of resisting the action of the digestive fluids. Since there are many poisons which the system quickly takes up from the intestinal canal, and others which are not so absorbed, we cannot expect to find the reason of these facts in an instinct of the absorbent organ, but in definite fundamental principles of the substances. On this account the author thinks it advisable to group the objects of digestion, not according to their nutritive powers, but, without reference to their useful or injurious properties, to divide them, as we have just stated, according to their digestibility—i.e., according to their greater or less capacity for being absorbed.

The author gives the following enumeration of the substances which reach the circulation, not through the lymphatics, but directly through the capillary bloodvessels:—all the neutral salts of the alkalies, the acids of which have not a greater affinity for other bodies to be met with in the intestinal contents; among these are the chlorides of sodium and potassium, the iodides and bromides of potassium; the phosphates, sulphates, chlorates, nitrates, borates, and arseniates of the alkalies; yellow ferrocyanide of potassium; the compound of rhodium and potassium (*Rhodan-kalium*); and the compounds of alkalies, with such organic acids as do not contain nitrogen. A second group of bodies, which are chiefly absorbed by the intestinal capillaries, are the acids, both mineral and organic. A third group contains alcohol, ether, wood-spirit, fusel oil.* A fourth, several volatile oils, both free from oxygen, and containing oxygen and sulphur (camphor, oil of radishes, oil of assafoetida, &c.); to this class belong also the empyreumatic and natural odoriferous matters, as musk, and the constituents of the animal oil of Dippel, &c. A fifth, several alkaloids, both fixed and volatile, for example, strychnia, brucia, morphia, thein, nicotin. Lastly, some pigments should be enumerated, which are not to be found in the chyle, but in the urine: for example, the colouring matters of alkanet, gamboge, bilberries, black cherries, rhubarb, logwood, madder, litmus, cochineal, sap green, and tincture of indigo.

In so great a variety as is presented by the substances above enumerated, it would be difficult, if not impossible, to find a common aggregate of properties to which their capability of absorption through the bloodvessels might be referred; but certain other bodies, which far exceed them in solubility, for example, do not, in direct experiments, show the least tendency to pass into the blood through the capillaries, while they are very easily taken up by the lymphatics, or, notwithstanding their great solubility, traverse the entire intestinal tract unabsorbed; thus gum, the colouring matter of turmeric, &c., which are extremely soluble, are neither taken up from the intestines by the bloodvessels nor by the lymphatics. The curara poison, which is probably identical with the wourali, and the

* Schlosberger: Arch. für Physiol. Med., Band ix. § 267—269.

poison of serpents, appear to belong to the latter class of bodies; we might think this a wise provision of nature, were it not that gum and turmeric pigment, which are comparatively harmless, are denied access to the chyle and capillaries equally with the poison of serpents, which seldom reaches the stomach: while no obstacle exists to the absorption of other poisons which are seldom received in wounds, but usually reach the intestine. It is clear that only soluble matters are capable of absorption, but the solubility of those quoted above is so variable that we cannot, by it alone, explain their capability of being absorbed by the capillary blood-vessels. The diffusibility of most of the substances, and their endosmotic equivalent, which is incontestably connected with it, have as yet unfortunately been too little investigated to refer their facility of absorption to these principles; but it is probable that this facility depends on their diffusibility, their volatility, and a certain simplicity of composition, approaching to a binary constitution; accordingly, those soluble matters which belong to none of the above groups, as albumen, emulsion, gum, and even sugar, have resisted all the attempts of chemists to account for their composition by reference to the usual laws of chemical affinity or polarity. In the 'Handbuch' it is stated that the substances which have been ascertained to be very diffusible, are absorbed through the blood-capillaries; while those the power of diffusion of which has been proved to be small, are taken up by the lymphatics. (p. 264.)

Professor Lehmann wishes not to be misunderstood in all this, as believing the process going on in the living body to be entirely physical; he is far from attempting to establish so purely mechanical a view; but he considers that the simplicity of physical principles affords a better foundation for our hypotheses, and a surer direction to our future investigations, than we should have, did we without earnest and deep reflection lightly throw ourselves into the arms of transcendental reasonings.

The substances we have enumerated do not, properly speaking, become the objects of digestion, as they pass into the circulation from the intestinal canal, in the same state in which they reached the latter. The compounds which some of them form within the body with acids, need scarcely be mentioned, as the acids do not effect any essential change in them.

Passing, then, to the actual objects of digestion, we first meet a group of bodies "which," observes Professor Lehmann, "have received the irrational name of carbo-hydrates; we know that among these cellulose, the several varieties of gum, starch, inulin, lichenin, and the true sugars have been enumerated."

Cellulose, or vegetable cellular matter, belongs to those bodies which resist the action of all the digestive fluids and other solvents; accordingly we find all vegetable tissues, which consist essentially of this substance, unchanged in the excrement of herbivorous and omnivorous animals. Cellulose has been proved by Mitscherlich to be perfectly isomeric with starch = $C_{12}H_{10}O_{10}$.* Some have supposed that the digestive fluids of the beaver may be capable of changing and dissolving it, and in fact, the organs whose secretions serve for the metamorphosis of the carbo-hydrates—viz., the salivary glands and pancreas, are unusually large in this animal.

* *Annalen der Chemie und Pharmacie*, Band lxxv. § 305—311.

It is doubtful whether the large gland attached to the stomach, and peculiar to the beaver, may be connected with the digestion of cellulose. Schleiden and Mulder have shown that cellulose is converted, by treatment with the second or third hydrate of sulphuric acid, into a substance very similar to starch; and Mulder has proved that phosphoric acid, of the consistence of syrup, may replace the sulphuric acid. Still, acid as the contents of the stomach of the beaver are usually found to be, they must be too dilute to allow us to ascribe this action to them. Mitscherlich's investigations of the action of very dilute solutions of the alkalies on cellulose, render it more likely that the alkaline juices of the salivary glands, the pancreas, and the cæcal glands effect its conversion into starch, and its further change into sugar. This chemist, too, has shown that a peculiar ferment, formed by the putrefaction of potatoes, exists, capable of destroying the cells of cellulose without attacking the starch. Might such a ferment exist in the juices of the beaver, and, in conjunction with the contents of the lower part of the small and of the large intestine, which in this animal have a tolerably strong alkaline reaction, accomplish the digestion of this substance? Still we cannot regard the digestibility of cellulose, even by the beaver, as fully established, until more direct proofs can be adduced in support of it. In the 'Handbuch,' caterpillars are also mentioned as probably possessing the power of converting cellulose into sugar, and as having highly-developed salivary organs.

Notwithstanding its solubility, the reception of *gum* into the animal organism is still doubtful. Though it seldom occurs in the food even of herbivorous animals, its frequent therapeutic employment, and its peculiar chemical and physiological behaviour, demand attention. The results of experiment make it highly improbable that even a small portion of gum is changed in digestion into sugar. All attempts to discover it in the chyle, blood, or urine have failed; but it is largely found in the excrements of animals fed on it:—thus, of 50 grains given to a duck, 46 were recovered from the excrement passed in the course of nine hours. From these and other experiments, it is evident that if this substance be at all capable of being absorbed, it is only very slowly, and in very small quantity, that it can pass into the circulation.*

Has anything been ascertained as to the diffusion or transudation of gum, which may account for the foregoing facts? According to Graham, its diffusibility is one-half less than that of sugar from starch, and four or five times less than that of chloride of sodium, but four times higher than that of albumen. Jolly found the endosmotic equivalent of gum to be much greater than that of sugar. The simplest endosmotic experiment will prove that animal membrane is not impermeable to gum; it remains to show what the mechanical conditions are which allow the passage of but so very little gum from the digestive tube into the blood. In a word, much remains to be done before we can pronounce a decided opinion on the behaviour of this substance in the intestinal canal, or venture to

* This is at variance with certain facts which appear well authenticated. The late Dr. Pereira quotes an instance in which a thousand persons supported themselves for two months on the gum which they were carrying as merchandise; and six or eight ounces for an adult are said to be sufficient to sustain life. Elements of Materia Medica and Therapeutics, by Jonathan Pereira, M.D., second edition, 1842, p. 49.

assume the interference of vital powers in resisting its absorption. The use of mucilaginous mixtures in acute diseases, if any, is evidently only negative.

“ It is well known that *starch* is the most important of all the carbo-hydrates, when considered as objects of digestion; we know that it belongs to those bodies which, in order to be absorbed, require to undergo a previous change; that the conversion of starch proceeds to the formation of dextrine and sugar, but that lactic acid is generated only to a more limited extent; and, lastly, that the saliva and pancreatic juice are the means by which this conversion of the atoms of starch is brought about.”

The facts brought forward by Lehmann in this portion of his work, in reference to the action of these fluids upon starch, are drawn from Bidder and Schmidt, and have been fully considered in our previous article; we need not, therefore, dwell upon them at present.

In addition to what we have there noticed, we may, however, observe, that a very great proportion, at least of raw starch (i. e., starch whose corpuscles have not been destroyed by boiling or other operations before their reception in the intestinal canal), remains unchanged as far as the rectum, and is very commonly found in the excrements. Starch-paste, when chewed, acquired a decidedly sweet taste in the course of two minutes. Unboiled starch did not acquire a sweet taste, even when subjected to mastication for ten minutes.

The behaviour of the digestive fluids to *inulin* is the same as to starch.

Sugar next engages the author's attention, and of its varieties he gives precedence to *glucose*, on account of its frequent occurrence in vegetable food, and of its importance as the most usual and normal product of the conversion of our most important unazotized article of nutriment, starch. Two questions present themselves in connexion with this substance—first, Does it become absorbed in an unchanged state, or does it undergo any previous change? This we have already answered in the author's words.* Secondly, Through what organs is glucose absorbed from the intestinal canal?

It is usually supposed that sugar introduced into, or formed in, the digestive tube, is simply and quickly absorbed, unchanged, by the capillary blood-vessels; a view which Professor Lehmann considers to be far from proved. He fed horses for three days on starch, one half of which was boiled, and the other half raw, mixed with one-twelfth of its weight of rye-bran; in addition, he gave each horse one kilogramme (about 2·2lbs.) of sugar in about twenty-four hours; on the third day, the amount of starch passed in the excrement of twenty-four hours was ascertained, and varied in the different horses from about a fourth to about a sixth of what had been consumed during the last twenty-four hours. In an hour and a half after the last feed, the horses were killed; the contents of the intestinal canal were then examined, and the chyle and the blood of the vena portæ were subjected to a careful analysis, in reference to their quantity of sugar.

The blood of the vena portæ was obtained by making a small opening in the abdominal parietes, and tying the vessel at its entrance into the liver, previously to opening the vein. This precaution was necessary, to prevent

* Vol. xli. p. 195.

a reflux of blood from the hepatic veins, which has misled some observers as to the presence of sugar in the vena portæ, as well as with reference to the nature of the blood of this vessel. Professor Lehmann did not, in any instance, discover either sugar or a trace of dextrine in it. He does not, however, infer from these simple experiments, that no sugar at all is, as such, taken up by the intestinal capillaries, but feels himself justified in maintaining that only a very small quantity of sugar can reach the vena portæ from the intestine.

Direct experiments proved that the absorption of sugar from the intestines does not go on so quickly as has been supposed. One or two grammes of sugar of starch were injected into the pharynx of rabbits, fed immediately before, and also after, with solid nourishment; the animals were killed in half an hour, one hour, and two hours subsequently; in every instance sugar was found in the stomach, duodenum, and jejunum, the contents of the two latter were strongly acid. Sugar (glucose) in considerable quantity was found in the stomach and duodenum of rabbits fed for some days previously solely on beet-root or on carrots.

Sugar, then, appears not to be as quickly absorbed by the intestinal bloodvessels as many other very soluble substances: the question remains, is it taken up in proportionally greater quantity, or perhaps solely, by the lymphatics? Professor Lehmann's experiments quite refute such a view. From the chyle of one of the horses alluded to, he obtained an infinitesimal quantity of sugar, in that of the other two he could scarcely discover a trace. His conclusion on this subject is, that "it cannot be denied that sugar is absorbed by the lymphatics, but it is certain that the amount which enters these vessels is a very small fraction of the quantity formed in the intestine from starch." Still, we cannot show that all the sugar in the intestine is further changed; all that we are as yet able to prove is, that a considerable quantity of starch quickly passes in the intestine through the transition stages of dextrine and sugar, to be converted into lactic acid; and that cane sugar, as well as glucose, very soon undergoes a similar metamorphosis. Bidder and Schmidt having opened the abdomen of a cat, pressed the contents out of a portion of the intestine, and subsequently opened it, introduced very thick paste of starch, tied the loop of intestine in two places, about three inches apart, and in three hours after examined the contents, which they found to consist of a fluid mass containing much sugar, very little starch, and having a strongly acid reaction. There can be no doubt that during normal digestion, a certain quantity of starch and sugar is changed into lactic acid. The foregoing experiment shows that even when the saliva, gastric juice, pancreatic juice, and bile are wholly excluded, the intestinal juice alone is competent to change starch into sugar, and sugar into lactic acid; indeed, it seems to exceed all these fluids in the rapidity with which it causes the formation of lactic acid.

The 'Handbuch' contains a few additional observations on this subject, and in them we may find the explanation of some of the foregoing facts.

Carefully-instituted experiments on animals have led to the following results, in reference to the absorption of sugar:

It has been proved that, when solutions of sugar are enclosed in tied loops of intestine in living animals, the quantity of sugar absorbed in a

given time is quite independent of the length of the loop or of the square extent of the absorbing surface; it is only when the tied loop, containing the concentrated solution of sugar, is so short that it cannot take up an amount of water corresponding to the endosmotic equivalent of the sugar, that an exception to this rule occurs.

Secondly, the absorption of the solution of sugar is in the *direct* ratio of its concentration. Entirely in unison with the laws of endosmose, do we see the loop of intestine, containing a concentrated solution of sugar, become distended in consequence of the absorption of water; a quantity of sugar, corresponding to the amount of water received by the intestine, enters the blood, until all the sugar has disappeared from the loop.

From the endosmotic law, it is evident why the extent of the intestinal loop is, if it be not less than a certain length, without influence on the absorption of sugar. If the loop be large enough to admit of the entrance of the equivalent quantity of water, only the corresponding amount of sugar can leave it, be the loop never so large. As the quantity of water which enters is dependent on the amount of sugar in the injected solution, the absorption must remain exactly the same, even in loops of the most different sizes, if the concentration of the solutions be equal.

In the results of these experiments, we find the explanation of the facts we have quoted, of the slow absorption of sugar from the intestine where it, under normal circumstances, occurs for the most part in very dilute solution, its rapid spreading over the entire small intestine, &c.

Hence it is difficult, with precision, to answer the question—important as it is in the physiology of the quantitative metamorphosis of tissue—How much sugar can, in a given time, be absorbed by an animal? as the result of the experiment will always depend on the degree of concentration of the solution introduced into the intestine. If this be very dilute, absorption will proceed slowly, as under normal circumstances; if it be concentrated, a very great quantity of water will be withdrawn from the blood, the intestine will become so filled with watery fluid as to distend the abdomen, and intense dyspnœa, and often death, *will* be produced.

It is likely that the gastric juice is the agent in digestion which effects the metamorphosis of *cane-sugar* into glucose. In the 'Handbuch,' this effect is, however, attributed rather to the action of the pancreatic juice, or the ingredients of the intestinal contents.

Sugar of milk behaves, in the intestinal canal, like glucose, passes very quickly through the small intestines, can be traced, in about an hour after being taken into the mouth, to the cæcum, and, like glucose and cane-sugar, leaves an intensely acid reaction in the jejunum and ileum, which continues for three or four hours after the sugar has been swallowed.

Vegetable mucus passes, for the most part, unchanged with the excrement (Frerichs), as do *pectin* and its derivatives.

No class of aliment has at all times presented so many difficulties to the physiologist as the *fats*; and even in the present day, we cannot flatter ourselves that we perfectly understand the process of their digestion. Our experience of some of their physical and chemical properties renders it difficult to explain the mode of their reception into the blood; since they are absolutely insoluble in water and watery solutions, and a watery fluid everywhere permeates the tissues of the body, we cannot admit their

diffusion, in the ordinary acceptation of the word; while their decomposition requires either more powerful agents than are usually met with in the intestinal canal, or a longer time than they generally remain in it. The saliva and gastric juice exercise no influence on the mechanical or chemical condition of fat, and all investigators admit that its digestion does not commence until it has reached the duodenum. Here, but still more in the lower portions of the small intestine, the globules of fat become smaller, the fat becomes more finely divided, and the chyme more like an emulsion. It is, therefore, with the aid of the microscope that we can trace the progress of its digestion.

Bidder and Schmidt have very clearly proved that Bernard was in error when he stated, that the absorption of fats was entirely due to the action of the pancreatic juice. Still, though the opinion of the importance which the latter observer entertained with respect to this fluid appears to have been refuted, certain facts discovered by him cannot be wholly laid aside. Thus, it cannot be denied that no animal fluid, when shaken for a short time with fat, gives so perfect an emulsion as the pancreatic juice; no animal fluid retains fat so long in a state of emulsive suspension: in reference to this property, the bile, for example, is not to be compared with it. The pancreatic juice, further, contains the only body, found in chemistry, which is capable of resolving with such rapidity the neutral fats into glycerine and fatty acids. Whosoever has brought fresh viscid pancreatic juice in contact with fat, cannot suppress the thought that it must essentially contribute to the digestion of this substance. But the experiments of Bidder and Schmidt will be found so conclusive, that this idea must, at least for the present, be given up. Lassaigne and Colin found the pancreatic juice of the calf and the horse to be very thin, and incapable of forming an emulsion with oil, while that of other herbivorous animals possessed this property in a high degree.

Bidder and Schmidt have shown by experiments on dogs, in which artificial fistulæ of the gall-bladder were produced, the ductus choledochus having been previously tied, that the bile which passes into the intestine is without any influence on the digestion of albuminous matters and of starch, but that it has a great effect in determining the quantity of fat to be retained in the body and applied to the purposes of life, $2\frac{1}{2}$ times less of the latter principle being, in the most favourable case, absorbed when the supply of bile is cut off than when it is allowed uninterrupted access to the intestine.

“The participation, then, of bile,” observes Lehmann, “in the digestion of fat, is placed beyond all doubt, although it can by no means be denied that a small portion is absorbed without the co-operation of this fluid.”

Brodie, as well as Tiedemann and Gmelin, thought they had proved, that after tying the ductus communis choledochus, the lacteals contained, notwithstanding the use of a diet abounding in fat, a colourless transparent fluid; while Magendie, and recently Lenz, observed their contents to be milkwhite under similar circumstances. But it has been shown, that even when bile is wholly excluded from the intestine, a small portion of fat is absorbed, which would be sufficient to give the chyle a milky appearance. Schmidt, however, proved, by quantitative analysis, that chyle taken from the thoracic duct of a healthy dog, fed with beef eight

hours before death, contained many times more fat than that obtained from dogs furnished with artificial biliary fistulæ, although the other ingredients of the chyle varied but little in the several specimens; experiments which fully confirm the statement, that the bile essentially contributes to the resorption of fat. We have in our twelfth volume alluded to the fact, that bile seems to render the intestinal mucous membrane more permeable for fats.

It is clear, then, that fat in digestion is taken up principally by the lacteals; but Chr. Fr. Schmidt's and the author's observations, show that the capillary bloodvessels take up a small portion, as is proved by the increase of fat in the contents of the portal vein some hours after feeding. This fat does not, however, pass directly into the capillaries, but, in common with the salts and all other matters which do not enter the lacteals, it has previously to traverse several series of cells.

We now pass to the group of bodies which must, before being absorbed, undergo an essential change in the intestinal canal. To this class belong not only the albuminous matters, but also their remote derivatives, as, for example, many gelatinous substances, and in addition to these, a number of less well-known matters, as synaptase and diastase, the poison of serpents, curarin, &c. The albuminous matters are not merely dissolved, but are changed into substances, which, although similar in their elementary composition to those from which they are derived, yet, in their physical and many chemical properties, differ essentially from them. The author proposed the name Peptones, for albumen, fibrin, casein, &c. changed by the gastric juice, believing that it is these peptones which undergo absorption, to be subsequently metamorphosed in the lymphatics into the well-known coagulable albuminous matters.

In speaking of the gastric juice, we have stated the discordant results of Lehmann's and Bidder and Schmidt's experiments, as to the proportion of albumen which that fluid is capable of dissolving. Schmidt has shown that gastric juice free from saliva is a more powerful solvent of albuminous matters than that which is mixed with the latter secretion; doubtless it loses some of its influence on these bodies by the saturation of a portion of its acid by the alkaline saliva. He has likewise proved that the addition of bile wholly removes its solvent action, even when the mixture still shows a decidedly acid reaction; hence, if undigested albuminates reach the duodenum, the gastric juice will have lost all power over them. If there be acid reaction in the duodenum, it proceeds not from free hydrochloric acid, but from biliary acids separated by it, which very soon become insoluble, or are absorbed; so that usually after the use of meat, no acid reaction is to be found in the jejunum. But although the gastric juice is by no means sufficient to digest completely all the protein taken into the stomach of carnivorous animals, and although the bile and pancreatic juice have no effect on the protein compounds, the albuminous substances which leave the stomach undigested are seldom found in the excrement; they must therefore be dissolved, and rendered capable of being absorbed by the action of the intestinal juice. And in fact, Bidder and Schmidt have, in the clearest manner, demonstrated that this fluid possesses the power of digesting coagulated and insoluble protein compounds; and it is remarkable that the bile and pancreatic secretion, which deprive

the gastric juice of the power of digesting the albuminates, do not even diminish this property in the intestinal fluid.

In the large intestine, however, the protein compounds can suffer but little change; not only is the amount of fluid secreted in this part extremely small, but Lehmann's and Steinhauser's experiments show that albuminates introduced into the large intestine, or lower part of the ilium, are found almost unchanged in the excrement.

Besides the protein bodies and their immediate derivatives, many other substances may be classed in this fourth group, which do not possess the great physiological value of the albuminates, but have a similar behaviour in digestion. We have already spoken of the gelatinous tissues in this point of view. In addition, we might enumerate some poisons which, like the albuminates, are not capable of direct absorption by the blood-vessels, but are, in the first instance, so changed by the gastric and intestinal juices, that they are taken up by the lacteals, and reach the blood as harmless substances. Nearly allied to the protein bodies, without exactly belonging to them, are diastase and emulsin; and analogous to emulsin are many substances which have, it is true, been less accurately investigated, but which agree with one another in their amount of nitrogen, and in being insoluble in spirit, and soluble in water. Of these bodies, curarin has been, perhaps, the most closely examined; taken into the digestive tube, it gives rise to no morbid phenomenon, while, introduced into the blood, it causes almost instant death. Closely related to it, both in a toxicological and chemical point of view, are the poison of vipers, and those poisons emanating from contagious diseases, as hydrophobia, typhus, distemper among cattle, &c.; at least, Renault* has shown that the flesh of beasts affected with these and similar maladies, may be eaten with impunity by carnivorous and omnivorous animals; while it is well known, that the juice of such flesh introduced directly into the blood or into wounds, produces consequences most dangerous to life; proving that these matters are incapable, in their unchanged state, of being absorbed either by the bloodvessels or the lymphatics of the intestinal canal; and since most of them cannot be demonstrated in the solid excrements, it is evident that they must be so metamorphosed by the digestive fluids, that when they reach the blood, they can no longer exercise their poisonous qualities. While Lehmann cannot agree with Bernard, that animal membranes are wholly impenetrable to these bodies (emulsin, diastase, curarin, and the poison of vipers), he observes, that it is established that, like albumen, they have very weak endosmotic powers; and it is for this reason that the protein bodies, apparently so fitted for the purposes of nutrition, must be metamorphosed by the digestive fluids before they can be absorbed. Without the intervention of the gastric and intestinal juices, even soluble albumen and casein would be taken up from the digestive tube in far too small quantity to suffice for the support of the organism. In fact, the albuminates are absorbed in very small quantity; it is not until they become peptones that they are largely taken up.

We have as yet no idea of the mechanism of absorption through the lymphatics. All attempts to explain it have been directed to the

* Comptes Rendus, tom xxxiii. pp. 532—535.

mechanism of the motion of the fluids in these vessels, but not to the process of absorption itself. Even the discovery by Brücke of fibre-cells in the intestinal villi, which has been confirmed by Kölliker, while it explains the emptying of the commencement of the lymphatics, does not unravel the mystery of the mode in which these minutest ramifications of the lymphatics become filled. It is the capillary bloodvessels of the villi, rather than their lymphatics, which may be compared to the fibrillæ of the roots of vegetables. In the lymphatics there is no fluid so concentrated, or no soluble substance, to cause an attraction of the fluids from the intestine; indeed, the points of the lacteals do not float in the intestinal fluids; the latter must traverse several series of cells, and come in contact with the fine capillary vessels before they reach the actual lymphatic. It would appear as if only what is difficult or incapable of absorption by the bloodvessels is taken up by the lymphatics. But neither can we, by assuming a specific permeability of the membranes, solve the problem of absorption through the latter vessels.

In our previous article we mentioned the results of Bidder and Schmidt's experiments, as to the quantities of the different digestive fluids secreted by an adult man in twenty-four hours. Startling as it may appear, the juices flowing into the *intestinal canal in that space of time, amount to about the sixth part of the entire weight of the body*, and form a mass of fluid far greater than the quantity of blood which, according to the more recent and most trustworthy calculations is contained in the body of an adult. This is, however, sooner or later, almost completely received back again into the vessels, a constant flux and reflux of watery solutions, which cannot be unimportant, and must react on the processes of nutrition and metamorphosis of tissue proceeding from the blood.

Recent investigations have placed beyond all doubt the immediate dependence of certain secretions on special portions of the nervous system: thus, not the smallest quantity of saliva will be secreted without the influence of the nerves; and Ludwig and Rahn have, by rigid experiments upon rabbits, proved the direct effect of the nervous system upon the parotid, through the facial nerve, indirect through the third branch of the trigeminus (by exciting to masticatory movements), and its reflex through the glosso-pharyngeal nerve. That the secretion of the gastric juice is under nervous influence, is proved by the copious flow of this fluid excited by holding tempting food before a fasting dog; and the par vagum has been supposed to be the agent in this case, yet the division of both vagi has been found in some cases to interfere neither with the secretion of the gastric juice, nor with the motions of the stomach. But it is not credible that a nerve, which so abundantly supplies this organ with filaments, should influence neither its secretions nor its motions, especially as Bidder and Schmidt have shown that it has nothing to do with the production of the feeling of hunger. Volkmann has correctly traced these apparent anomalies to the anatomical relations of the nerve, for it expends the greater part of its cerebro-spinal filaments on the head and upper part of the neck, and the further it sinks down towards the diaphragm the more filaments it receives from the sympathetic; the abdominal vagus, then, is quite different from what it is when issuing from

the skull; it contains filaments which cannot be excited by its cervical portion, and the effect of which on the motions of the stomach cannot be interrupted by the division of the nerve in the neck. What is true of the motion of the stomach in this respect, may also be true of the secretion of the gastric juice. Thus it is clear, that further experiments, connected with great difficulties, are required in order to answer the question, on what nerve or combination of nerves the secretion of the gastric juice immediately depends?

Having considered the process of digestion in its several bearings, and especially the relations between the objects of digestion, the digestive agents, and resorption, the author proceeds to give some hints as to the digestibility of compound aliments, as distinguished from the alimentary principles already spoken of. This is, however, a subject beset with many difficulties; and, as the author does not place much confidence in the results of his experiments, we shall touch but lightly on them. It is one, he observes, which belongs more properly to the practical physician than to the chemist. Physiological chemistry can do little more than supply the physician with the fixed principles or scientific means by which he may more accurately appreciate well-observed practical facts, and on them commence the foundation of a dietetic system.

By the digestibility of an aliment we understand the ease with which the digestive fluids prepare it for absorption, or the shortness of time in which it undergoes absorption, and disappears from the intestinal tract. This is much influenced by the quantity of food ingested at a time. Soluble coagulable albumen is, according to the result of Lehmann's experiments, changed in the stomach; and is not, as Frerichs supposed, absorbed unaltered. The albumen of one egg given to a dog previously kept for twelve hours without food, had entirely disappeared from the stomach at the end of one hour, but when the albumen of eight or more eggs was given at a time to the same dog, the presence of coagulable matters in the stomach could be demonstrated after the lapse of three or four hours.

Fluid, finely-divided, and porous aliments, are more accessible to the digestive fluids, and must consequently be easier of digestion [*cæteris paribus*] than others which do not possess these properties in a similar degree.

Frerichs has demonstrated by experiments on living animals, that boiled fibrin dissolves much more slowly in the stomach than unboiled; and a similar result is obtained by treating both kinds of fibrin outside the system with natural or artificial gastric juice.

Soluble casein, as it occurs in milk, is, as is well known, very quickly coagulated in the stomach, and is then again, but gradually, dissolved or digested; consequently, it is probably the most difficult of digestion of the unboiled protein substances; it differs much, however, in this respect according to the looser or denser quality of the coagulum, for, as Elsässer states, the jelly-like coagulum of women's milk is much more quickly digested than that of cows' milk, which forms in the stomach a compact ball-like mass.

Gelatine is one of those bodies which most easily dissolve in the stomach. Tendons and cartilage are among the substances most difficult of digestion,

and are often found little altered in the excrement of carnivorous animals: the proper elastic tissue and elastic fibres entirely resist the action of the digestive fluids.

Chemically-prepared muscular fibre is, as appears by some experiments of the author's, very easy of digestion; in the coagulated state it is about equal to coagulated albumen and casein. Although this substance is perfectly identical in all kinds of flesh, and also in the smooth muscles, experience shows that the digestibility of the smooth and striated muscles, and even of the latter, from different animals, is extremely different. This is owing to the fact of the former wanting the dense though thin coating, so difficult of solution, in which the fasciculi of the latter are enclosed; and to their being enveloped merely in loose areolar tissue, which is easily permeated and dissolved by the digestive fluids. It was for this reason that Beaumont found tripe to disappear so quickly from the stomach (within an hour), and oysters to be digested more quickly than beef and other varieties of meat. Professor Lehmann goes on to say, that the flesh of young animals is, in consequence of its fasciculi being much thinner, and consequently exposing a greater surface to the action of the gastric juice, much easier of digestion than that of older animals. This is certainly contrary to the usually-received opinion, as experience seems to prove that lamb and veal, for example, are not so easily digested as mutton and beef. Fish is likely to prove difficult of digestion to many people in whom the powers of digestion are not over strong, because, when it is brought, in a state of fine division, into contact with fluids, it forms a solid homogeneous lump, on which the digestive juices can work but slowly from the surface. Neither can meat be classed with the easily digestible bodies, because the areolar tissue must be dissolved before the gastric juice can reach the muscular fibres.

The difference between the digestibility of raw and boiled or roast meat is not very considerable, the good effect of cooking, in loosing the areolar tissue and partially destroying the organic structure, is, to a certain extent, counteracted by the coagulation of the albumen contained in the juice of the meat. The author adds, that meat which has lain in vinegar is rendered more digestible by the loosing of its areolar tissue and muscular fibres, while its digestibility is greatly impaired by the process of smoking.

As the digestion of fat does not commence until it reaches the small intestine, this substance, if taken into the stomach in large quantity, acts injuriously by liquefying and enveloping other substances, which it thus renders inaccessible to the digestive fluids; it also, if detained in the stomach, becomes rancid, and forms volatile acids, which, in some way not clearly understood, materially injure digestion; but if used in smaller quantity, it is quickly absorbed, and even facilitates the digestion of many albuminous and amylaceous substances.

The chief nutritious principle contained in *vegetables* is starch, which, if not taken in quantity disproportionate to the amount of saliva, pancreatic juice, and intestinal juice, is easy of digestion. But the digestibility of vegetables generally depends on the nature of the cells which contain the starch and protein bodies; if these be covered with epidermis, nothing will be dissolved, as the epidermis of plants is perfectly impermeable to

the digestive fluids. Boiling is useful by loosing the intercellular substance of the parenchyma, and by bursting the outer layer surrounding the starch-corpuscles: but the most important part of the digestion of vegetables takes place in the small and also in the large intestine; and the enormous size of the cæcum in most herbivorous animals, shows that an essential part of the process must there take place.

To these remarks on digestion, we may add a few concluding observations on nutrition.

In estimating the nutritive power of food, we must consider two principal points—first, the proportion in which it contains the four grand elements of animal regeneration—viz., albuminous matters, fat, carbohydrates and salts; and, secondly, the circumstances under which the organism has more or less need of all or any of those elements, in order to maintain its integrity, or to produce certain powers of action. In this estimation, the condition of the elements belonging to the several groups is not to be lost sight of, as this will greatly influence their digestibility; thus, lightly boiled albumen is more digestible than hard boiled, &c.; this branch of the subject has, however, already been briefly considered.

As the azotized ingredients of food—i.e., the albuminous—are those which principally contribute to the reproduction of the tissues and organs of the animal system, investigators first directed their attention to the amount of these matters contained in the several aliments. Lehmann quotes the tables of Boussingault, Thompson and Schlossberger, and Kemp, &c., in reference to this point, and adds:

“Of the interesting conclusions which may be drawn from these investigations, we will only mention, that the *amount of nitrogen contained in muscular fibre is not essentially different in the entire animal kingdom*. The flesh of fish affords the same absolute amount of nourishing material as that of the higher animals; oysters, on the other hand, contrary to the usual opinion, yield much less; demonstrating the difference which exists between containing an absolute amount of nourishing matter, and containing what is easily digestible.

“We need scarcely mention, that we cannot infer the value of animal food for the reproduction of the blood and tissues from its amount of nitrogen;—for the nitrogen we obtain from it is partly derived from its gelatinous matters, and it is very doubtful whether the latter can contribute anything to the reproduction of the tissues—at least, it follows with certainty, from their composition, that they cannot fulfil the same ends as the proper albuminous substances.” (p. 443.)

On the scale constructed by Liebig, showing at a glance the proportion between the albuminous, or plastic, and the unazotized elements of the ordinary articles of the food of man, the former being taken as unity, the author remarks that, as this chemist considers the unazotized matters principally as the generators of animal heat, and as the latter—i.e., the fats and carbo-hydrates—exercise an influence in the production of animal heat, varying in proportion to their amount of oxygen, it would be necessary, in order to simplify the proportion, to make the value of the fat equal to that of the carbo-hydrates by calculating the oxygen; thus, ten parts of fat will about equal twenty-four of starch; sugar of milk and glucose can, of course, be reduced to the corresponding value in starch by calculating the water. Lehmann, however, remarks that, in estimating the relative value of aliments, it would be necessary to distinguish the fat from the carbo-hydrates; for that both are required to form food perfectly

suited to the wants of the animal system is evident, amongst other things, from the universal desire to mix amylaceous articles with fat, and *vice versa*, and from the presence of both fat and sugar in milk. As yet, we have not ascertained, by decisive experiments, the specific functions of each in the metamorphosis of tissue: Lehmann proposes, until this shall have been done, to take the mean constitution of human milk as the emblem of the combination of those four groups of bodies which is best adapted to the wants of the human organism—i.e., 10 parts of plastic matter, 10 of fat, 20 of sugar, and 0·6 of salts; but in considering this part of the subject, we must not forget that the proportions of the several species of aliment should vary as the wants of the system, under particular circumstances or at different times, may require, of which we have illustrations in the changes which the mammary secretion itself undergoes according to the age of the child, and in the variety presented by the milks of the sundry classes of animals.

REVIEW IX.

Epilepsy and other Affections of the Nervous System, which are marked by Tremor, Convulsion, or Spasm: their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M.D., &c., &c.—London, 1854. 8vo. pp. 144.

DR. RADCLIFFE divides his work into two parts; the first treats of the physiology of muscular contraction; the second of the pathology, periodicity, and treatment of epilepsy, and of the affections which Dr. Radcliffe considers to be allied thereto. The physiology professes to be novel, and to be the basis of the pathology and therapeutics. It is wholly opposed to the current views entertained as to the nature of muscular contraction; we therefore propose to examine it critically, as well as the pathological and therapeutical views by which it is followed; moved thereto, however, rather by a feeling of regard for the author, than by a high estimate of the validity or importance of his doctrines.

Dr. Radcliffe is of opinion that the common doctrine of muscular contraction is erroneous. It is generally believed that it is induced by certain stimuli; such a supposition does not, he thinks, bear the test of examination. The condition of the muscular system known as *rigor mortis*, and the "contraction which occurs in the muscle-like tissue of the ductos under the influence of cold," especially suggested doubts to Dr. Radcliffe as to its accuracy. Dr. Radcliffe's hypothesis is this:

"That all stimulants, vital and physiological, antagonise muscular contraction; and that contraction happens from ordinary molecular attraction, when the muscle is not stimulated."

Or, in other words of the author:

"Because some stimulus has been removed which had prevented the natural molecular attraction of the tissue from coming into play, when the phenomenon becomes analogous to the contraction which takes place in a bar of metal, or in any other inorganic body, on the abstraction of heat."

By this hypothesis the contraction of muscle, the movements of the

blood in vessels independently of the heart, and the action of the heart, receive a (to Dr. Radcliffe) satisfactory "physical explanation."

Dr. Radcliffe builds up an obscure hypothesis on premises still more hypothetical. We will give illustrations of his method of procedure in establishing his doctrines.

"A muscle is said to be irritated, to contract by the simple contact of certain natural or artificial substances—the bowel by the food, a detached fibre by the point of a needle—how is this? What is the influence at work, and what is the manner of the operation? It is difficult to acquiesce in the common belief that the hollow involuntary muscles are excited to contract by their natural contents. If the morsel exerts this influence in the gullet, how can it effect an entrance? Or, if effecting this, how is it that it is not fixed immovably in one place? There is no physical necessity why the movements should follow in any certain and definite order. On the contrary, the morsel may move upward or downward indifferently, as it is seen to do in the throat of a cow during rumination. Indeed, so far from exciting contraction, it appears as if the food remained quietly in the stomach, and in every other part of the alimentary canal, until the digestive process is complete, and that contraction *happens* when those molecular changes are at an end which could have acted as a stimulus to the muscle." (p. 9.)

The entire difficulty encountered by our author arises, we need hardly say, from the circumstance, that he has not rightly comprehended the physiology of deglutition and of peristaltic action. It is very well known that there is a necessity, not physical, but *vital*, why the movements of the apparatus called into action during the process should follow in a certain and definite order. It is an essential point, indeed, in all vital processes, that they go on necessarily, according to a pre-ordained and fixed method and plan. Science investigates this order: hence physiologists have traced out the existence of a covering to these hollow involuntary muscles, of incident excitor nerves distributed through and upon that covering, of nerve-trunks taking their course to the central axis where the co-ordinating apparatus is placed, and of other nerve-fibrils coming from that axis and distributed to the muscles which they regulate; all these take their share in the act of deglutition; but the "natural contents" are never in "simple contact" with the hollow and involuntary muscles, which perform by their pre-arranged and combined action the act of deglutition. Nor is it in accordance with the facts elucidated by physiological research to say, that it appears as if the food remained quietly in the stomach until the digestive process is complete, for observations prove exactly the contrary.

We need not multiply extracts by way of illustrating Dr. Radcliffe's method, for his other arguments do not differ in weight or method from the preceding. The following summary of his first chapter ends, however, with a *chemical* argument, as facile and as insequential as his physiological; we therefore subjoin it:

"Contraction, then, as seen in ordinary muscle, would appear to be analogous to that contraction which takes place in inorganic bodies on the abstraction of heat, with this only difference, that more forces have to be abstracted from the organic than from the inorganic body. The analogy is indeed perfect, for even that remarkable degree of contraction which is witnessed in muscle, as compared with that which is seen in inorganic bodies, may be a natural consequence of the physical constitution of muscle; for as muscle is composed almost exclusively of

certain gaseous elements, it *may* contract to a great degree under a small abstraction of heat, because it is the law of its constituent gases so to contract." (p. 24.)

In chapter second, muscular contraction, as manifested in the coats of vessels, is elucidated. *Heat* generated during nutrition and innervation expands the vessels.

"If heat, and the agents associated with heat, excite expansion in the vessels, it is easy to understand how they must give rise to a force—a '*capillary force*'—which is independent of the heart; and how they must co-operate with the heart in facilitating the progress of the blood through the vessels of the living animal, in precisely the same way as the warm-bath co-operates with the injecting-syringe of the anatomist in procuring the admission of the melted wax into the vessels of the dead animal. It would be different if the vessels acted upon were composed of ordinary solids, for then the heat would cause a greater degree of expansion in the fluids contained within the vessels, than in the vessels themselves; but in reality the vessels are gases, *coerced* into solidity for the time, rather than ordinary solids." (p. 27.)

We never before suspected that the "subject" to be injected was put into warm water for any other purpose than to prevent the melted wax becoming solid too soon, by the abstraction of its heat; and as to the vessels being constituted of *coerced* gases, we would ask, why are they more so than water itself, or albumen, or the *materies* of the blood-corpuscles?

The third chapter treats of "muscular contraction as manifested in the heart;" and here we have a repetition of assumptions as groundless as they well can be. We subjoin an illustration:

"There is reason, also, for supposing that the systole is always contemporaneous with a lessened supply of nervous influence, and not with an increased supply. The blood escapes from the heart in gushes; and there is some reason for believing that the nervous energy, in some parts at least, is generated in corresponding gushes. At all events, the blood passes by gushes into the great cerebro-spinal masses, and causes corresponding pulsations in them whenever they are exposed to sight. It may further be supposed that nervous influence is developed and distributed the moment the blood comes in contact with the organ in which it originates, for this influence is sudden and subtle as electricity. All this may be supposed, and, if so, then the heart will be supplied with the gush of nervous influence, not during the systole, but during the diastole." (p. 32.)

There is no more interesting study than the circulation of the blood through the capillaries; and when the difficulty of the study is considered, there is no branch of physiology in which greater progress has been made. This we owe principally to microscopic research. Never yet, we believe, has the blood been seen flowing through the capillaries by "gushes," nor is there a single phenomenon known which would indicate any such condition of the capillary circulation. We have seen how, in the last paragraph, "all this may be *supposed*." In a page or two, these pure (and unfounded) suppositions assume the shape of "the rational and physical explanation of the heart's rhythm," as if they were demonstrated propositions.

"The heart contracts, and sends a flood of blood to the great sources of innervation. There the blood originates a gush of nervous influence, which, passing along the nerves to the heart, causes the diastole. The diastole cuts off the arterial stream, and the blood sets in from the auricles and veins. The cutting off of the arterial stream interrupts the development of nervous influence, and this interruption, reacting upon the heart, removes the influence which had caused the

diastole, and the systole returns. And so round the same circle. The arterial stream sets out at the systole, and develops the nervous influence at its proper sources, and the nervous influence inducing the diastole causes the systole by cutting off the supply of blood, and by thus suspending for a time the active development of nervous influence. And thus in a regular series systole gives rise to diastole, and diastole is followed by systole, so long as the vascular and nervous systems retain their integrity. This is *one* part of the process." (p. 35.)

It certainly is not. There are no "gushes of nervous influence," and no "cutting off" of the gushes. The whole scheme is purely imaginary.

To determine the PATHOLOGY of epilepsy, Dr. Radcliffe examines "the real state of the great systems chiefly concerned—the vascular, the nervous, and the muscular." In all he finds want of tone. In the vascular system, "all the symptoms are found to offer a direct contrast to those symptoms of plethora which are met with in the butcher." As to the nervous system, all the facts declare the same want of vital activity. As to the muscular, its condition he finds to agree with that of the vascular and nervous systems. Dr. Radcliffe finds—

"Everything is in harmony with the physiological premises; and, as might be anticipated from these premises, the convulsion would seem to depend upon *want* of vital stimulation, which want had allowed the molecular attraction of the muscles to come into play, and gain the ascendancy."

Dr. Radcliffe is silent as to the etiology, whether of the predisposition or of the paroxysm.

The affections which are considered as "allied" to epilepsy, are, in fact, neither more nor less than incongruous groups of diseases, in which there is the common symptoms of irregular muscular action, chorea, delirium tremens, poisoning by mercury, hydrocyanic acid, lead, &c.; fever, retention of urea in the blood, tetanus, hydrophobia, &c., are considered as allied affections, in so far as the muscular system is affected. In all, Dr. Radcliffe finds a corroboration of his views.

"In conclusion, therefore, it must be admitted, that the entire history of epilepsy, and of affections allied to epilepsy, is at complete variance with the idea that the muscles are provoked to excessive contraction by excessive stimulation. It is as much at variance with this hypothesis as it is in harmony with that doctrine of muscular contraction which was propounded at the commencement of this inquiry, which doctrine is—that all stimulants, vital and physical, antagonize muscular contraction, and that contraction is brought about by ordinary molecular attraction, when the muscle is *not* stimulated. This doctrine, indeed, supplies the key to the pathology, and the facts belonging to the pathology furnish the only proofs which were wanting of the truth and universality of the doctrine." (p. 108.)

The chapter on periodicity communicates nothing new. Dr. Radcliffe refers the reader for information as to diurnal, monthly, and annual periodicity, either to personal observation, or to "the admirable little treatise of Dr. Mead, '*De Imperio solis ac lunæ in corpora humana et morbis inde animalis*.'"^{*}

The chapter on the treatment of epilepsy and the allied affections, needs no special criticism. Dr. Radcliffe recommends a tonic and stimulant plan of treatment; a nutritious and animalized diet; a liberal allowance

* We need hardly state that a little library of books on the subject has been written since the date of Mead's essay; and that it has received a large development through natural history on the one hand, and meteorology on the other.

of wine or beer; coffee, to the exclusion of tea; celibacy; quinine, iron, turpentine, camphor, chloric æther; counter-irritants; hot baths, one every day, &c.

"In actual practice, I have rung changes upon these different stimulants, either giving them alone, or combining them with iron or quinine, substituting one for another, according to the changing circumstances of the case, and always allowing, at the same time, a liberal supply of dietetic stimulants—upon which, indeed, hope is mainly to be based; and I have had every reason to be satisfied with the results. I have never met with a patient who has not been benefited; for even where the case has been of long standing, and the fits have kept their ground, there has been a manifest diminution of intellectual torpor, the face had lost a good deal of the brutalized expression which had been creeping over it, and the distressing nervous headache has disappeared, if that symptom had been present; and I have met with many patients who have been completely cured." (p. 130.)

Dr. Radcliffe has grounds for congratulation as to the success of his treatment, whatever may be thought of his hypotheses. If he had occupied his pages with details of some of the cases he has so happily cured (or if not cured, at least relieved), his work would have had a practical value which it does not possess in its present form. Empirical knowledge of this kind is very estimable in the absence of information as to the true nature of epilepsy—we mean histories of carefully-observed cases; for it is mainly by such histories that the circumstances under which remedies may be prescribed with some reasonable hope of success, can be ascertained. We are sincerely sorry that we cannot express a more favourable opinion of Dr. Radcliffe's little work; for we are assured that he is a gentleman of personal worth and an estimable character. We should, however, do a wrong to our readers, if we were to say more of it than it deserves.

REVIEW X.

Cases of Bright's Disease, with Remarks. By SAMUEL WILKS, M.D. Lond.
(*'Guy's Hospital Reports,'* Second Series, vol. viii.)

IN a previous number* of this journal, we endeavoured to correct what we hold to be the errors of those pathologists who see, in the various abnormal states of kidney which are commonly included under the term "Bright's disease," only successive stages of a single morbid process; the continual tendency of the disease being, as they suppose, to produce that small, contracted, granular kidney, the weight of which is, perhaps, by one-half, or even two-thirds, less than that of the healthy organ. We adduced two classes of facts which appear to us to be quite irreconcilable with the theory of the *oneness* of Bright's disease—these are, *first*, the anatomical characters of the diseased organ, as revealed by a post-mortem examination; and, *secondly*, the condition of the urine, and the attendant symptoms, observable during the lifetime of the patient. If, by an edict of arbitrary power, any pathologist were restricted to the observation of one of these classes of facts, he would labour under a great disadvantage, and he might be expected for a long time to halt between

* Jan. 1853, vol. xi. p. 56.

two opinions with respect to the theory in question; but for those who have the opportunity to examine and compare the clinical history with the pathological anatomy of renal disease, it appears not difficult to arrive at the conclusion that, while the term "Bright's disease" is retained, scientific accuracy and practical utility alike demand a clear distinction between the different and, in a certain sense, antagonistic morbid conditions which are included under it.

The appellation "Bright's disease," as a comprehensive generic term, is one which may very conveniently be perpetuated, and the disuse of which we should regret, if for no other reason, because we delight to honour the name of the distinguished physician who opened to us the great field of renal pathology. But, undoubtedly, the term in question affords a good illustration of that class of idols which Bacon has called *idola fori*.* Is not the idea of unity, of a single disease, implied in the very term "Bright's disease?" This is, unquestionably, the idea which the name conveys to every young pathologist, and the error which it implies can be met and corrected only by teaching him that the term is applied, not to a single disease, but to a *class* of diseases whose existence and whose clinical history were first made known by Dr. Bright.

The intelligent author of the paper which we purpose now to pass in review believing, as he states, that the microscope has led into error some who have trusted too exclusively to its use, "with a view of testing the value of some of the new theories on renal disease," has reviewed his cases; "and, to put them to further proof, he has carefully watched every instance of the disease which has entered Guy's Hospital during the past year." The result of this labour is very creditable to Dr. Wilks as a pathologist and a practical physician. With respect to some points of detail, we shall have to express our dissent from his views; but, so far as regards the broad principles of renal pathology, we are happy to find that the erroneous doctrines of Reinhardt and Frerichs, which we endeavoured to refute on a former occasion, find no advocates within the walls of Guy's Hospital.

Dr. Wilks makes it one of the main objects of his communication, to draw a broad line of distinction between the two great classes of Bright's disease—the one being characterized by the "well-known large white kidney," and the other by the "small contracted kidney." With reference to this point, he observes:

"Finding that Frerichs mentions these two conditions as merely stages of the same morbid process, I have been anxious to find his proof, and have failed. Of course, he could not show that any morbid condition of an organ had been preceded by a different morbid condition; but this he might have done—he should have attempted to show that the symptoms, so generally connected with the former, have existed in the cases where the latter had been found. If this were generally, or occasionally, the case, there might have been some ground for believing in the identity of the two; but the opposite is the fact, that, in cases where the small contracted kidney is found, no symptoms of acute dropsy or of an inflammation has existed, such as is found in connexion with the other variety of kidney."

* "But none are so troublesome as the idols of the market, which insinuate themselves into the mind, from the association of words and terms. For though men believe that their reason governs words, it also happens that words retort and reflect their force upon the understanding." (Novum Organum.)

The case thus broadly stated is undoubtedly true in so large a number of instances, as should suffice to convince an unprejudiced inquirer, even without the help of other evidence. We think, however, that in this, and in some other passages of his paper, Dr. Wilks has made the distinction between the clinical history of the two classes of cases more absolute than it actually is. The large white kidney is not unfrequently a chronic disease, commencing insidiously, without exposure to cold or the previous occurrence of inflammatory symptoms, and sometimes even making considerable progress before the occurrence of dropsy excites a suspicion of renal disease. Then, on the other hand, we have seen more than one case of contracted kidney in which we had good evidence that the commencement was an attack of acute dropsy, occurring in a person previously healthy. Again, it must be borne in mind that, before the termination of many cases in which the small contracted kidney is found after death, the dropsy is as great and as general as in any case of the large white kidney. The chief distinction between the two classes of cases is this—that whereas, according to our experience, the large white kidney *never* proves fatal without the previous occurrence of dropsy—which is, in fact, usually one of the most prominent and distressing symptoms—the small contracted kidney, in many instances, proceeds to its extreme limit of degeneration, and at length destroys its victim without giving rise to dropsy in any form or in any degree: a fact which we hold to be totally irreconcilable with the hypothesis which assumes that the small contracted kidney is only a more advanced stage of the large white one.

An examination of the urine, as to quantity and quality, in the two classes of cases, affords the means of explaining the much more frequent occurrence of dropsy in connexion with the large white kidney than in cases of the small contracted kidney. In the former class of cases, the urine is almost invariably less copious than in health, and it contains a large amount of albumen; whereas, in cases of the small contracted kidney, the quantity of urine, as a rule, is considerably above the normal standard, while its albuminous contents are much less than in the other class of cases; so that the quantity of albumen which is drained off from the blood is widely different in the two forms of disease, and this not only relatively to the quantity of urine, but absolutely as to the amount of albumen discharged in the twenty-four hours. Now, clinical observation is continually teaching us that the risk of the occurrence of dropsy is, *cæteris paribus*, in inverse proportion to the quantity of urine secreted, and in direct proportion to the impoverishment of the blood, which is occasioned by the escape of its albumen through the kidneys. In accordance with this general statement is the fact that, in the advanced stages of the small contracted kidney, a sudden diminution of the quantity of urine is a frequent precursor of dropsy, and of a speedily fatal termination.

With reference to the character of the urine in the two classes of cases, one point of distinction is to be found in the *density* of that fluid. The urine secreted by the large white kidney has a density rarely below 1·015, and it ranges from that point to 1·025, or even as high as 1·030; while in cases of the small contracted kidney the density is more frequently below than above 1·015, varying from this point to 1·010, or even as low as 1·005. The high density in the former class of cases results from a scanty secre-

tion of liquid, with an abundance of albumen, and a more or less efficient excretion of the normal constituents of the urine; while the comparatively low density in the other cases is chiefly due to the excess of liquid. It is obvious that the actual amount of urinary solids eliminated within a given time may be greater with a copious secretion of urine of low specific gravity than when the density is high, but the measure in a more than corresponding degree scanty.

Proceeding further with this investigation, we shall find in the pathological anatomy of the kidney some explanation of those remarkable differences in the quantity and quality of the urine to which we have referred as being characteristic of the two great classes of Bright's disease.

One of the most remarkable points of difference between the two forms of disease, is to be found in the condition of the epithelial lining of the uriniferous tubes. In that form of disease which leads to the small contracted kidney, the gland-cells become disintegrated, detached from the basement membrane, and finally washed out with the urine. The tubes are thus either entirely denuded, or they are found to be lined by a layer of delicate cells, entirely different from the normal epithelium. It appears in the highest degree probable that a tube in either of these conditions, while it has lost its power of secreting the solids of the urine, may yet retain that of separating the watery constituents from the blood; and that in this pathological fact is to be found the explanation of the abundant flow of urine, pale in colour and low in density. Thus much is, at any rate, certain, that with respect to the condition of the tubes and their epithelial lining, the small contracted kidney stands in the most marked contrast to the large white kidney. In the last-mentioned form of disease, the gland-cells are never so detached, disintegrated, and swept away as to leave the tubes denuded. They remain adherent to the basement membrane, and undergo changes more or less considerable in different cases, varying from a slight granular opacity to a complete oily degeneration, or they become replaced by an albuminous or fibrinous material, which more or less fills the tube. In this form of disease, then, that condition of the tubes which we have referred to as being apparently so favourable for the transudation of water, is entirely wanting.

The large amount of albumen secreted by the large white kidney, and the comparatively small quantity separated by the contracted kidney, is probably in great measure dependent on the relative degrees of vascularity of the gland in the two forms of disease. In the large kidney there is often an excess—an actual hypertrophy—of glandular tissue; and the number of pervious bloodvessels, if not greater than in the healthy kidney, is rarely in any considerable degree less than normal. In the contracted kidney the opposite condition is found; for as the disease advances, many of the uriniferous tubes shrink, and the vessels which supply them—both arteries and Malpighian capillaries—may be seen to have their canals obstructed, and their walls covered with oil globules. To such an extent does this proceed, that in the later stages of the disease the kidney is reduced to the condition of an organ but scantily supplied with blood; and, obviously, in the same proportion the materials for a copious secretion of albumen are wanting.

More than once in the course of his paper, Dr. Wilks alludes to the

fact of the urine being entirely free from albumen in cases of renal disease, and he states that he has frequently found it so "in the chronic degeneration;" that is, as we understand him, in cases of the small contracted kidney. This statement, unqualified and without explanation, appears to us calculated to mislead. It has frequently been asserted by authors of deservedly high repute, that the occasional absence of albuminuria is a fact of not uncommon occurrence in the *advanced stages* of this form of disease. Our own experience leads us to doubt the accuracy of this statement, and to class it with those traditional errors—by no means uncommon in the science of medicine—which have passed current from one writer to another without having been subjected to a sufficiently rigid scrutiny. Although in the later stages of this form of disease the absence of albumen is an occurrence which we do not remember to have observed in a single instance, yet in the earliest stages a microscopic examination of the urine not unfrequently affords unequivocal evidence that the morbid change in the kidney has commenced and is in progress, while as yet the most careful examination can detect no trace of albumen in the urine. With reference to this point, then, we believe the truth to be that the urine may contain no albumen until the renal degeneration has made considerable progress; that in the more advanced middle stages the quantity of albumen is in proportion to the activity and rapidity of the disease; and that, again, in the latest stage of the renal degeneration the albumen is present in less quantity, for the reasons before stated. If during this period of the disease the urine be at any time entirely free from albumen, this is unquestionably an event of rare occurrence.

And here it may not be out of place to observe that, for the detection of minute quantities of albumen, considerable attention and care are necessary to avoid some sources of fallacy. It should be borne in mind, that heat is decidedly a more delicate test than nitric acid, when the urine is very slightly albuminous. A quantity of albumen, so small as to be readily decomposed by nitric acid, will be easily detected in *acid* urine by the careful application of heat. After coagulation by heat, a few drops of acetic acid should be added, to avoid the error of mistaking a sediment of earthy phosphate for coagulated albumen. If a drop of nitric acid be added for the same purpose, this is best done after the urine has been allowed to cool, lest, at a high temperature, a scanty coagulum of albumen be decomposed by that acid. Within the last few days, we nearly failed to detect the presence of albumen under the following circumstances. The urine was acid, and when boiled, it deposited rather copiously a white flaky precipitate; on the addition of nitric or acetic acid, this was rapidly dissolved, and we concluded that the sediment had been saline, and not at all albuminous. On microscopical examination, the urine was found to contain a considerable number of tube-casts, and this discovery led to a repetition of the tests for albumen, when it was ascertained, that although the sediment caused by boiling was in great part phosphetic and dissolved by acids, there yet remained a scanty, though unequivocal, albuminous coagulum, which was insoluble both in acetic and nitric acid. The urine is occasionally rendered turbid by the admixture of leucorrhœal or other discharges. The detection of a scanty albuminous coagulum in such a specimen will be much facilitated either by filtering

the urine before the application of tests, or by allowing it to remain at rest until the deposit of extraneous matters has cleared the supernatant liquid, which may then be submitted to the tests.

We have hitherto spoken of two forms of Bright's disease, and we have indicated some of the principal features by which they are characterized and distinguished. With regard to the question of a more minute classification, our author expresses himself dissatisfied with all attempts which have hitherto been made in that direction. He, however, is not prepared with any subdivision or nomenclature which he can recommend as being more worthy of general adoption than those which have been proposed by others. The term "coarse," which he uses to express certain external appearances of the kidney, is too vague and ill-defined to have any value beyond the post-mortem room of Guy's Hospital; and the "fibrous degeneration" of which he speaks as a fourth form of disease, is, to say the least, so rare an occurrence, that it may conveniently be excluded from consideration, until we can agree as to the nature and the name of some morbid conditions of far greater frequency and importance.

Now, it is an essential element of success in any undertaking, that we should have a clear and definite notion of the object which we desire to accomplish; and just in proportion to the doubt and confusion which may exist as to this point, will be the risk of the failure, which results from our efforts being misdirected. With reference, then, to the subject which we have now under consideration, it is important to determine for ourselves what is the object which we seek to attain by a subdivision of the forms of Bright's disease, beyond that into the "large white kidney," and the "small contracted kidney;" this primary distinction being one respecting which we agree with our author, in thinking there can scarcely be a reasonable doubt.

In the first place, it should be clearly understood, that the term *specific*, in the sense in which that term is defined by zoologists and botanists, is not applicable to any of the distinctions between the different forms of Bright's disease. There are some diseases which, with strict propriety, are called specific; inasmuch as not only are they distinguishable from other diseases by well-defined and easily-recognised characters, but they have also the power of perpetuating the species, by the reproduction of disease, in all essential respects identical, in the bodies of other living beings. As illustrations of this class of diseases, we need only mention measles, scarlatina, and small-pox, amongst acute diseases, and syphilis amongst chronic maladies. Now, in this sense of the term specific, it might be a question how far it is strictly applicable to many forms of general and local disease, the distinction and the definite nomenclature of which is yet a matter of much practical importance. With respect, however, to the forms of Bright's disease, there can be no question that the term specific is inapplicable, and the attempt to apply it in this case would be only to introduce a new element of confusion, without the prospect of any compensating advantage.

It being, then, admitted that the forms of Bright's disease are not specifically distinct from each other, it remains for us to determine in what way, and for what purpose, their subdivision is to be effected.

Suppose now the following statement to be a correct expression of

facts—that there are certain morbid conditions of kidney, readily distinguishable from each other by a post-mortem examination—that by a microscopical and chemical examination of the urine these several morbid conditions of kidney may be recognised and distinguished, during the lifetime of the patient, with scarcely less ease and certainty than when the diseased organ itself is exposed to view—further, that certain of these morbid states are of far more serious import than others, either as being originally and essentially more formidable in character, or as indicating a more inveterate and advanced degeneration of tissue.

If it should be found, upon careful inquiry, that these propositions can be admitted as established facts, then, whatever may be said by the mere morbid anatomist, who breathes only the atmosphere of the dead-house, we are confident that the pathologist and the physician will at once recognise the importance of distinctions which may afford valuable aid in the diagnosis and prognosis—nay, even in the treatment—of renal disease. It is not our intention now to enter at length upon the consideration of the various forms of Bright's disease—we have fully discussed that subject elsewhere—but in adverting to some points of difference between Dr. Wilks and ourselves, we shall briefly state certain facts which must, as we believe, form the basis for the subdivision and the nomenclature of the various forms of the disease in question.

Dr. Wilks, in commenting upon some terms which have been proposed by the author of this review says:—"the terms [*acute and chronic desquamative nephritis*] convey some meaning, and some real facts; but if the former apply to an acute inflammation of the tubes of the kidney, and the latter to a chronic inflammation, these expressions would be better." Now, that the term acute inflammation is not sufficiently precise will be evident from a consideration of the following facts. Two cases of dropsy after scarlatina present themselves to our observation. In their general features both cases are alike; in both the urine is scanty and highly albuminous; but on a microscopical examination, we find in one case that the urine contains, besides blood-corpuscles and tube-casts, numerous cells of renal epithelium, partly entangled in the casts and partly scattered through the urine; in the other case we find that the tube-casts entangle, not epithelial cells, but pus-corpuscles, many of which are also scattered over the field of the microscope. Now, this remarkable difference in the microscopical characters of the urine in the two cases is not accidental but essential; and if we watch these urines from day to day, we shall find that they retain their distinctive characters: only, during the progress of the disease, undergoing certain modifications which we need not now particularly describe. There probably are few pathologists who would hesitate to give a distinctive appellation to two forms of disease thus characterized; and the physician finds an additional motive for distinguishing them, in the fact that one form of disease is much less curable, and much more frequently fatal, than the other. Now, in both sets of cases there is what may be called "acute inflammation of the tubes of the kidney;" and in order to express this fact, and also to distinguish one form of inflammation from the other, we have proposed to call one "*acute desquamative nephritis*," the other "*acute suppurative nephritis*." In short, to the expression which Dr. Wilks considers alone sufficient,

we prefix another, for the sake of marking a real and a very important distinction.

In a third case of dropsy after scarlatina, the urine may present appearances different from those in either of the cases to which we have just now referred. The urine is scanty, and highly albuminous; it deposits little or no sediment, and on microscopical examination it is found to contain neither pus nor renal epithelium, there being perhaps only a slight cloudy deposit, composed of transparent fibrinous casts, the small diameter of which is evidence that they have come from tubes which retain their epithelial lining, the casts having been moulded within that space in the tubes which is immediately bounded by the gland-cells. Now, again, this character of the urine is not accidental, nor does it indicate an earlier stage of either of the two conditions which we have before described; it usually remains the same from day to day, and if the case terminates fatally, as these cases not unfrequently do, there is found, as might be expected, an intimate relation between the condition of the kidney and the characters of the urine. The kidney-tubes are not filled with desquamated epithelium, as in the first case, nor transformed into pus, as in the second, but they retain their position in the tubes, only being more opaque, granular, and bulky, than in the normal state; and this slightly altered condition of the epithelium, with some opacity of the Malpighian capillaries, and here and there, perhaps, a tube containing extravasated blood, constitutes the entire morbid anatomy of this form of disease, which it has been proposed to call *non-desquamative*. We are not prepared to maintain that this term is better than any other which might be proposed; but it is certainly expressive of a fact which is of essential importance in the pathology of the kidney—namely, the absence of epithelial desquamation. It appears to us that if a name can be made to express the most important feature of the morbid condition to which it is applied, this is as much as can be expected; since it is not possible to condense into one or two words the entire history of a disease.

Our limits will not permit us now to enter upon the question of the proximate cause and the pathology of the desquamative process in the kidney; but this point we beg to submit for the consideration of our readers, with the hope that they will be induced to test the accuracy of our statement—that the acute desquamative disease is in general a much more favourable form of disorder than either the non-desquamative* or the suppurative. Happily, too, it is the most frequent, not only in cases of dropsy after scarlatina, but in all cases of acute renal dropsy, from whatsoever cause arising.

Now, if we do not mistake the language of Dr. Wilks, he objects to any system of nomenclature for renal disease which dissevers “scarlet-fever dropsy cases.” Such an objection, it appears to us, can only be based upon a misapprehension of the object for which cases of Bright's disease may usefully be subdivided. If our subdivisions and our nomenclature are to have reference, not to the pathological conditions of the kidney

* The comparison between the desquamative and the non-desquamative process can be fairly instituted only with cases in which the urine is equally and copiously albuminous. There are some cases of slight renal congestion in which, with a small quantity of albumen in the urine, there is neither desquamation nor sediment of any kind; these cases are, obviously, not comparable with the more severe forms of disease.

itself, but to the nature of the constitutional disorders which give rise to the renal disease, the result will obviously be, that under one name—of scarlet-fever kidney, for instance—will be included very different morbid conditions; while to pathological states of the kidney, which are essentially alike, would be applied different names, inasmuch as the same form of disease in the kidney—for instance, acute desquamative disease—may result from scarlet fever, from measles, from erysipelas, from exposure to wet and cold, and from various other causes. And, be it observed, we do not find a sufficient reason for withholding from the various morbid states of kidney to which we have referred, or shall hereafter refer, a distinctive appellation, in the fact, that one condition may sometimes be seen to pass into the other, or that two morbid conditions may coexist in the same kidney. If this were a sufficient reason, then it would apply with at least equal force in the case of diseases of the liver or lung, most of which might, on such grounds, be included, without further distinction, under the comprehensive terms, hepatic or pulmonary disease.

With respect to the chronic varieties of Bright's disease, we have proposed to call that form of disease which results in the small contracted kidney, *chronic desquamative disease*—not, as Dr. Wilks repeatedly states, *non-desquamative disease*.^{*} The name is expressive of a process which is a constant feature of the disease—namely, a continual scaling off and crumbling away of the renal epithelium, which the urine, after standing for a short time, deposits as a white sediment. On microscopical examination, the disintegrated epithelial fragments may be seen partly scattered, and partly in the form of granular tube-casts. These tube-casts are of great diagnostic value, for they are often present, and indicate the existence of commencing renal degeneration, before the occurrence of albuminuria; and, at a later stage, their number affords a tolerably accurate index of the rate at which the disease is making progress. We gather from Dr. Wilks's general remarks, as well as from the brief reports of his cases, that he finds no microscopic sediment in the urine in connexion with the small contracted kidney. This is a point which we have examined with much care in a large number of instances, and it will be seen that the result of our experience, being such as we have just now stated, is in direct opposition to that of Dr. Wilks.

The cases of chronic disease in which there is either no sediment or only a light cloud, which subsides after the urine has remained for a few hours at rest, are those to which the term *chronic non-desquamative disease* are strictly applicable. The urine is of comparatively high density, and contains an abundance of albumen, and the kidneys are large and pale. The cloudy sediment in the urine contains small wax-like fibrinous casts. In some cases, the kidneys undergo a further change; the epithelium, in certain sets of tubes, suffers a fatty transformation; this change is indicated in the urine during life by the appearance of oil in the cells and casts, and in the kidneys, after death, by the peculiar yellow granulations, like small specks of atheroma, which are scattered through the cortical substance. If any term more expressive than that of *granular*

^{*} Thus, at p. 303, Dr. Wilks says, "Surely an organ which has undergone such a change or degeneration as we witness in the small granular kidney, deserves a more significant name than the negative one of non-desquamative." Here the negative is introduced by Dr. Wilks, and his criticism is so far misapplied.

fat kidney can be suggested for this form of disease, we shall very gladly adopt it in place of that by which we have proposed to distinguish this from other forms of "the large white kidney."

To recur once more to the end and object of these subdivisions: Why—it may be asked—do you make a distinction between the chronic non-desquamative disease, as you are pleased to call it, and the granular fat kidney, since you admit that the latter is, sometimes at least, only a more advanced stage of the former? Our reply is, that the distinction affords an important aid to the physician in the formation of his prognosis, and therefore, sometimes, in the choice of remedies. Two cases of dropsy, in all their outward appearances alike, may come under observation. In both the urine is of nearly the natural colour, but highly albuminous; both specimens deposit a light cloud after standing, and this cloud, in both instances, contains small, transparent, waxy casts. The important distinction is this—that while, in one case, the fibrinous casts contain only here and there a fragment of epithelium; in the other case, a large proportion of the casts entangle oil, partly in the form of scattered globules, and partly contained in modified epithelial cells. In the first case, a cure must be perseveringly attempted, and may, in fact, be reasonably expected; in the second instance, recovery is scarcely to be hoped for. From the first condition, we know of numerous and complete recoveries; from the second, hitherto, not one. We would remind our readers that often, during the convalescence from acute desquamative disease, a certain amount of oil appears in the urine, but the history and the associated appearances in the urine will serve to distinguish these cases from those of confirmed fatty degeneration of the kidney.

There are few subjects of inquiry more interesting than the question, as to the possibility of tissues which have undergone fatty degeneration being restored to their normal condition. It appears that, in a large proportion of fatal cases of delirium tremens, the muscular substance of the heart has undergone more or less of fatty transformation. The question then arises, may not the hearts of some of those who recover from that disease have been for a time in the same condition of fatty degeneration? Or may it be that, upon the presence or the absence of this structural change in the heart—obviously a very serious complication—depends the curability of delirium tremens? It is evident that the difficulty in the way of giving a definite answer to these interesting queries arises from the fact, that we have no means by which to determine with certainty, during the patient's lifetime, either the presence or the absence of fatty degeneration of the heart. Now this difficulty does not exist with regard to that form of fatty degeneration of the kidney to which we have alluded, under the name of *granular fat kidney*. And those who are desirous to observe the commencement and the progress of fatty degeneration, with a view to determine the extent to which it may proceed before it becomes irremediable, can find no field of observation so favourable for the study as the urine of patients who are threatened with this form of disease. We do not imagine that fatty degeneration of muscle is identical with the similar change in glandular tissue, but there must be an analogy between the two; and every well ascertained fact with regard to one may serve to throw light upon the other.

Returning now to our author, it may be observed that his account of the general history, the complications, and the treatment of Bright's disease, although containing little that is new, yet affords satisfactory evidence that he is making good use of the wide field of observation which is open to him at Guy's Hospital. We have marked a few passages for comment.

With respect to the prognosis in cases of the large white kidney, Dr. Wilks remarks,

"That if, after an attack of acute dropsy, recovery do not shortly take place, but after a lapse of time renal disease still exists, severe symptoms must again be speedily expected, and that three years will be the maximum of time in which the patient has to live."

We do not doubt that in the circumstances referred to many cases will prove fatal within a period of three years, but that a considerable number will live much beyond that period we can testify from our own experience. In the course of last year we attended a patient who died with Bright's disease, which originated in an attack of dropsy after scarlatina ten years before. The urine was albuminous during the whole period. Two years since we examined the urine of a medical man who had dropsy after scarlatina, sixteen years before. He recovered from the dropsy, and thought no more of his illness, until his urine was accidentally discovered to be albuminous five years afterwards by a fellow student. From that time to the period of his visit to us, his urine had not ceased to be albuminous, and it is probable, as he himself believes, that albuminuria had existed since the attack of dropsy. These cases will suffice to show that Dr. Wilks must extend his maximum period considerably beyond three years.

Our experience is quite in accordance with that of Dr. Wilks, to the effect that a complaint of *pain in the loins* is the exception in cases of Bright's disease. In some few acute cases, pain in the region of the kidney is severe, but in by far the greater number of chronic cases the patients not only assure us that they have no pain, but frequently have they urged this fact upon our attention, with the expression of a doubt whether an organ so entirely free from pain can be the seat of any serious morbid change.

With regard to the nature and origin of renal cysts, which often occur in connexion with the chronic desquamative disease, Dr. Wilks appears to be in the unhappy position of a man who has forsaken one creed before he has firmly grasped another. He has lost confidence in Mr. Simon's theory, that the cysts in question are modified epithelial germs; but his assent to the contrary statement, that they are merely altered portions of uriniferous tubes, is interfered with by the fact that many of the cyst-like appearances are much smaller than the normal size of the tubes. This apparent difficulty will be removed if he will take into consideration the fact that a wasted and contracted condition of the tubes is a more frequent result of the destruction of their epithelial lining, than the opposite state of dilatation. Dr. Wilks says:

"It has been taken for granted that the larger cysts found so often on the surface of the kidney are of the same formation as the microscopic ones. Of this there can be no positive proof."

We, on the contrary, are of opinion that the point in question is sufficiently proved by the fact, that the tubes may be seen dilated in every degree, from the normal size until they become visible by the unaided eye. There can be no other proof than this, but surely this is sufficient for the purpose.

Dr. Wilks lays much stress upon the existence of a rigid and tortuous condition of the arteries throughout the body as a concomitant of Bright's disease, and with reference to this subject we find the following passage:

"Dr. Johnson speaks of the renal artery being especially affected in Bright's disease, and caused by its increased efforts in the propulsion of blood through the Malpighian tufts. This I cannot admit from my own experience; I have very frequently found it diseased, but never without the same evidence of disease in other arteries. I am obliged, therefore, to put the disease of the renal vessel down to a general cause, and not arising necessarily from any proximity to the organ which is supposed to produce the mischief."

We gather from this passage, that Dr. Wilks has misapprehended the observations of the author whom he quotes. The observations in question relate, not to the trunk of the renal artery, as Dr. Wilks appears to suppose, but to the minute microscopic branches, the muscular coats of which, in the advanced stages of all forms of Bright's disease, are hypertrophied in a very remarkable manner. The change in question has nothing corresponding with it in the coats of the larger vessels. And whatever may be the correct interpretation of this peculiar condition of the minute renal arteries, the anatomical fact itself is so easily demonstrated by the aid of the microscope, that if it has not been seen, this can only be because it has not been carefully looked for.

Our author's remarks on treatment are judicious. He appears, however, to have a remarkable unwillingness to confine his patients to bed and within doors. He cites the case of a man with acute dropsy who, contrary to orders, went out—the weather, however, being warm—and rapidly recovered. "Very frequently," Dr. Wilks says, "I have seen patients with Bright's disease put to bed, and there have died, who I believe would have continued alive for a long time if they had had a moderate share of exercise." It is sometimes difficult to determine whether a patient at a particular period of his illness should be allowed to go out, but it is safer to err on the side of caution; and for every instance of a patient being benefited by disobeying our injunctions to remain at home, we could refer to at least three others who have thereby incurred a serious and even a fatal relapse. Dr. Wilks doubtless intended his observations to apply chiefly to cases of chronic disease, in the treatment of which exercise in the open air is often of service; but it appears to us that they are less guarded than is desirable, in reference to a point of practice about which both patients and practitioners are often more negligent than is consistent with safety.

And now, having referred to the chief points in Dr. Wilks's communication, and having criticised it, as we trust, in no unfriendly spirit, we may remark, in conclusion, that any one who determines to read the paper itself, may do so with the assurance that his time will not have been unprofitably bestowed upon it.

George Johnson.

REVIEW XI.

Sudden Death. By A. B. GRANVILLE, M.D., F.R.S., M.R.C.P. Lond., &c., &c.—London, 1854. 8vo. pp. 286.

ALTHOUGH this work is intended as much for the public as for the profession, its contents are in many respects such as to merit the attentive regard of the latter. Moreover, where it addresses itself to the former, our jealousy of popular semi-medical writings does not find grounds for objection so much in the topics as in the manner in which they are handled. The subject being such as does most seriously touch the dearest interests of all persons, medical or non-medical, the proverb, *homo sum nihil humanum alienum a me puto*, may be allowed to lay its weight in the popular scale, when we carefully balance the wisdom or the folly of addressing the non-professional public upon professional matters.

In laying the contents of this essay before our readers, we shall follow the author's division of his subject, which he introduces by a few preliminary observations upon the necessity that he conceives to exist for bestowing greater attention upon "head-diseases," which he opines have not heretofore received sufficient attention (!?). However, the deficiency is to be supplied at some future time by Dr. Granville. Let us, then, wait patiently for the fruits of Dr. Granville's experience, derived from "a career of thirty-five years in the metropolis." In the author's own words, the promised and the present work are to be received "as a respectful tribute of accumulated professional experience due by me to the public, from whom I have received every encouragement during a career, &c." We should have more cordially hailed the expression of such a tribute of gratitude, had it been made to the members of a *profession* who are competent to judge of medical facts and reasoning, rather than a public, who can equally admire homœopathy, mesmerism, table-turning, and spirit-rapping.

Pass we now to the book itself, in its own order of subjects. After some introductory remarks, which might have been omitted without detriment, Dr. Granville gives us a description of the arrangements of the office of the Registrar-General. From the records there to be found, Dr. Granville extracts several of the more striking incidents accompanying the returns of the district registrars, *more* Dickens.

Death statistics contain, first, the general mortality of all England, during the ten years elapsed between the two last censuses, with the corresponding mean population of each year, corrected by calculation. Secondly, the number of deaths, under the three heads of sudden, apoplexy, and paralysis, which have occurred in all England in the course of a period of three years. Lastly, the total number of the same kind of deaths that have taken place in the metropolitan division during the same periods, distinguishing the sexes. In this section, instances of sudden death are also quoted from the returns.

From the statistics of the Registrar-General, it appears that the total deaths in all England and Wales, in ten years, have been 4,220,723. The increase of population over deaths during the same period, 2,013,461. From the same records, Dr. Granville shows that a high mortality of one

year is counterbalanced by a low average of another, as it were, striking the average. Thus the mortality of London, in 1849, was 68,432; in 1850, it was 48,579; this mortality being inferior to that of each of the three years preceding cholera. The average is seen in the following statistics for the metropolis:

1847 and 1848—116,759 = average, 58,378.

1849 and 1850—117,011 = average, 58,505.

This average is found equally in a similar calculation for the whole of England. The fact is one of great interest, and Dr. Granville has our thanks for having brought it under our notice. It is to be regretted that the author should have connected these facts with such inferences as are exhibited in the following remarks, characterized, in our opinion, by bad taste.

“It were well that those philanthropists, who are running the *whole hog*, to use a vernacular expression,* with their theory of cholera being the offspring of filth and insalubrious localities, requiring large measures of Government interference, expensive Boards of Health, and extramural interments, should reflect on these statistical facts, and the conclusions to which they lead.”

Dr. Granville, not a little presumptuously, pretends to know that the ways of God are as our ways, or else it must be that he consoles himself with a sort of pseudo-Christian fatalism, under a dispensation of which himself holds the authentication. Thus we are authoritatively informed by him, that plagues are not sent “to depopulate whole countries, or to destroy mankind; for when the great plague has passed away, and on the following years ‘the tribes are again numbered,’ fewer than usual of the people are found to have died in that year, that the ‘mass of living flesh’ may continue the same. Such is the Divine Covenant.” We shall not follow the author in his criticisms upon the reports of the Board of Health—which hence are obviously not likely to find favour in his eyes. We have accompanied Dr. Granville already too far in a digression rather wide of his subject. Returning, then, to the matter in hand, we learn that the number of sudden deaths, as well as those from apoplexy and paralysis, for England and Wales, during the five years 1847-51, has been 92,774; out of which, 15,054 occurred in the metropolis. These numbers, if examined for a series of years, show a progressive increase in the deaths from these causes, over and above what might be expected from increase of population. These deaths are also more frequent in the winter and autumn quarters than in the spring and summer seasons. These statistical results resemble, Dr. Granville points out, those given in a former number of the ‘British and Foreign Quarterly Medical Review,’ as observed in Italy. It appears that the middle ages—i.e., from twenty-five to sixty, afford the greatest number of deaths from apoplexy. As regards sex, Dr. Granville is of opinion that sudden death from apoplexy is as frequent in females as in males, while paralysis is more frequently the cause of death in the former. The general result of Dr. Granville’s investigations under the two last heads is, that from infancy to manhood, or mature age, the deaths from these causes are fewest in number; that

* The italics are ours. We would remind the author that a wide difference exists between a slang, and a vernacular expression.

nearly double the number of men die suddenly from apoplexy at mature than at old age; that women and men die in almost equal numbers from these causes in the periods of mature and old age; and, that the only difference regards paralysis, which is more destructive to females, after sixty years of age, than to males.

Among other interesting deductions from the tables prepared by Dr. Granville, we find that there exist peculiarities belonging to locality, thus:—apoplexy and paralysis are more frequent in counties than in cities, and sudden deaths more frequent in cities than in counties. Of the *status* or condition in life of those who have died from these three causes, it appears that the greatest number have occurred among the wives or widows of the industrial classes; next, among domestic servants, cabmen, coachmen, porters, &c.; then, among labourers, as house-painters, plumbers, stonemasons, bricklayers, &c. Noblemen and independent gentlemen, it seems, are about level with tailors, bootmakers, hatters, &c. Many more facts equally important and instructive are contained in this section.

The "*Early destruction of life in England*," or, as we should phrase it, the destruction of life at an early age, is a fact well known to the medical profession, and here extensively illustrated by Dr. Granville. The returns in the registrar's office give the proportions of deaths as one-fourth for males and one-fifth for females, within the first year of life, excluding still-born and premature births. The proportions of births being 100 females to 105 males, the cause of this exemption in favour of female infants remains to be discovered. Infantile deaths occur in much greater number during the first thirty days of life, gradually decreasing as the first year approaches completion. The early destruction of life is greater in certain manufacturing than in purely agricultural districts, and in the latter than in the metropolis; and again, it is greater in sea-ports than in other towns. The causes of these early deaths are unsatisfactorily explained in the coroners' verdicts appended to the registry, in a large number of instances. In a vast number, the children, as Dr. Granville observes, have been illegitimate. So frequent are these cases, that the coroners, the author remarks, appear compelled to employ some set expressions, without attempting to explain the causes in the description of death under consideration.

That the crime of infanticide has greatly increased of late years, the records of coroners' courts, assizes, and criminal courts only too amply testify. We have ourselves endeavoured to learn, from the records of the metropolitan police-courts, the exact amount of increase in this crime; owing, however, to absence of a classification of this from allied offences, we have not succeeded. Much of the increase in question may doubtless be traced to the absurd and unscientific state of our law of infanticide, which requires proof of the commission of the crime after entire birth and separation of the infant from its mother. Under these conditions, it is well known to coroners that cases in which all evidence brings home the perpetration of the crime to the real culprit will, nevertheless, in the criminal courts, obtain acquittal. Hence often the indefinite character of the verdicts of coroners' juries. The causes assigned for infantile death are, we agree with Dr. Granville, often ridiculously insufficient.

Still-births are omitted from the registration returns. The number of deaths occurring at each of the first three months are not specially noted, but included in one general enumeration. Dr. Granville follows the number of early deaths through the variations attending locality, status of inhabitants, &c.; and a painful impression is left upon the mind by the perusal of the details adduced, as to the fearful demoralization and indifference to human life manifestly prevailing among the inhabitants of some districts in Great Britain—not in China, but in the British metropolis. The infantile mortality of Bethnal Green is to that of St. George's, Hanover-square, as $6\frac{1}{8}$ to 1!! Is comment needed? Still more solemn is the lesson inculcated by the state of things in Preston, up to a very recent date, as set forth by Dr. Granville.

In this section, the author has taken the opportunity of offering suggestions with regard to the registration of deaths, more particularly of those under one year, which, if acted upon, would tend to the enhancement of the value of the already highly-valued documents of the Registrar-General's office.

"Frequency of sudden death."—In order to impress his readers with the truth of his statements, and to fix their attention upon the subject, Dr. Granville has brought together a series of *"Facts."* These facts however consist of what appear to us to be newspaper paragraphs and obituaries, recording sudden deaths; and which, by dint of scissors and paste, might have been almost indefinitely added to, including other equally impressive and striking occurrences.

"What is sudden death?"—To give the answer to his question imposes upon the author the necessity of discussing the preliminary question, "What is life?" In so doing, Dr. Granville criticises the principal doctrines of vitality, the definition adopted by himself being that "Life is the communication of an immaterial principle to the organized being, forming no part of it, but simply using it as a machine subordinate to its will."

The answer to the first question, "What is sudden death?" is promised to be more fully given in a future work, which shall treat practically of the causes, and their treatment, and the prevention of sudden death. For the present, Dr. Granville is content with the statement that there is no such thing as *sudden* death, except as the closing scene of a series of pathological changes going on for a longer or shorter period of time.

The conclusion of the present volume consists of a summary of what is contained therein, for the purpose of putting on their guard those individuals who are "likely to be the designated next victims, by showing, 1st, That in our days apoplectic seizures, &c., are more common than in former times. 2ndly, That neither youth nor manhood goes free, but rather the contrary, from such calamities. 3rdly, That these are not confined to the larger masses, but reach the home of the better and most exalted. Lastly, That circumstances attending the sudden deaths of infants are brought to light which demand further investigation."

REVIEW XII.

1. *Bericht über die elektro-physiologische Arbeiten des Dr. DUCHENNE, de Boulogne, zu Paris.* Von Dr. HERMANN EBERHARD RICHTER. ('Schmidt's Jahrbuch,' No. 11, 1853.)
Report upon the Electro-physiological Researches of Dr. DUCHENNE. By Dr. H. E. RICHTER. ('Schmidt's Jahrbuch' for October, 1853.)
2. *Die Electricität in ihrer Anwendung auf practische Medicin.* Von Dr. MORITZ MEYER, arzt in Berlin. 1854.
Electricity in its Application to Practical Medicine. By Dr. MORITZ MEYER, of Berlin. 1854.
3. *Histoire de l'Electricité Médicale.* Par M. J. GUITARD, M.D., Président de la Société Médicale d'Emulation de Toulouse. 1854.
History of Medical Electricity. By M. J. GUITARD, M.D., Toulouse. 1854.
4. *De l'Action Thérapeutique de l'Electrisation Localisée dans le Traitement des Para'ysies Consécutives à l'Hémorrhagie Cérébrale.* Par M. le Dr. DUCHENNE, de Boulogne. ('Bulletin Général de Thérapeutique Médicale et Chirurgicale,' Mars et Avril, 1854.)
On the Therapeutic Action of Topical Electrification in the Treatment of Paralysis, consecutive to Cerebral Hæmorrhage. By Dr. DUCHENNE.
5. *Heilung der Impotentia virilis, mittels Electricität.* Von Dr. B. SCHULZ, in Wien. ('Wien Wochenschrift,' 10—12. 1854.)
On the Cure of Impotence by means of Electricity. By Dr. B. SCHULZ, of Vienna. ('Schmidt's Jahrbuch,' August, 1854.)

ALTHOUGH Scribonius Largus attempted the cure of gout and headache by placing the feet of his patients in a bucket of torpedos, and Paracelsus recognised some therapeutic properties in the magnetic bar, it was not until the middle of the last century that electricity was employed with any zeal for the cure of disease. The discovery of the Leyden phial at this time gave fresh impetus to the application; by its aid success was obtained beyond the most sanguine hopes; anticipations were raised; great pretensions made, and followed by still greater failures; until what was wanting in fact had to be supplied from fiction; and electricity, in its therapeutic relations, passed from the earnest inquirer after truth, through the hands of the enthusiast, into those of the most heartless charlatans.

Falling into the same category of disgrace with the talisman, amulets and sympathetic cures, its scientific history was for some time lost; and nothing was heard of it, beyond vague rumours of miraculous cures from the shady regions lying outside the circle of sincere investigation and legitimate therapeutics.

In an earlier number of this review,* attention was directed to the researches of Mr. Donovan, Dr. Golding Bird, and others, who had submitted the claims of electricity to renewed examination. It was then shown that many cases of chorea, paralysis, and of amenorrhœa were amenable to this form of treatment, when others had proved unsuccessful.

* Vol. iii. p. 373. 1849.

Since that time, scarcely anything of importance has been added by the physicians of our own country; but in France and Germany there has been great attention bestowed upon the subject; and if no new facts have been added, the real use of electricity has been established, the several methods for its application have been tested, and their relative value pointed out. M. Duchenne (of Boulogne) has been the most vigorous labourer in this field, and it is our special object at the present time to present the results of his inquiries. Before doing so, we have to speak highly of the history furnished by M. Guitard, to which we are indebted for some of the following information.

After the discovery of the Leyden phial, and before the time of Volta, i. e., from the year 1745 until the commencement of the present century, friction-electricity was the only form used in medicine. Sparks drawn from the "prime-conductor," or from the individual, placed upon an insulated stool; shocks from the Leyden-jar; the "electric aura," from a pointed wire; and the so-called "electric bath," were the methods employed. M. Jallabert (1748) published a cure of paralysis with atrophic muscles, affecting the right arm. He recognised as results of electric stimulation—acceleration of the pulse, increase of temperature, involuntary contraction of the muscles, and emmenagogue properties.

M. Jallabert's method was the application of sparks to points of the skin corresponding to the attachment of muscles; and in the *Encyclopædia* of Diderot and d'Alembert (1777) this mode of exhibition is considered to account for Jallabert's success. M. l'Abbé Sans (1772) related several cases of hemiplegia, and considered electricity the only means calculated to effect a cure in paralysis of long duration.

In 1780, M. l'Abbé Bertholon published an elaborate treatise upon the electricity of the body in health and disease, in which those conditions are referred to its equilibrium on the one hand, or to some variable kind of inequality on the other. Every sort of malady is, according to this learned Abbé, amenable to electric treatment, if the physician would only find out the form of electricity required.

We view with irresistible distrust the statements of a man, who treats successfully with the same remedy all classes of disease; and finding that the Abbé Bertholon cured affections of the skin, fevers, inflammations, cachexiæ, convulsions, and paralyzes with almost equal rapidity and ease, we pass on to the less ambitious but more careful memoir of M. Mazars de Cazèles. In this are related the cures of twenty cases, consisting of chronic rheumatic affections, hemiplegia, general paralyzes, and neuralgiæ. In a second memoir by the same author, similar reports are furnished of forty-six cases; and M. Sigaud de la Fond confirms their general truthfulness by the detail of his own success in analogous affections. The latter author sums up the *modus operandi* of electricity by affirming that it augments transpiration, saliva, and the alvine secretions, causes abundant deposit in the urine, increases the temperature of the body, and improves its nutrition. In 1785, Cavallo published a general treatise, collecting the observations of previous experimentalists, and bearing his own testimony to the value of electrical applications for the increase of secretion generally, and in the treatment of skin diseases.

After the discoveries of Galvani and Volta, Valli was the first to apply

the new form of electricity (galvanism, or contact electricity), for the restoration of those who were in trance, or apparently dead from other causes. Sömmering pointed out the region of the phrenic nerve as the most advantageous spot for its application with this end in view. Pfaff, Reil, Humboldt, Aldini, and others announced the utility of galvanism in the treatment of paralyses of motility; Schaub, Eschke, and others, in loss of sensibility; and Sarlandière, by means of electro-puncture, made its application possible to deeper organs. Since Professor Faraday's discovery of induction-electricity, the older methods of electrifying have been almost entirely discarded, and it is with this mode of operation, now commonly called "Faradisation," that Duchenne and others have accomplished their results. There are, then, three generic forms of electricity, which may each be applied in different manners; and Duchenne has the following comments upon their relative value.

The action of *electricity* by *sparks* is always limited to the skin, or to some superficial muscle. The Leyden phial causes energetic contraction of the muscles, and some commotion of the nervous centres, with a painful feeling of shock, or of contused nerve. The *galvanic* current may be either interrupted or continuous; the latter acting only upon the skin, and producing, in proportion to its strength, either simple erythema, or an eschar. The *induction* current is, from its very nature, of momentary existence only, and, although the rapidity with which its intermittences take place may be increased or diminished at will, a continuous stream is impossible. A current of the first order acts directly upon the muscular contractility; the secondary current excites powerfully the retina and cutaneous sensibility.

The *therapeutic value* of these three forms is various. Electric sparks afford a convenient mode of stimulating the skin. They are too feeble for action upon the muscles, and the Leyden phial produces too great commotion of the central organs. The continuous current from galvanism is of service when it is desirable to obtain thermic or chemical effects; and it is the best form of application to the retina. For action upon the muscles, the induction-apparatus is the best. Its special value appears to be, that its intensity can be regulated with great precision; that it can be directed upon any organ, and limited to that organ; that it exerts no appreciable chemical action on the tissues, and leaves no textural change; that it causes less burning sensation on the skin, causes and leaves less pain in the muscles, and rarely induces disturbance of the nervous centres. Duchenne's apparatus consists of an induction-machine attached to one of Bunsen's carbon-batteries; and his special improvements are the "Graduator," a well-adjusted arrangement, by which the force of the current may be increased or diminished at will; the "Rheometer," which enables the operator to measure the force he is employing, and consequently to enhance the value of the Graduator; and the "Rheophors," or handles for applying the current. The whole apparatus is neatly arranged in a case, and may be procured of Charrière, Rue de l'Ecole de Médecine, No. 6, for 160 francs.

With regard to the *mode of applying* the conductors, Duchenne has made some important observations. If they are dried and placed upon the dry cuticle, not far from one another, the action is confined to the

surface. If both cuticle and conductor are well moistened (his own method being the attachment of wet sponge to the ends of the conductors) there is no action upon the skin, but marked contraction of the underlying muscles, accompanied by a very peculiar sensation like that caused by placing the conductors (through a wound in the cutis) upon the muscles themselves. The muscles may be reached, directly, by placing the moistened conductors over them; or indirectly, by placing the conductors over the trunk of their nerve. The latter method is not so precise as the former, but is of use when anæsthesia is to be combated. The sensitiveness of the skin varies widely in different situations, and the same is true not only of the sensibility, but also of the contractility of the muscles. Hence, Duchenne observes, it is necessary to become acquainted with these differences in order to administer the proper dose to a particular muscle, or group of muscles.

Some individuals are much more influenced than others, and unpleasant disturbance of the nervous centres may be induced, such as vertigo, confusion of thought, &c., without any local sensations from the application.

The *purposes* for which Faradisation may be employed are the advance of physiology, pathology, and therapeutics. The great utility of this agent in *physiology* is in establishing the functions of particular muscles and nerves. Every voluntary movement of a muscle, or group of muscles, is complicated by the involuntary contraction of their antagonists, a provisional association to insure firmness and stability in the limb; and farther, it is impossible to determine the contraction of any particular muscle by a direct effort of the will; we can determine only the kind of movement, which shall be the "end" or resultant of muscular combinations; and the production of these combinations is the function of some part of the nervous system independent of volition. Duchenne imagines that by accurate local Faradisation many errors in our ideas with regard to the mechanical uses of particular muscles may be corrected; and this may be the case, but it must require the practised hand of M. Duchenne himself to single out deeply situated muscles, and determine their special contraction apart from those which closely surround them. Richter gives an interesting account of some conclusions to which Duchenne arrived, and the aid which he received therefrom in directing his local Faradisation for the treatment of certain local spasms and paralyses.

Some of the most interesting conclusions relate to the complicated movements of the scapula, which cannot be described without much detail, or the aid of plates. With regard to results more readily appreciable, we may mention that Duchenne considers the zygomaticus major the muscle which gives the special expression of laughing; the zygomaticus minor, that which gives the appearance of crying; that the musculus extensor digitorum communis, musculus extensor pollicis longus, and the musculus extensor digiti minimi, extend only the first phalanges of the fingers and thumb, while the abductors, adductors, interossei, and lumbricales effect extension of the second phalanx of the thumb, and of the second and third phalanges of the fingers; that the musculus flexor pollicis brevis is a flexor of the first phalanx of the thumb, but to a higher degree an extensor of its second phalanx; and that the musculus supinator radii

longus is not a supinator, but a pronator of the forearm. As to nerves, Duchenne thinks that he has made out satisfactorily the function of the chorda tympani—viz., that of conferring sensibility, and the sense of taste, upon the anterior two-thirds of the tongue.

In its relation to *pathology*, electricity appears to be of service in diagnosis and therapeutics. Neither Duchenne, Meyer, nor Guitard add anything of importance with regard to the former, although they speak very confidently, and, it must be added, somewhat contradictorily upon the subject. Dr. Marshall Hall pointed out, many years ago, that when paralysis was of such a kind as to sever muscles from the influence of the spinal cord, their irritability to stimulation from the galvanic trough was less than the irritability of muscles still in functional connexion with the cord. The use of the term "spinal" to denote this kind of paralysis led to misapprehension, and was supposed to imply disease of the cord itself; consequently, cases of ordinary paraplegia became the field for experiment. Duchenne and Meyer use the words spinal paralysis in this sense, and hence modify or contradict what they suppose to be the meaning of Dr. Hall. It is at once obvious that, although muscles may be cut off from the influence of the brain, and thus paralysed in respect of volition, by many diseases of the medulla, some portion of the cord below the seat of lesion may remain intact (or even in a state of exalted reflex activity), and that these cases are not examples of "spinal paralysis," using that term in the sense clearly defined by Dr. Hall.*

The paralysis, from *exclusion* of the spinal cord, is termed by the recent writers on the subject (we think somewhat inaptly), "traumatic;" and their conclusions upon the condition of muscular irritability thus induced agree precisely with those of Dr. Hall—viz., that it is notably diminished, or absent altogether. Meyer adds, that in these cases muscular sensibility is less diminished than contractility. With regard to the *vexata quæstio* of irritability in cerebral paralysis, there is little additional information. It has been contended, on the one hand, that irritability is relatively greater in the paralysed than the non-paralysed muscles, when they are severed from cerebral influence alone, and thus paralysed to the will; it has, on the other hand, been stated that precisely the reverse is true. The observers on each side have recognised exceptional cases to what they consider the general law, and some of the differences between them are referrible to the mode of experimentation adopted. Duchenne states that in simple cerebral paralysis there is no diminution of irritability; that, as a rule, it is normal and equal on the two sides; but that often it is greater in the paralysed than the non-paralysed muscles. Meyer, although urging that the contractility on the two sides is normal and equal, relates cases in which some muscles evinced increase of irritability; and one of Guitard's hemiplegic cases, which is referred to cerebral hæmorrhage, exhibits perfect absence of contractility. We have nothing to do at present with the theories employed to account for these phenomena; it is plain that too much difference has been observed to consider the laws of irritability and paralysis established so firmly, that electricity may be used with much advantage in diagnosis. The discrepancies probably arise from variations in the mode and time of examination, and in

* See Medico-Chir. Trans., vol. xxx. p. 207.

the cases submitted to the test. Some of the latter are readily appreciable: such as permanent and active contraction of the muscles on the one hand, their relaxation or atrophy on the other. The point of interest at present established is, that simple cerebral paralysis causes, as a rule, little or no diminution of irritability; but that spinal paralysis frequently does. This general result Duchenne and others have used with some advantage in the discrimination of a few doubtful cases. It is well known that the condition of the orbicular muscle of the eyelid may, in some cases, be made the means of distinguishing facial hemiplegia, of central origin, from paralysis of the portio dura. When the orbicularis is paralysed, there is strong probability, amounting almost to certainty, that the cause of paralysis is some injury to the seventh nerve; but the absence of orbicular paralysis by no means proves the reverse; since lesion of the portio dura, not involving the filament supplying the orbicular muscle, may present the precise characters of cerebral paralysis of the face. The diagnosis is then very difficult, but Faradisation may decide the question, which is an important one both in respect of prognosis and therapeutics, as we shall see in the sequel. The following case, recorded by Duchenne,* is an apt illustration:

“In 1846, I observed at La Charité (Salle St. Joseph, No. 13, Service de M. Bouillaud) a patient who had presented for three months partial paralysis of the right side of the face. He had awaked one morning with distortion of the features. When I examined him, the right commissure of the lips was lower than that of the opposite side, but the palpebral opening was as large on one side as the other; when he laughed, only the left side moved, drawing towards it the right cheek. He could not draw up the lips on the right side in order to whistle; his right cheek became distended, and the air escaped by a large aperture, which formed between the lips on that side. Food accumulated on the right, between the cheek and the teeth, and he had difficulty in articulating the labials. But he approximated the eyelids, contracted or raised the eyebrows, as well on the right side as on the left. I found in this patient all the apparent signs of facial hemiplegia, from a cerebral cause. But the electro-muscular examination soon dissipated all doubts, for I observed that the paralysed muscles had lost their electric contractility, a phenomenon which is always found in paralysis of the seventh pair, and which does not exist in hemiplegia of cerebral causation. Without this examination, I should have been very much embarrassed to establish a diagnosis, for the patient did not recal any exposure to a current of air. In the history nothing could be found to suspect compression of the nerve. The patient submitted to local Faradisation, and left the hospital very nearly cured some weeks afterwards (he would not attend for the completion of his treatment).”

In an obscure case of paralysis affecting the muscles of the shoulder and arm, Duchenne, from observing loss of electro-muscular contractility, inferred the existence of local injury to the nerves; and subsequent examination led to the discovery of a syphilitic exostosis, compressing certain branches of the cervical and brachial plexus.

In other forms of paralysis certain differences have been observed, but they are not sufficiently established at present to be of great utility. For example, Meyer states that in paralysis from progressive muscular atrophy, and from lead poisoning, the contractility and sensibility of the muscles are diminished, *pari passu*, with their nutrition; but that in rheumatic paralysis contractility is normal, and sensibility, though some-

* Bulletin Générale de Thérapeutique Méd. et Chir., tome xlv. p. 341.

times normal, is frequently exalted. The latter condition (exalted sensibility) is the more frequent, according to Duchenne; but Meyer adds, and quotes Froriep to support his statement, that sensibility is often diminished, and that in such cases the contractility is also less than normal. This deviation from the general rule, Meyer accounts for by the presence of exudation matter in the cellular tissue or muscles.

In lead-palsy, and in traumatic (spinal, Dr. Hall,) paralysis, contractility disappears before nutrition is affected. Duchenne adds that paralysis from lead never affects contractility in the flexor muscles of the fingers, the interossei, or supinator longus, and that this is a diagnostic mark between such palsy and traumatic paralysis from injury to the radial nerve.

Richter thus sums up the general data from which some assistance may be derived in diagnosis:—Faradisation divides paralytic affections into two groups; first, those in which irritability (or contractility) is absent; and second, those in which it is present. The first group includes lead-poisoning, progressive muscular atrophy, and traumatic paralysis; the second, cerebral, hysteric, and rheumatic. In the three forming the latter group, there are differences in respect of sensibility. It is normal in the first, absent in the second, exalted in the third.

As we have said before, it does not appear that these conclusions can be considered as established, but we have thought it right to bring them forward, in order that subsequent observation may confirm their accuracy, or correct their error.

The *therapeutical* effects of electricity have been subjected to close examination by the writers whose names are placed at the head of this article. Meyer says that electricity is of great service in the treatment of neuroses, and of diseases depending upon anomalies in the processes of secretion and excretion.

The more important facts connected with the latter class refer to amenorrhœa, and these have been discussed by Dr. Golding Bird, and reviewed in an earlier number of this journal,* so that we shall not allude to them in the present article.

With regard to the former, the neuroses, Meyer speaks very favourably of the employment of electricity in neuralgia, and mentions the cure of many cases of sciatica, &c. M. Videt (quoted by Guitard), in a *Thèse Inaugurale*, 1853, relates original observations in l'Hotel Dieu de Toulouse. Of these, six are cases of neuralgia, varying from two days to three years in duration, which were all cured in from two to nine applications. Videt adds that the secondary current, and a mixed method of exhibition, were the most successful.† M. Guitard also relates six cases of neuralgia cured, or improved; three in which no amendment took place. Duchenne says that sciatica is often radically cured.

In *spasms* of various character some benefit appears to have been derived from electricity. When the contractions are clonic, the affected muscles have been subjected to excitation; when tonic, their antagonists have been stimulated to activity. Meyer states that the cause of the

* Medico-Chirurgical Review, 1849, p. 373.

† By the mixed method is intended the Faradisation of the nerves and the muscles at the same time, as distinguished from the direct and indirect modes before described, p. 141.

latter class of spasm is frequently paralysis of the opposite side, and he brings forward three cases to support this view, and to prove the efficacy of Faradisation. Guitard's observations show little in favour of this mode of treatment.

After looking carefully through the statements made with regard to these two groups (hyperæsthesia and hypercinesis) we feel compelled to say, that Faradisation appears of only doubtful value in their treatment, for it is quite impossible to form a diagnosis of the nature of disease from the records which are given; the results have been but partially successful; and the diseases are well known to disappear under various forms of treatment, differing most widely from each other in all that we know of therapeutic action. With regard to the opposite conditions of disease, there is more satisfactory information, and we notice first:

Anæsthesia.—When of peripheric origin, or when the result of rheumatism or hysteria, Meyer says Faradisation is a most important mode of treatment; and also when of centric origin, such as extravasation, &c., at the base of the brain, there is evidence to show that electricity may recal the function of the nerves, provided that the extravasation, &c., have been removed or lessened. This was first shown by Steinrück, and subsequently demonstrated by M. Brown Séquard. Meyer relates three cases of anæsthesia which were cured by Faradisation. One of these implicated the branches of the fifth nerve, and the posterior divisions of the four upper cervical nerves; the other two were of the ulnar nerve. Guitard and Duchenne relate cases of amaurosis cured by electric treatment.

Paralysis.—This class of diseases has afforded the widest and richest field for the experimenter with electricity. We have already shown that the older observers met with many successful cases; and in the present day it is extremely common to find some form of electric stimulation recommended for the paralytic patient. Duchenne asks, "Is it reasonable to use local Faradisation in the treatment of paralysis consecutive to cerebral hæmorrhage?" In reply, he states that the fact is incontrovertible, that at certain periods some cerebral paralyzes are cured, or improved, by purely local treatment. The following case is an instructive example:—

"La Charité, Salle St. Felix, No. 10, Service de M. Andral. Paralysis of the upper limb, stationary in a man æt. 32, and in whom a cerebral hæmorrhage had produced, ten months previously, complete hemiplegia of the right side. State of the patient before treatment by Faradism: the lower limb is no longer paralysed, the upper limb falling against the side of the body, can be moved only slightly from the trunk, by contraction of the deltoid; flexion of the fore-arm is impossible, the hand is deprived of motion, is of violet colour, and lower temperature, it has little sensibility, and has lost almost all sense of touch. After a few applications of local Faradisation, the patient could raise the arm to a right angle from the trunk, carry the hand to the head, neck, and back; flexion of the fore-arm, its extension, pronation, and supination became easy; power returned rapidly. At an advanced period of the treatment, flexion and extension of the fingers were performed with difficulty, none of the little muscles of the hand contracted voluntarily; it was only after a considerable number of applications that these muscles were cured of their paralysis. . . . Subsequently, the colour, temperature, and sensibility were perfectly restored."

In commenting upon this case, Duchenne remarks with regard to the

muscles of the hand, that each of these little muscles was successively Faradised, and that after this they recovered their movements and agility. But in all cases there is not this success; in some there is perfect cure, in others partial improvement, in a third group there is not the slightest benefit. Duchenne has endeavoured to discover the cause of these different results; to establish their diagnosis, and base thereupon, not only the prognosis, but their treatment.

In 1852, M. Debout* gave the following *résumé*. Duchenne finds two periods of paralysis. In the first, the paralysis is symptomatic—i.e., dependent upon the central injury; in the second, it is idiopathic—i.e., dependent upon the muscles. In the latter case, the paralysis is localised, and this it becomes when the organ of innervation recovers its functions, the muscular fibre remaining paralysed by the simple fact of habit. The confusion of these two states has necessarily led to great mistakes in treatment. Many injurious results have followed from Faradisation during the earlier period, and Duchenne says that it should not be attempted for at least six months after an apoplectic seizure. In the later papers of this author, examples are given of injurious effects from Faradisation, when attempted in recent cases, at La Charité and l'Hôtel-Dieu. It is also urged that such treatment should not be adopted when any signs of spontaneous improvement present themselves, as the physician may then reap honours due to nature alone. It is only after six months' duration, and when the paralysis is stationary, that Duchenne thinks it right to employ Faradisation. When, however, this period has arrived, success is variable. In one-twentieth there was absolute cure, in one-fourth there was improvement, in three-fourths there was none. These different results are to be referred to the nature and present condition of the central lesion. When treatment has been successful, it is inferred that the original lesion was removed, and consequently that the paralysis was purely muscular; when only partially beneficial, that there is still some paralysis directly dependent upon the lesion or its effects, and that over and above this, there was muscular paralysis, which the local Faradisation has removed; but that when no good results, the paralysis is entirely symptomatic, or dependent upon the original lesion. Duchenne admits that it is impossible to form an accurate diagnosis of these conditions, but gives, as the result of his experience, that those patients who, after several months, present paralysis without the least muscular spasm, are readily cured by local Faradisation; while, on the other hand, those in whom spasm is observed derive no apparent benefit. He concludes that the former are instances of localised, or muscular paralysis; and the latter, of paralysis depending upon persistent central lesion: and these conclusions he has many times verified by post-mortem examination.

Duchenne endeavours to explain this relationship by the excess of spinal activity resulting from defective action of the brain; an excess which, of course, does not exist when the brain has recovered its functions. The degree of reflex activity may be considered (according to this author) an index of the amount of cerebral injury. We cannot agree with this dogma; active contractions are frequently present, when no excess of spinal reflection can be demonstrated by irritation of the skin;

* Bulletin Générale de Thérapeutique, p. 97.

yet in these cases the muscles are extremely sensitive to percussion. The whole question is involved in considerable obscurity, and is not to be decided from a few cases only; the important fact to be noticed now is, that slight contractions of the muscles do not preclude the hope of benefit from local Faradisation. Duchenne relates a case in illustration of this position, the prominent points of which are the following:—A woman, æt. 62, suffered an apoplectic seizure, leaving complete right hemiplegia, with paralysis of the tongue, involuntary passage of fæces and urine, and spasm of the muscles in the fore-arm and hand. These phenomena had been stationary for six months, when local Faradisation was first practised; and this proved eventually perfectly successful, the patient being completely cured. She was at the time under the care of M. Andral, at La Charité.

Permanent contraction of the muscles is considered by Duchenne to afford evidence of inflammatory action in the brain, which contraindicates the employment of Faradisation. This condition is to be distinguished, however, from that of simple rigidity of the flexors, arising from their persistence in a state of passive contraction. The attempt to extend the limbs in the former instance is acutely painful, and increases the spasm; in the latter it is much less painful, and will, if commenced early enough, and if cautiously maintained, remove the contraction. Faradisation, if employed in the former case, leads to very injurious results; if in the latter, there is no danger.

Duchenne finds that in facial paralysis of cerebral origin much good may be effected by electric treatment, but that the patient is thereby exposed to danger of a renewed attack. In ten cases of this description, serious accidents occurred to three; and Duchenne states, that notwithstanding the knowledge which he now possesses of the degree of excitability enjoyed by every muscle of the face, so that he can administer the precise dose which they require, it is never without hesitation and fear that he undertakes their treatment. There is no danger, however, when the facial paralysis is from injury to the portio dura; hence the importance of diagnosis, previously pointed out.

The last paper by Duchenne is concluded by some practical directions. The first of these is, that as the occurrence of one apoplectic seizure indicates some probability that another may take place, it is desirable to avoid all excitation of the central organs; and hence Faradisation should be limited as closely as possible to the muscles affected, and only a short current allowed to pass through the limbs. Farther, the intermittences should be long, for by this means there is little sensation of pain, although the current may be powerful; and at the same time with this stronger power it is possible to reach the deeper muscles. Rapid currents are too painful to be employed with anything but a low power, and then only the superficial muscles are affected.

The second general direction is, that it is necessary to act individually upon all the muscles that are paralysed. The third, that when there is spasm present in a group of muscles, it is by Faradisation of their antagonists that this treatment is of service. It is an important phenomenon, that this method of operating causes immediate cessation of the spasm, which relief continues for a certain time after the application. Lastly,

Duchenne remarks, that the "*séances*" ought not to be too long, and that if, after fifteen or twenty applications, there is no improvement, there is little hope of success.

In M. Guitard's treatise are given the records of four cases of hemiplegia, treated by Faradisation, and three of these with some measure of success. The first of them presented complete paralysis of motion and sensation in the arm, but incomplete in the leg. The duration of paralysis was two years. Hand and fingers rigidly flexed; wrists pronated. Treatment was continued, more or less regularly, for nine months, and at the end of that time there was great improvement; the fingers could be voluntarily extended, and the arm was useful. At the first, there was no contractility observed in the muscles, but it returned after the third application. The diagnosis in this case was hæmorrhagic apoplexy, but it is somewhat at variance with other statements made by these electricians, that the electro-muscular irritability was absent.

The second case is one in which hæmorrhage was diagnosticated. The patient, a man æt. 72, presented complete hemiplegia of two months' and a half duration; there was marked muscular atrophy. After treatment for one month, there was some improvement, the fingers and arm being capable of executing voluntary movements.

The third case is one of hemiplegia in a woman æt. 23. Electric treatment was only attempted twice, and without result.

The fourth case is of a complete hemiplegic attack in a chlorotic female æt. 18. The leg recovered in eleven weeks, but imperfect paralysis remained in the arm. After two Faradisations, the catamenia appeared (for the first time); and by subsequent treatment the cure was established. M. Guitard, in the same treatise, refers to numerous cases of paralysis treated with success by recent observers; but of those which he has recorded from his own experience, there cannot be any very satisfactory conclusion established. The last case is one in which the diagnosis of cerebral lesion is imperfect; the third shows nothing in either direction; the second is probably one of the cases in which Duchenne would think the honours due to the *vis medicatrix naturæ*; and the first relates "improvement" only, after nine months' treatment. But this first case, and the results of M. Duchenne's treatment, and those with which we are familiar in our own country, prove that many cases, even of long duration, are amenable to treatment. It is erroneous to suppose that the only way in which Faradisation acts is by inducing contraction in, and thus improving nutrition of, the muscles; for in many cases no contraction was induced until after repeated applications. The explanation given by Duchenne is the following:

"Le mouvement volontaire repose sur une *trame organique* soumise à une double influence, *l'innervation*, et *l'hématose*. Comment expliquer autrement les paralysies chlorotiques, saturnines, etc.? C'est donc à leur point de conflit qu'il faut s'adresser pour ranimer les propriétés vitales. . . . Le mouvement curateur se produit au *conflit du sang et de l'influx nerveux avec la fibre musculaire*." (Guitard, p. 194.)

Whether this is a correct account of the process or not, it is important to remember the fact upon which it is based—that those cases are not incurable in which contractility appears to have become extinct. The order in which improvement takes place is said to be the following:

The return of (1) electro-muscular sensibility; (2) Warmth of the skin; (3) Improved nutrition; (4) Electro-muscular contractility; (5) Obedience to volitional stimulus.

Meyer concludes, from his own observations, that the duration of paralysis exercises little or no influence upon its prognosis, but that the age of the individual affected does; the greater the age, the less is the probability of cure.

Paraplegia.—M. Guitard refers to five cases under the care of MM. Fouquier, Roux, Leroux, James, and Polin, all of which were successfully treated by electricity, but of course there is no evidence to show that, in any one of these, there was organic lesion of the spinal cord. M. Guitard relates two cases occurring in his own practice, in one of which (incomplete at the commencement) there was some improvement; in the other, marked amelioration for a time, but subsequent aggravation of the disease, which necessitated the discontinuance of Faradisation.

In those very interesting but obscure forms of paralysis which occur as the direct or remote consequences of certain blood-conditions, such as *rheumatism, lead-poisoning, &c.*, Faradisation has proved of immense service. M. Guitard relates two cases of rheumatic paralysis, the first of which existed in a most aggravated form. The patient was twenty-three years of age, and had suffered for three years; there was general emaciation, and an emprosthotonic condition, the head being drawn forwards on the chest, the thighs flexed upon the abdomen, and the legs upon the thighs. After Faradisation for one month, the head could be moved into an erect position, and towards either side; the legs could be moved into and out of the bed. Treatment was neglected for a month, and all the worst symptoms returned: it was recontinued for six weeks, and at the end of that time restoration was almost complete. The second case was of rather less severity, but of the same character as the first; and after treatment for one week, many voluntary movements could be executed with facility. Although electro-muscular sensibility was preserved, contractility was lost (another observation opposed to the general statements of Meyer and Duchenne). The first and second applications produced abundant diaphoresis, after which there was notable improvement; the patient became weary of the applications and their painfulness, or it was hoped that much greater improvement would have ensued.

In *General Progressive Paralysis*, whether idiopathic or complicating insanity of mind, electric treatment has proved of service. M. Briere de Boismont asserts that these cases may be diagnosticated by means of the electric current; contractility being absent in the former, but retained in the latter. To support the truth of this statement, nine cases are recorded. M. Duchenne relates the following cases of paralysis from progressive muscular atrophy: In the first, the serratus magnus of the right side, the trapezius, rhomboid, and other muscles of the shoulder and trunk, were affected, and fibrillar contractions occurred in many regions of the body. The second case was one of far-advanced general atrophy. The third was limited to the sacro-spinalis, deltoid, trapezius, and some small muscles of the hand. Each case was completely cured.

On Traumatic Paralysis.—M. Guitard quotes the following remarks from M. Debout: The excitation of a muscle takes place only in those

points which are *en rapport* with the excitors; the latter ought, therefore, to be moved successively over the whole surface of the muscles. The intensity of the current should be in direct proportion to the thickness of the muscles: the rapidity of intermittences in inverse proportion to their contractility. In traumatic paralysis, there is nothing to fear from central disturbance. Duchenne remarks, it has been too generally considered that the paralysis which occurs from lesion to a nerve is incurable, since local Faradisation frequently effects their cure. Cases are recorded in support of this assertion. Dislocation of the shoulder downwards produced paralysis in two instances, and the cure was complete in each. In a third case, there was paralysis with atrophy of the muscles in the hand, the result of laceration of the radial nerve four years previously. The fingers had assumed a vicious position. Under the influence of local Faradisation, there was the successive return of—1. Warmth; 2. Nutrition; 3. Tone; 4. Position of the fingers; and 5. Volitional movement; without the production of a single electric contraction during the whole course of treatment.

Paralysis arising from compression of the muscles themselves, or from distension of their fibres, has proved amenable to the same therapeutic agent. Duchenne remarks that, when muscles are thus paralysed, they retain their electro-contractility, which may serve to diagnosticate the case from one of injury to the nerve.

That form of immobility, in respect of volition, which is accompanied by tonic spasm of the muscles, has been frequently cured through Faradisation of the antagonistic muscles. Thus, M. Debout relates a case of torticollis posterior, due to contraction of the rhomboid muscle, and the levator anguli scapulæ cured by three applications of electricity to the serratus magnus. M. Duchenne records another case of torticollis of the clavicular portion of the trapezius, cured by Faradisation of the trapezius on the opposite side; also, an instance of distorted face, from tonic spasm of the zygomaticus minor, cured by causing contraction in the muscle of the opposite side. M. Guitard relates the cure of similar deformities.

In the September number of Schmidt's 'Jahrbuch' are, detailed by Krug, eleven cases of impotentia virilis, cured by electricity. This account is the abstract of a paper (in the Vienna 'Wochenschrift') by Schulz. The cause of impotence was, in each case, the absence of erection, due to exhaustion from past excesses of various kinds.

Although some of the records which we have extracted from the several treatises under review, and many others which our space will not permit us to relate, were followed by an almost fabulous success, there can be no doubt that electricity, in its recent mode of application, has been of greater service than many in this sceptical age were willing to admit. Upon the employment of this agent in surgical and obstetric practice, it is not our intention to enter. In the former, and, to a certain extent, in the latter, the object for which it is used, and the mode of its application, are so different from those of the affections which we are now considering, that they require a more special notice. The class of disease which is more or less amenable to treatment is, as is well known, the paralytic. We do not consider that the recent writers upon this subject have added much to the information which we possessed when Dr. Bird's lectures were deli-

vered at the College of Physicians, in 1847. In respect of diagnosis, there is some confirmation of what was almost as certain before; but we derive little or no fresh assistance. With regard to treatment, we find only the old belief established—viz., that where paralysis persists from habit, after the removal of some central lesion, or of some modified condition of the blood, electricity is of great service in re-awakening the dormant muscular function. It is, in the present state of therapeutical science, the only means by which we can arrive at these inactive muscles; but when the paralysis depends upon some persistent disease, or upon some disorganisation of the nervous centres, electricity is of no service to the muscles, it can effect no removal of the paralyzing cause, and it may be highly injurious to the individual in various ways.

What it does appear that MM. Duchenne, Guitard, and others, have shown is—1. That local treatment—i.e., Faradisation of the muscles individually—is the safest and most effectual mode of proceeding; 2. That it is possible, by a practised adaptation of the apparatus, to accomplish this individual excitation; 3. That such treatment should not be discouraged, because the muscles exhibit at the first no contractility; 4. That it should be continued in spite of apparent failure, provided no evil effects result.

The records of successful treatment by electricity of numerous other diseases—such as epilepsy, cholera, angina pectoris, coughs, ascites, &c., &c., make little addition to the sum of our previous knowledge—viz., that all of these affections may occasionally yield to this or any other form of medication, without our being able to account for the results upon any received principle of pathology or therapeutics.

The cases of this kind enumerated by Guitard and others need farther confirmation to call for special consideration.

The researches of M. Duchenne have hitherto appeared scattered through various journals, and extending over some length of time; but he has promised—and there is “*actuellement sous presse*”—a general account of his works, in the form of a ‘*Traité de la Faradisation localisée, et de ses Applications à la Physiologie, à la Pathologie, et à la Thérapeutique*,’ and it is with much pleasure that we anticipate its appearance.

J. Russell Reynolds.

REVIEW XIII.

Census of Ireland for the Year 1851. Part III. Report on the Status of Disease. By W. DONNELLY and W. R. WILDE. Presented to Parliament. Folio. 1854.

WHEN the arrangements were in progress for taking the census of Ireland, on the night of the 30th March, 1851, it appeared to the Commissioners that some valuable information regarding the sanitary condition of the country might be gained, by procuring returns of all persons labouring under disease of any kind, either at their homes or in public institutions. This was accordingly done, and forms the groundwork of the Report now before us. The statements regarding some of the diseases were subsequently verified, and more detailed information concerning the subjects of them obtained, through the medium of the constabulary force.

The results cannot be considered as representing the average health of Ireland, for they merely show the number sick, and their diseases on a given day; and there are no means of ascertaining whether sickness was more or less prevalent than usual at the time. They are also open to the objection, that although the returns from public institutions are probably accurate, those of the sick at their own homes must be deemed to be merely approximations to the truth. As, however, the sources of error must be nearly the same over the whole country, the results may be relied on as showing the relative prevalence of the various classes of diseases in the different Provinces and Counties. Much valuable and interesting information has been collected on the subject of certain specific affections, particularly deaf-dumbness and blindness, to both of which conditions Mr. Wilde had long directed his attention.

The diseases in the general returns were divided into two classes:—permanent and temporary maladies.

“Among the former may be classed the deaf and dumb, the blind, the lunatic, idiotic, paralytic, and epileptic, as also the lame and decrepit; while under the latter head may be placed all those labouring under the ordinary acute and chronic maladies to which the inhabitants of this country are liable.”

In his remarks on the “Permanent Diseases,” Mr. Wilde has given a very interesting outline of the history of the instruction of the deaf and dumb from the earliest periods, and of the institutions founded in Ireland for that purpose. He has given a similar account of the asylums for the blind, and of those for lunatics and idiots. In the report upon the number and condition of the sick in the public hospitals, he has traced the history of these establishments from the time when “stood the *Broin Beury*, or the ‘House of Sorrow,’ where the sick and wounded were provided for,” down to the epidemic years of 1847-8, when many temporary hospitals were erected for the treatment of the sick poor. Into these subjects, however, we cannot enter, nor can we afford space for the consideration of many interesting details respecting the condition of the permanent sick, such as the sexes, ages, occupations, and social condition of the deaf and dumb, the causes of congenital and acquired muteism, and the extent of education among this class; or the numerous facts recorded concerning the blind and the insane. Much of the information is given in a tabular form, and is so condensed already as to be difficult of further abridgment. Recommending the Report, therefore, as well worthy of careful study by all who are interested in the subjects treated, we shall content ourselves with stating a few of the general results as to the amount of these diseases, and which, for convenience, we have condensed into the following table:

	Males.	Females.	Total.	Proportion to population.	Relative proportion of sexes.	
					Males.	Females.
Deaf and dumb	2688	2059	4747	1 in 1380	100	74.5
Blind	3588	3999	7587	1 in 864	100	111.5
Lunatics	2503	2571	5074	1 in 1291	100	102.3
Idiots	2666	2240	4906	1 in 1336	100	84.0

For the purpose of comparison, the following statement, compiled from the population tables of Great Britain, is submitted, showing the proportion of deaf mutes and blind in England and Scotland respectively. The

results as regard lunatics and idiots are not given, because in the Irish Report, all of that class, whether at large or in confinement, have been included, while, in the English and Scotch returns, none have been enumerated but those in public and private establishments for the insane.

		Proportion to population.	Relative proportion of sexes.	
			Males.	Females.
Deaf and dumb	{ England and Wales . . .	1 in 1738	100 ... 82·9
	{ Scotland	1 in 1340	100 ... 80·0
Blind . . .	{ England and Wales . . .	1 in 979	100 ... 88·4
	{ Scotland	1 in 960	100 ... 105·3

The most striking facts elicited by this comparison are the greater number of deaf-mutes in Scotland and Ireland than in England; the higher proportion of blind in Ireland; and the difference in the relative proportion of males and females affected with blindness, being in England, 88, in Scotland, 105, and in Ireland, 111 of the latter to 100 of the former. In the Great Britain Report, this excess in Scotland is said to be “a result probably traceable to the preponderance of aged women in that country.” We know not whether the same preponderance exists in Ireland, but it is worthy of remark, that the excess of blind females there is entirely among those above forty years of age. The greater number of blind in proportion to the population, is probably the result of the epidemics of ophthalmia which have occasionally prevailed in Ireland, and the last of which extended over the three years 1849-51. During that period, 86,959 cases of the disease were treated in the Irish workhouses; and on the day on which the census was taken, there were 3457 persons affected with it in these establishments.

Passing from the consideration of these diseases to the general returns, including all cases, whether belonging to the class of permanent or temporary maladies, we find that the number sick, at their own homes or in public institutions, on the night of the 30th of March, 1851, amounted to 104,495, being 1 in 63 of the whole population. The proportion, however, varied materially in the different provincial divisions of the country. Thus, it was 1 in 46 in Munster, 1 in 58½ in Leinster, 1 in 74 in Connaught, and only 1 in 92 in Ulster. This striking difference appears to be intimately connected with the social condition of the people. If we divide them into three classes—1st. The paupers in the workhouse; 2nd. The inmates of the various prisons; and 3rd. The remainder of the population—we find that, of the first class, 1 is sick in every 5⅓; of the second, 1 in 15; and of the third, only 1 in 112, although in the last are included all the hospitals and asylums, except those attached to the prisons and workhouses. It follows, therefore, that if there be a great preponderance of paupers in any Province, the proportion sick will stand relatively high. According to the data in the Report before us, the proportion of paupers in the workhouses at the time of taking the census was, in Munster, 1 in 13⅘ of the population of the Province; in Connaught, 1 in 24⅔; in Leinster, 1 in 32⅞; and in Ulster, 1 in 84⅞. From this it will be clearly seen how much the general results must have been influenced by this circumstance. The amount of sickness in Leinster is higher than in Connaught, although the proportion of pauper population

is less; but this difference may probably arise from the number of cities and crowded towns in the former, and the numerous hospitals and asylums for the sick in Dublin.

In the appendix to the Report, detailed abstracts are given of "the number, sexes, and diseases of the sick at their own homes, or in public institutions," in each of the Provinces on the night of the census. From these we have compiled the following Table, showing the proportion sick by each class of diseases in every 10,000 of the population:

	Leinster.	Munster.	Ulster.	Connaught.	Ireland generally.				
Zymotic, or epidemic, endemic, } and contagious diseases . . }	47·8	...	94·3	...	23·1	...	47·9	53·4
Sporadic diseases:									
Diseases of the brain, nervous } system, and organs of sense }	44·3	...	37·3	...	36·1	...	29·0	37·4
Diseases of the circulating organs	1·1	...	0·9	...	0·7	...	0·6	0·8
Diseases of the respiratory organs	18·1	...	20·6	...	12·3	...	11·6	16·0
Diseases of the digestive organs .	8·3	...	7·1	...	6·0	...	5·9	6·9
Diseases of the urinary organs .	0·7	...	0·3	...	0·3	...	0·5	0·4
Diseases of the generative organs	1·7	...	1·0	...	0·7	...	0·9	1·0
Diseases of the locomotive organs	15·9	...	15·5	...	10·9	...	10·7	13·5
Diseases of the tegumentary organs	12·2	...	16·1	...	4·2	...	12·9	10·9
Diseases of uncertain seat . . .	18·0	...	20·2	...	11·6	...	12·6	15·9
Accidental causes	1·9	...	1·8	...	2·0	...	1·6	1·9
Causes not specified	1·4	...	1·8	...	0·9	...	0·8	1·3
<hr/>									
Total by all causes	171·4	...	216·9	...	108·8	...	135·0	159·4

An examination of this Table shows that, with the single exception of accidents, the prevalence of all the classes of diseases is, in Ulster, below the average of Ireland generally, but that the exemption is most striking in the zymotic diseases, and in those of the respiratory and tegumentary organs. Connaught enjoys a like exemption from pulmonary affections, and the diseases of the brain and nervous system are lower in it than in any of the other provinces. Zymotic diseases were four times as prevalent in Munster as in Ulster, and twice as prevalent as in the other two provinces. The reporters observe that they were most numerous

"In the city of Kilkenny and the counties of Clare and Kerry, the city of Waterford and the town of Galway, in which localities the proportion varied from 1 in 55 to 1 in 94 of the population; and least in the counties of Antrim, Down, Armagh, Donegal, and Dublin, and also Belfast town; showing, in the former instance, the effects of poverty and destitution in the production and maintenance of epidemic diseases; and in the latter, those of comfort, industry, and cleanliness in maintaining a comparative immunity from diseases of an epidemic or contagious character."

Fever was the most common disease of this class, constituting, indeed, one-eighth of the whole number sick, and upwards of one half of the cases were in the workhouses and the workhouse hospitals. Dysentery and diarrhoea were next in point of numbers; "they usually follow in the track of fevers;" and, like them, are chiefly to be found in the workhouses. An investigation seems imperatively called for on the part of the authorities into the causes of fever being so very prevalent in the workhouses, and into the best means of reducing its amount. They are, for the most part, large and handsome buildings, not overcrowded, and remarkably clean, and yet fever appears to prevail to a great extent in them. It is worthy of note, that while in Munster two-thirds, and in

Leinster three-fifths, of the fever cases were in the workhouses, in Ulster the proportion in these establishments amounted to little more than one-fourth. Again, the ratio which the cases of fever bear to the inmates of the workhouses differs greatly, being in Leinster 40, in Munster 34, in Ulster 25, and in Connaught $17\frac{1}{2}$ per 1000. Here, then, is a field for investigation; what differences exist in the construction and in the management of these establishments, and what influence are they likely to exert on the health of the inmates?

Influenza and ophthalmia were the other prevalent diseases of the zymotic class; the latter was most rife in Cork, where as many as 1 in 50 of the population were affected; and it was also very common in Tipperary, and in the county and city of Limerick.

Next to the zymotic, the most prevalent diseases were those of "the brain, nervous system, and organs of sense," consisting chiefly of blindness, insanity, idiocy, deaf-dumbness, and paralysis. These, as already stated, have been treated of separately under the head of permanent diseases, and we must refer our readers to the Report itself for the full details concerning them. The reporters observe: "It is gratifying to find that so few cases as 9 were returned under the head of delirium tremens;" a remark in which we should most cordially concur, if we could only persuade ourselves that no cases had been omitted, or returned under a different name.

The next diseases in point of frequency are those of the respiratory organs, furnishing a tenth of the whole amount of sick. They are considerably more prevalent in the south and east Provinces than in the north and west. About two-fifths of the cases were consumption, which appears to be much more common in Leinster and Munster than in Ulster and Connaught; the proportions being 79 and 68 in 10,000 of the population in the former, and 55 and 49 in the latter Provinces.

Diseases of the locomotive organs form a considerable item in the table. Nearly one-half of the cases were rheumatism, and above a fourth were returned as "lameness." On the day of the Census, 519 persons were labouring under fracture, 75 under dislocation, and 58 under the effects of amputation of some of the extremities.

The only other class to which we shall advert is that of diseases of the tegumentary organs, from which Ulster enjoys a very marked exemption, the ratio being only one-third as high as in Leinster and Connaught, and one-fourth as high as in Munster. The difference arises chiefly from the almost entire absence of itch, and also from the low proportion of cases of scald-head and ulcers.

We must now conclude these remarks, referring our readers for further details to the Report itself. Mr. Wilde is entitled to great credit for the industry with which he has collected the facts, the clearness with which he has arranged them, and the interest he has contrived to throw into the usually dry details of a statistical report.

REVIEW XIV.

1. *De cognocendis et curandis Placentæ Morbis.* WILDE.—Berol., 1833.
2. *Handbuch der Speciellen Pathologischen Anatomie.* Von CARL ROKITSANSKY.—Wien, 1842.
3. *Lectures on the Theory and Practice of Midwifery.* BY ROBERT LEE, M.D.—1844.
4. *Ueber die Krankheiten des Eies und der Placenta.* GIERSE und H. MECKEL. ('Verhandl. der Ges. für Geburtsk.')—Berlin, 1847.
5. *Abhandlungen über den Bau der Molen.* Von H. MÜLLER—Würzburg, 1847.
6. *Ueber den Bau der Molen.* METTENHEIMER. ('MULLER'S Archiv,' 1850.)
7. *Note sur les Altérations du Placenta.* Par CHARLES ROBIN. ('Archives Gén. de Méd.,' Juin, 1854.)*

(Continued from No. 27, p. 36.)

IN the writings of the older obstetric authors there occur isolated reports of cases in which morbid conditions of the placenta were observed. Many of these cases are of great interest, as illustrations of the general fact of a close connexion between morbid alterations of the placenta and abortion or death of the fœtus. But very few, if we except the instances of that remarkable and unmistakable condition known as the vesicular mole, are of any value as illustrations of the morbid anatomy of the organ. We have already stated our opinion, that it is owing to the want of that minute microscopical analysis of the altered structures, without which an accurate knowledge of structural alterations cannot be obtained, that the greater part of the cases recorded by the older authors must be rejected as useless in any attempt to delineate the history of the diseases of the placenta. What would it avail to discuss the pathological import of morbid appearances, the real nature of which is uncertain? They defy all attempts at interpretation. In thus excluding all those cases which are defective from imperfect investigation, or obscured by erroneous interpretation, we are not insensible to the attending advantage of greatly simplifying our task by narrowing the field of our inquiry. In passing under review the facts recorded by recent observers, we shall tread upon firmer ground; and the soundness of the conclusions we may arrive at will be more easily tested by others.

From time to time various authors have sought to collect the scattered records of individual diseases into a systematic compendium of placental pathology. Some of these evince considerable research; few bring any original additions to extend or to correct what was previously known. Besides these necessarily imperfect attempts at forming systems of placental pathology, there exist innumerable records of cases and many monographs treating of particular diseases. It is not our purpose to give a bibliography of the subject, or to enter upon a minute critical analysis

* The titles of other works will be found at the head of the first part of this article.

of the more important contributions. But the plan we have proposed to ourselves would not be complete without passing in rapid review the names of the principal authors who have laboured to illustrate the subject before us.

One of the earliest systematic attempts was that of Schacher and Seiler, who published a treatise, entitled '*De Placentæ Morbis*,' in Haller's '*Disputations*,' (vol. iv.) in 1709. This essay contains a summary of the old opinions, but not much that deserves to arrest attention at the present day. In the edition of the works of Vallisnieri published at Padua, in 1710, there is an excellent commentary upon a case of hydatidinous placenta, entitled '*Storia del Parto Vesicolare*.' In the 48th epistle of Morgagni, '*On Moles and Abortions*,' are several interesting cases. Stein and D'Outrepoint have each contributed important cases and observations.

But the first author whose views we think it useful to notice is Murat. This writer, in a special article on the diseases of the placenta, drawn up for the '*Dictionnaire des Sciences Médicales*,' gave a systematic summary of the scattered observations of previous authors. He observed that the placenta might be altered in its colour, dimensions, structure, or consistence; that it might be scirrhus, cartilaginous, or osseous; that its adhesions might be too dense or too slender; that different concretions, hydatids, might be found; that its protracted retention in the womb might occasion different modes of alteration; and lastly, that rupture of the parenchymatous tissue had been observed. He especially observes, that the colour and substance of the placenta are often changed in women affected with syphilis; and that these affections seem to favour detachment. He says, further, that osseous or calcareous concretions have been found; sometimes true steatomatous concretions; and, occasionally, sanguineous concretions. Of all the diseases of the placenta, he says, that in which this organ is transformed into a vesicular mass is the most frequent. It cannot be said that the article of Murat possesses any greater merit than that of being the first attempt to epitomise the observations of other authors.

The contributions of M. Dance are of a more original character, and of greater interest than that of Murat. In his first paper* M. Dance relates two cases: the first is an example of inflammation of the decidua; the second, of congestion of the placenta. In another paper† M. Dance relates an example of inflammation of the chorion and amnion. These cases are all valuable, as elucidating the pathology of the ovum: they will again engage our attention.

The next author is Brachet, who describes several cases of inflammation of the placenta and membranes. He sums up his observations with the general remark, that the placenta is subject to the same affections as the other organs, and that these are produced in it by the same causes. Inflammation, he says, is the most frequent. The facts he relates constitute an important accession to the subject.

Cruveilhier, appreciating fully the great importance of the subject, added largely to our knowledge by numerous valuable cases, and by many admirable reflections. He observes, that "by the placenta, morbid causes

* *Répert. Gén. de Méd.*, tom. iii., 1827.

† *Arch. Gén. de Méd.*, 1829.

are transmitted directly to the foetus, the complex organization of which is susceptible of all the diseases observed in the adult; but the placenta itself may be subjected to the influence of some of these causes, and in some degree may arrest them; the channels for the transmission and revivification of the nutritive materials being interrupted wholly or in part, the child is born dead, or greatly enfeebled. It may be said that the diseases proper to the foetus influence its nutrition only in a moderate degree, and that the diseases of the mother exercise over this nutrition a much smaller influence than do the diseases of the placenta itself." We shall adduce evidence hereafter to show that this eminent pathologist has, in this summary, somewhat underrated the influence of the diseases of the mother upon the foetus. He thus classifies the diseases of the placenta:

1. *Hypertrophy*.—This consists sometimes in a serous infiltration, analogous to that so often observed in the umbilical cord. In one case related, this condition coincided with a pseudo-membranous infiltration.

2. *Atrophy*, which may be either general or partial, invading particular cotyledons only.

3. *Inflammation*.—M. Cruveilhier cites the observations of Brachet in proof of this affection.

4. *Ossification*.—This almost always takes place on the uterine surface. Two kinds may be distinguished: in one, there is an osseous, or rather a stony shell, one or two lines in thickness, covering uniformly, or in patches, the uterine surface, without penetrating its substance; in the other, a kind of osseous needles penetrate the placenta, and traverse it in every direction. This kind of petrification always proceeds from the uterine towards the foetal surface. The seat of the first appears to be in the fibrous membrane which invests each cotyledon (decidua); the seat of the needle-like concretions, or of the small masses forming grit, is very obviously in the arterial vessels.

5. *Hydatidiform Cysts*.—This is the most frequent of the alterations of the placenta. Cruveilhier has the merit of demonstrating that these cysts are not hydatids.

6. *Apoplexy*.—This name is given to a condition in which collections of blood are found in the torn substance of the placenta.

We shall have occasion to recur to the facts and views adduced in the writings of Cruveilhier.

Wilde,* who next attempted a methodical arrangement of the diseases of the placenta, has been frequently, but without much reason, referred to as an authority upon the subject. He divides all diseases of the placenta into three genera: *dynamical*, *organic*, and *mechanical*. Dynamical diseases are those which affect the vital forces of the placenta. Organic diseases are those which attack and impair the structure of the organ. Mechanical comprise injuries and solutions of the relations of the organ with adjacent parts. Under the head of dynamical diseases he ranges inflammation, suppuration, hepatization and induration, and gangrene. Under organic diseases he places hypertrophy and atrophy, designating these, diseases of evolution; as diseases of *intimate cohesion*, hardness and ossification, and mollities or malacia; as diseases of the proper texture, scirrhus, and the "placenta obesa;" as diseases of hetero-

* De cognoscendis et curandis Placentæ Morbis. Berol., 1833.

logous formation, tumours. The mechanical diseases are, faults of adhesion—viz., firm and lax; faults of position, as placenta prævia and antica; wounds; faults of original conformation, as defective, double, and membranaceous placenta. Hardly any original information, either in the way of fact or deduction, is contributed in this thesis. It may be characterized as an imperfect digest of the then current ideas about placental pathology. That Wilde's own notions concerning the diseases of the placenta could not be very accurate may be inferred from the following statement concerning the physiology of the organ:—"Multi placentam distinxerunt in fetalem et uterinam, quæ tamen differentia non existit." Poverty of material is concealed under the imposing garb of the Latin tongue. As a digest of the subject, it is inferior to the article of Murat.

M. Ollivier (d'Angers) relates an interesting case of inflammation of the membranes of the ovum. In 1835, Troll published an inaugural dissertation, 'De Placentæ Morbis.' It is a mere compilation.

The work of Dr. Granville supplies many valuable materials towards the pathological history of the placenta. The descriptions of this author are indeed somewhat obscured by his erroneous ideas concerning the nature of the several membranes of the ovum. He insists that the covering found external to the chorion in early ova, that is the decidua reflexa, is a proper tunic of the ovum, and calls it the "cortex ovi." The effect of adopting this error would be to render futile any attempt to connect the morbid conditions of this structure with the morbid states of the uterus and the mother. Fortunately, however, the morbid ova described are also depicted in most accurate and truly beautiful drawings. We are thus enabled to judge of the exact nature of the structures affected, and of the changes they have undergone, with almost as much certainty as if the fresh specimens were placed before us. These plates exhibit various morbid appearances of the decidua, chorion, and amnion in the early ovum; sanguineous infiltration of the decidua; hydatidiform chorion; thickening and discoloration (inflammation) of the chorion and amnion. The conditions so frequently observed in ova aborted at an early period are well illustrated; but no light is thrown upon the alterations the placenta is liable to at the more advanced periods of its development.

The elaborate essay of Professor Simpson contains a most able analysis of much that was hitherto known, and its value is greatly increased by the addition of many important original observations. The scheme of the Professor was evidently intended to embrace the entire range of the morbid alterations of the placenta, but the portion published includes only *congestion* and *inflammation*. He states that these two are

"The most frequent and important diseased states to which the placenta is liable. . . . The placentary parenchyma, and the membranes investing the organs, are, however, liable to other morbid states—to *hypertrophy* and *atrophy*, to *softening* and *induration*, to *cartilaginous* and *calcareous degeneration*, and the secretion or formation of other morbid products and tissues, to an anormal *cystoid* or *hydatiform* structure, and to various forms of *malformation* and *displacement*; but in a practical point of view most of these lesions are comparatively less important than congestion and inflammation, and the effects which these conditions produce."

We shall have frequent occasion to recur to this essay for illustrations of the various morbid alterations of the placenta. In the clinical lecture

of Dr. Simpson, the title of which is given at the head of this article, he again refers to inflammatory induration and degeneration of the placenta, and to hypertrophy.

The account that Rokitansky* gives of the pathology of the placenta is so summary as to display most clearly the obscurity of the subject. After observing that the placenta may vary as to *size*, *shape*, and *position*, he observes that placental *hæmorrhages* may occur from violence causing separation from the uterus, or lesion of the placental structure. He says that the blood may be either infiltrated in the parenchyma, or collected into foci. The placenta may be also affected with *plethora* and *congestion*. *Inflammation*, which he says is the most frequent of placental diseases, he describes more fully. We shall revert to this part of the Professor's description hereafter. He barely alludes to the *hydatid mole*; refers to bony and calcareous depositions in a few words; denies the existence of *tuberculosis* of the placenta; exposes the misapplication of the term *scirrhus*; and says that adhesions of the placenta to the foetus have been observed. For minuteness and definition, the account given of the morbid alterations of the placenta, by this great pathologist, is in striking contrast with the luminous precision with which he has discussed the morbid alterations of other organs.

Dr. Leet† has, from the stores of that ample experience which has illustrated so many difficult obstetric problems, contributed many facts of interest in placental pathology. He questions the frequency of inflammation. He says that hypertrophy or atrophy, and apoplexy, are amongst the most frequent diseases. He relates in detail the histories of thirteen cases in which the placenta was found diseased. These cases are mostly striking examples of the relation frequently existing between disease of the placenta and abortion. In some instances, although no microscopic characters are given, the general description of the morbid appearances is perhaps sufficiently clear to indicate the pathological nature of the change. But upon the hazardous ground of conjectural interpretation, we have already declared our resolution not to tread.

The subject-matter of the contributions of Gierse and Meckel, H. Müller, Mettenheimer, Schröder van der Kolk, Virchow, of those of the reviewer, and the consequent researches of Dr. Handfield Jones, Dr. Druitt, M. Ch. Robin, and Dr. Cowan, will be more conveniently discussed hereafter.

Having thus rapidly surveyed the principal writings which contain the most authentic and systematic information on the subject, we will now apply ourselves to the task of including in a methodical survey all that may be considered available and trustworthy in the construction of a comprehensive placental nosology, such as may serve to define the boundaries of our actual knowledge, and be suggestive of further advances. We have already pointed out the leading principle in such an investigation, which appears to us the most promising in useful results. 1. There are morbid conditions of the placenta which may originate in its own structure. 2. There are morbid conditions of the placenta resulting from the state of the blood of the mother brought into it, or from contact with

* Handbuch der Specie'len Pathol. Anatom., 1842.

† Lectures on the Theory and Practice of Midwifery, 1844.

diseased uterine structures. 3. There are morbid conditions secondary to disease, or defective developmental force in the embryo. In adopting this plan, we will not disguise the difficulties that must attend the attempt to adhere to it rigidly. It will often be difficult, if not impossible, to determine whether a particular lesion owes its origin to one or more of the sources referred to. But difficulties, perhaps not less grave, would attend any other plan. Some classification is essential; and we are sure we shall meet with indulgence, if we select that which seems to us to present the greatest facilities for developing our views upon so extensive and intricate a subject. We shall not hesitate to diverge, as occasion may require, from a scheme which is adopted only on account of its general convenience.

The lesions that may with the least doubt be ranged under the first division of strictly local affections are, mechanical injuries, such as rupture of the placental tissues, congestion, extravasation of blood (by some authors called apoplexy and aneurism), inflammation, hydatidiform degeneration of the chorion. Fatty degeneration is sometimes primary in the placenta, but perhaps more frequently of secondary origin. To what extent hypertrophy or atrophy are primary is doubtful. Calcareous and osseous deposits are most frequently connected with constitutional conditions of the mother, and their seat is commonly in the maternal structures.

Under the second or maternal division may be included all those conditions which may be clearly traced to abnormal states of the mother's blood. Inflammation probably arises from some such condition. We shall adduce evidence to show that one form of hypertrophy, at least, arises from disease of the mother's blood. Atrophy we believe to be more commonly dependent upon foetal conditions. One form of fatty degeneration, that commencing in the decidual element, belongs especially to this division. That form of this affection which begins in the foetal element, the chorion, may also, sometimes at least, be traced to a maternal cause. Calcareous and osseous deposits are almost invariably so associated. The source of fibrinous deposits may be either the maternal or the foetal blood. The same may be said of serous or dropsical effusions.

The third or foetal division will comprise atrophy; many cases of fatty degeneration of the chorion; some deposits of fibrinous masses; serous effusions; some forms of sanguineous congestion, and of extravasation.

I. We put aside all consideration of the mechanical lesions of the placenta, such as laceration of its structure, as scarcely coming within the definition of disease. The first morbid condition we propose to consider is that which presents the least divergence from the healthy state, namely, congestion. Inasmuch as there are portions of two distinct circulating systems in the placenta, there must also be two distinct forms of placental congestion. The maternal placenta may be congested. The foetal placenta may be congested. Strictly speaking, in the great majority of instances, each of these forms is connected with some abnormal condition of the circulatory apparatus, or of the blood, of the mother or of the foetus. But either form, in a mixed form in which both the maternal and foetal placentas are congested, may, under some circumstances, depend upon simply local causes. In the case of a placenta of an advanced period, it must often be difficult to determine whether the maternal or foetal congestion predominate. In early ova, in which the apposition of

the two portions of the placenta is incomplete, the vascular condition of each admits of being more easily distinguished. We believe it may be generally stated that maternal congestion is more frequent in early ova; and that foetal congestion, or the mixed form, is more frequent in older ova. Foetal congestion, in its simplest form, may be observed in cases of delivery at the full term, in which the child is born alive, the cord having been tied on the placental as well as on the foetal side of the point of division by the scissors. In such a case, the vessels of the cords are seen to be greatly distended, presenting the appearance of varicose enlargements. Tracing the vessels back, a similar appearance is seen on the foetal surface of the placenta. The whole mass of the organ is firm, rounded, of a dark purple colour, and gorged with blood. The vessels in the villi, if examined under the microscope, are seen to be crammed with blood-corpuscles, and enlarged in their dimensions from distension.

The illustration given by Professor Simpson marks a second and more advanced degree of congestion. He refers to the condition of the placenta in cases in which the head of the child has happened to be long impacted in the passages of the pelvis.

“The appearance which the placenta exhibits on its being expelled after such cases, and more particularly if the impaction has been so great as to prove fatal to the child, are well known to every practical accoucheur. The external surface of the organ is of a more or less deep violet, and sometimes almost livid colour; its internal structure, when torn or divided by the scalpel, presents a deep purple hue; its vessels are everywhere distended with dark-coloured blood; the organ appears enlarged, and its substance feels heavier and more solid than natural.”

Rokitansky gives a precisely similar description of the appearance of congestion of the placenta. We witness the counterpart of congestion of the placenta in the intensely livid hue and swelling of the face of the child, arising from stagnation of the blood from long-continued pressure. The cases cited are examples of *foetal* or *chorial* congestion. If we bring to our aid the physiological homology of the placenta, and the air-breathing lung, we shall have no difficulty in understanding how the placenta may be exposed to congestion, inflammation, and effusions from analogous causes to those which induce similar lesions in the lung. In the adult, death by asphyxia is revealed by inspection of the lungs. In the foetus, death by asphyxia is read in the placenta. The true foetal trachea is constituted by the utero-placental arteries, which convey to the cavernous structure of the placenta the oxygenated blood of the mother. If this flow be intercepted, the foetus dies of suffocation. If blood not duly oxygenated, or blood impregnated with some noxious ingredient, is supplied, again, the foetus dies of asphyxia or of poison, just as the adult would perish if made to inhale carbonic acid gas. This is not a matter of theory, but of observation. We are not aware that in the entire range of medical literature an experimental observation more apposite or more interesting can be found than the following. A case occurred, in which we deemed it necessary to bring on premature labour at the seventh month. When the labour had made some progress, the cord fell through the os uteri into the vagina; by holding this lightly in the fingers, we were enabled to feel the pulse of the unborn child. When the uterus was quiescent, the pulsations of the cord were 80 in the minute, and strong.

The torpid uterus was roused to action by galvanism. During every contraction so induced, the pulsations became first intermitting, feeble, and then stopped. Had not the galvanic stimulus been withdrawn, the child must have died of asphyxia. Withdrawn in time, the uterus relaxed, blood flowed again into the placenta, the foetal circulation was again set in motion, and the pulsations returned. Presently, uterine contractions came on spontaneously. The same phenomena were observed. Nothing could prove more clearly that uterine contraction, in compressing the utero-placental vessels, acted in precisely the same manner as a ligature upon the windpipe. In another case of premature delivery, brought about by attachment of the placenta over the os uteri, the following phenomena were accurately noted. In the absence of uterine contraction, the foetal heart was heard by the stethoscope beating 90 times in the minute. During uterine contraction, the pulsations fell to 60. After the expulsion of the child, which was scarcely sufficiently mature to be viable, the uterus contracted firmly, but the cord was not severed at once. The firm contraction of the uterus, we believe, arrested the placental circulation. The child's life depended upon breathing air. It gasped feebly; the heart beat 90 in the minute. The gasp at an end: the pulse fell to 60. Respiration excited artificially: pulse rose immediately to 90; and dropped again to 60 as respiration stopped. And so, for a considerable time, the pulse ebbed and flowed, as respiration ceased or was renewed; and this even after the cord was severed. It will be observed that the effect of respiration by the lungs upon the foetal pulse was precisely the same as the effect of placental respiration had been. It matters not whether the oxygenating medium be brought into contact with the foetal blood through the aërial trachea of the born infant, or through the sanguiferous trachea of the utero-placental system.

Whilst these precise observations, which only confirm the general testimony of physiology, prove the homologous functions of the placenta and the lungs, observations in morbid anatomy tell the same thing. Bayard, Casper, Ritgen, Cruveilhier, Litzmann, Krahmer, Hecker, and others have described the post-mortem appearances in numerous cases of foetal asphyxia, occurring from a variety of causes. The constant appearances were punctiform ecchymoses, scattered over the pleuræ and pericardium, resembling in character those found in asphyxia in the adult. It may seem unnecessary to dwell upon this analogy, or rather identity, of function. But it is not so. The entire physiological importance of the placenta has been very recently called in question; and if the physiology is null, the pathology is null also. Dr. Druitt has contended that—"The placenta being, *par excellence*, a temporary organ, might fairly be expected to be prone to degenerate towards the close of its term of office;" and that—"Incipient degeneration is a normal condition of the placenta at the end of pregnancy." Again, in commenting upon the appearances exhibited in the organs of a foetus which had died in utero, a day or two before the expiration of the full term of gestation, Dr. Druitt pointed out that—

"The mother was in perfect health, and had undergone no illness which could account for the death of the foetus. The placenta exhibited no more than the usual amount of alteration. The abdomen, pericardium, both pleuræ, and the pla-

tinous tissue of the cord, was filled with bloody serum; and *the surface of the heart and lungs was covered with small ecchymoses.*"

He alluded to the obscurity of the causes which produced such a fatal alteration in the character of the blood in this case, and to the necessity of examining the bodies of foetuses. No doubt, the cause of death in this case may seem obscure; but Dr. Druitt, entertaining a very mean opinion of the placenta, or of its use towards the end of gestation, does not hesitate to place that cause in the foetus. But what organ was there in the foetus, the functions of which were of equal importance with those of the placenta? And the placenta was diseased. What were the morbid appearances observed? Ecchymoses on the serous membrane of the heart and lungs—that is, the constant appearances found in death from asphyxia, in death from interception of the relations between the foetus and the placenta. But it is said that the alteration in the placenta was of "no more than the usual amount." What is the usual amount? Is it true that, in proportion as the foetus grows in size, as its assimilative and eliminative functions increase in activity and extent, that the organ which is the chief agent in all these processes becomes worn out, and less and less useful? If the placenta become partially disabled towards the end of gestation, what organ takes up its functions?—for those functions must be performed. And if so, the placenta, in all its integrity, can no more be spared, even up to the very eve of being superseded by the lungs, than can, from that moment, the lungs themselves. In discussing the history of degeneration of the placenta, we shall have occasion to show that the facts upon which Dr. Druitt rests his singular physiological heresy have a very different significance from that which he assigns to them. We proceed with our subject.

If the cord be obstructed, the foetus will equally perish. The obstruction of the cord is the same thing as tying the pulmonary arteries and veins in the adult. Asphyxia results; and the consequence of asphyxia is again seen in the placenta. This consequence is congestion; and the congestion will be both maternal and foetal. But asphyxia may happen by other modes than those we have referred to above. In the same way as pleuritic effusions may compress the lungs, as exudations, inflammatory and of other natures, may render a large portion of the lungs unfit for their function, as degeneration or other alteration of the elementary tissues of the lungs may equally render them unfit for their function, so may pressure, inflammatory and other exudations, alteration or degeneration of tissue, arrest the function of the placenta: the asphyxia resulting may be rapid, or gradual and slow.

Congestion of the foetal vessels, carried to a high degree, may lead to sanguineous effusion. Congestion with certain diseased modifications of the foetal blood, may lead to dropsical effusions, or fibrinous effusions. We have not observed any clear examples of simple sanguineous extravasation from the foetal vessels. Dr. Radford has described cases of hæmorrhage arising from the accidental rupture of these vessels. But this is a different lesion from that we are considering. That state of the placenta which has been frequently called "apoplexy," and which consists in a circumscribed effusion of blood in the placental parenchyma, is essentially a disease of the decidua or maternal side of the organ: we, therefore, for the present, postpone the considera-

tion of it. That dropsical effusion may sometimes arise from the foetal vessels, we have no doubt; and we shall have occasion to describe examples in point. Whether those fibrinous masses, of variable shape and size, so often seen through the foetal membranes on the foetal surface, or surrounding the margin of the placenta, more frequently owe their origin to the foetal or the maternal circulation, we are, at present, unable to decide. That they are not always, or even generally, the reliquiae of blood-clots, as many believe them to be, we think, is certain. The examination of the nature of these deposits will also be more conveniently taken up further on. It may be confidently assumed that wherever bloodvessels and blood are found, there inflammation may occur. We may therefore conclude, that inflammation may arise in the foetal side of the placenta. That inflammation may occur in the peritoneum, and other organs of the foetus, is a matter of constant observation. In so far as inflammation is dependent upon an altered condition of the blood, it is, then, clear that, since the blood in the foetal vessels may be altered in a similar manner to that in the abdominal viscera of the foetus, so inflammation may, in like manner, arise in the foetal placenta. We are not, however, at present, able to verify this *à priori* conclusion, reasonable as it is, by positive observation. A marked case of recent inflammation, exhibiting the ordinary traces of exudation in the villi of the placenta, we have not yet seen. To what extent the fibrinous deposits, so frequently seen on the foetal surface of the organ, or atrophy, or fatty degeneration of the villi and bloodvessels, are to be admitted as evidences, as results, of bygone inflammation, is a point that admits of considerable discussion. Those pathologists who deny the possibility of inflammation in the placenta, and those who look upon inflammation as the principal disease to which the placenta is liable, tracing all other lesions to that as their source, of course see no difficulty in the matter. In their minds, the whole question is prejudged. But admitting that it may be occasionally true, that the morbid appearances referred to are the effects of antecedent inflammatory action, we believe that, in the majority of cases, these appearances are due to causes not of an inflammatory nature.

The following case related by Dance, is an interesting example of inflammation of the membranes:

“Obs. xxi. A workwoman, aged 20, aborted at the fourth month of her first pregnancy, having previously exhibited febrile symptoms. The foetus was of the fourth month, the skin red, and showed no signs of life. The foetal surface of the placenta presented a singular yellowish-white colour, similar to that of the false membranes of the pleura. Scraping removed nothing. This membrane being carefully removed, we found between it and the chorion a plastic layer of false membranes, thin and soft; and even a little true pus. This purulent layer was spread over the entire inner surface of the placenta, but unequally. The chorion was twice or even three times the normal thickness; it was hard, as if scirrhus, and quite opaque; the thickest portions corresponded with those parts where the false membranes were the most abundant.”

A case related by Ollivier d'Angers, seems also to be a clear example of inflammation of the foetal membranes.

“Mad. —, 18 years of age, had reached the fourth month of her first pregnancy without accident, when she fell ill, had constipation, a red discharge, pains in the loins, pain on pressure over the abdomen, and slight fever. On the fifth day there was enormous distension of the abdomen. By rest, low diet, &c., these symptoms disappeared. She was delivered naturally of a living child. An hour

before the descent of the head, a tumour of the size of the fist suddenly came down. It was of a dull-white colour, and was found to be formed by the membranes, which had altogether the appearance and thickness of parchment which has been soaked some time in water. This bag, filled with liquor amnii, remained thus at the valve until it was burst by the descent of the head. The membranes, throughout one-third part, were considerably thickened, whitish, opaque, and villous on their internal surface. The thickened portion was traversed by very fine vessels in the neighbourhood of the placenta."

A thickening and opacity of the membranes, such as Dance and Ollivier have described, are not uncommonly observed. We have met with one case in which the membranes were so thick and strong, that labour was altogether prevented from this cause. The liquor amnii could not be discharged until an opening was made in the presenting pouch by the scissors. No amount of force that the fingers and nails could exert was of any avail. Examples have also been seen of extensive vascularity, conjoined with thickening and opacity; these evidences of inflammation occupying a part or even the whole membranes.

In 1809, M. Mercier published a most interesting paper on dropsy of the amnion, which he attributed to inflammation of that membrane. He relates three cases. The first is that of a woman five months pregnant, who, after being fatigued and overheated, drank a quantity of cold water, and was, in consequence, seized with pains in the pubes and loins, cold shivering, nausea, anxiety, and cough. The pain in the lower part of the abdomen increased, and the hypogastrium became tense and swollen. On the 16th day, the abdomen became greatly enlarged, labour-pains came on, and ten pints of liquor amnii were discharged, and afterwards two foetuses, which scarcely showed any signs of life, were expelled. The foetal surface of the amnion was partially coated with false membranes, and the amnion itself covered with bloodvessels of a rose-red colour. In the second case, the infliction of an injury during pregnancy was soon followed by vomiting, and lancinating pains in the hypogastric region and pyrexia. On the 10th day after the accident, the pains having been relieved by bleeding, the abdomen began to acquire an unusual size. On the 43rd day, the abdomen became enormously swollen, and respiration was laborious. The membranes were soon after punctured with a long needle, and as the water flowed, the swelling gradually subsided; labour-pains came on on the following day, and two dead children were expelled. In this case, about a quarter of the foetal surface of the amnion was inflamed, being of a deep-red colour, and double the natural thickness. The history and morbid appearances of the third resemble those of the two preceding cases.

In commenting upon these cases, Dr. Lee remarks, that "in very few of the cases he has seen, has the formation of an excessive quantity of liquor amnii been accompanied with inflammatory and dropsical symptoms in the mother, and in none did the amnion exhibit those morbid appearances produced by inflammation, which M. Mercier has described." In some cases of excess of liquor amnii which we have observed, we have also verified the absence of inflammation of the amnion. Still M. Mercier's cases are precise in their details. We may admit that inflammation is an occasional cause of dropsy of the amnion; but it is equally certain that excess of liquor amnii may arise from a variety of other causes. We are not of opinion that thickening of the coherent amnion and chorion always

depends upon inflammation; or even that the presence of a layer of fibrin upon or between these membranes is always owing to that cause. We shall hereafter have occasion to discuss the nature and origin of fibrinous deposits.

Of all the diseases to which the placenta is liable, that which is most undoubtedly local and peculiar to the chorion, is the hydatidiform degeneration. There is no other disease, the general appearances of which are so familiar to the obstetric practitioner. When this disease has attained an advanced stage, there is no overlooking it, and no mistaking its nature. In its incipient stages, in some cases of very early abortion, we have, however, known it escape detection, until the chorion was examined with a lens or the microscope. Almost the entire interest, in an anatomical point of view, of this remarkable affection, is centered in the question of its origin. From the moment that this morbid conversion of the chorion begins, the safety of the embryo is assailed. The instances are rare—if, indeed, any such exist—of the birth of a living child in connexion with a hydatiginous placenta. In the greater number of instances in which the placenta has undergone this change, the embryo is found of a size so minute as to be strikingly disproportionate to the bulk of the placenta; and not seldom not even a trace of an embryo can be discovered. The morbid developmental power soon conquers that of the embryo, and usurps its place. The embryo cut off, the diseased placenta continues to grow for itself.

It is unnecessary to refute in this place, and at the present time, the obsolete doctrine out of which arose the name of this disease, which recognized in the characteristic vesicular growths true hydatidic parasites. Since the days of Percy, we are not aware that any observer has seen any evidence of independent animal life in the vesicles of the hydatidiform placenta. No one, we believe, has seen anything to confirm the following statement:—

“J’en ramassai un plein grand verre (of the vesicles), que je portai aussitôt à M. Hermann pour le rendre témoin des mouvements que j’avais aperçus dans le premières; mais la plupart étaient mortes, et les autres si languissantes, qu’elles ne nous donnaient que de très-faibles signes de vie.”

In passing, we would observe, that Percy, being firmly convinced of the independent animal nature of hydatids, chivalrously defended the chastity of two women who were delivered of hydatid moles!

That method of minute structural analysis which has in recent times cleared up so many histological questions, has demonstrated that this disease, like almost every other that we shall have to consider, consists not in the implantation and growth of elements foreign to the placenta,—that is, in a new growth,—but in a perverted development or involution of the proper component structures of the organ. Indeed, we may state it as a general rule, that heteromorphous formations are far more rare in the placenta than are alterations of normal tissues. These last embrace by far the most important part of the pathology of the placenta.

Sœmmering, Lobstein, Andral, Cruveilhier, and Moreau believed that the cysts arose from an obstruction and partial expansion of the bloodvessels of the placenta. More minute observations, however, prove that the view entertained by these illustrious pathologists, as to the genesis of the cysts,

* *Mémoire sur le Part Hydatique: Journ. de Méd. Chir. et Pharm.*, 1811.

is not correct. Rokitansky agrees with all those who have in recent times carefully investigated the subject, in attributing the origin of the cysts to a degeneration of the villi of the chorion.

The hydatidiform degeneration of the placenta presents as striking an example as any that can be adduced of the origin of abnormal or pathological structures, from the perverted or aberrant developmental tendency of normal or physiological elements. In order to understand rightly the histology of the hydatidiform chorion it is, therefore, necessary to observe the mode of growth and extension of the healthy villi. If the shaggy chorion of early ova, of that period when the villi are in a state of active growth in order to form a placenta, be examined under a moderate magnifying power, innumerable bud-like processes of varying shape will be seen springing from the extremities and sides of the perfect villi. These bud-like processes are pyriform, clavate, or fusiform; they vary in length and form, from the mere buds sessile upon the parent-villus to prolongations bearing more and more distinctly the ordinary characters of villi. There can, in short, be little doubt, that the growth and increase of the number of the villi take place through this process of gemmation and ramification—a process that may be most accurately compared to the growth of the roots of a tree. These budding villi are thus referred to by M. Désormeaux :*

“Velpéau has seen in ova of one month, or six weeks, the extremities of an infinite number of lateral ramuscles to present a sudden, rounded, or ovoid dilatation, having the appearance of a vesicula; these vesicles exist also in great number on the continuity of these ramuscles, so that these vascular branches present the appearance of a bunch of currants, or better still, of one of those bunches of vesicles, which constitute the hydatidic mole.”

We have not ourselves observed a specimen of healthy chorion in which the dilated processes existed in the exaggerated form described by Désormeaux. We are disposed to infer that the villi which formed the subject of his observations, were in reality morbid, and in process of hydatidiform degeneration. But if we subtract somewhat from his description; if we figure to ourselves the dilated bodies less numerous and less uniformly vesicular, then we fall easily within the physiological limits, and possess an image of a condition which is often observed in the healthy chorion. Seiler, and Vrolik, who reproduces Seiler's plate, appear to regard these vesicular processes as of normal occurrence, but as not persisting, unless under morbid conditions. Vrolik thus expresses himself:—“*Flocci secundum Seiler a quintâ vel sextâ graviditatis hebdomade, vesiculis terminati, quæ dein evanescent, sed nonnunquam abnormi ratione permanent, et tunc isti spuriae graviditatis speciei ansam dant, quam molam botryoidam vel hydatidicam dicunt.*” The drawing thus referred to is rather rudely executed; but the chorion, as represented, appears to be thickened, opaque, and partially detached—appearances strongly indicative of fatty degeneration. This change may have been consecutive upon the vesicular change of the villi. It seems to us more probable that the vesicular processes in this case, also, were the commencement of a true hydatidiform degeneration, than that they were simply healthy budding villi. With regard to the remark that these vesicles disappear (*evanescent*) at a later period, we would observe that they only disappear by changing

* Dict. de Méd., Art., Œuf.

their form; they are lost as vesicles, because they become developed into villi. That the development of new villi is really effected by the budding of new shoots from the extremities and sides of the old villi, is well figured by Schroeder Van der Kolk. The pyriform extension of young chorion-villi is also figured by Mettenheimer. Virchow also describes this as the regular process of formation. To this we may venture to add numerous observations of our own. We have found these bud-like processes far more constantly, and in greater numbers, in the early chorion and in young placentas, than in placentas of an advanced period of gestation. As we have already stated, they are by no means uniformly vesicular in shape, but pyriform, clavate, fusiform bodies, the narrow part connecting them, like a stalk, with the end or side of the villus; or sometimes the extremity of a villus appears enlarged, and divided into a number of lobes, no contraction of a part resembling a stalk being observed. In perhaps the earliest ovum we have had an opportunity of examining in the fresh state—one probably not four weeks old—the termination of *every* villus exhibited a simple or compound lobular appearance, or distinct projections; some villi had similar processes from their sides. At a later stage of growth, instead of the clavate or fusiform processes, there are seen cylinders of greater or less length, but still bearing a resemblance to the primitive shape in their somewhat dilated extremities: these are young villi. Often upon these, again, secondary processes or buds may be observed. Inasmuch as the rapid growth of the foetus, towards the term of gestation, is ever calling for an increased expansion of placenta, it may be presumed that fresh villi are constantly growing. These buds are accordingly seen on the villi of placentas approaching maturity: but they are far less frequent than in early ova.

Now, it appears that under the influence of a perverted developmental force, these buds, instead of growing into villi, may dilate into true vesicles, or hydatidiform cysts. Such a perversion of growth necessarily involves the destruction of the placenta as a respiratory organ, and the consequent death of the embryo. Those who have examined a great number of ova of different epochs, and who have registered what they have seen, will have become familiar with various appearances which can neither be distinctly referred to healthy villi, nor to hydatidiform degeneration. They will have observed bodies attached to villi which, although evidently of the same origin as the ordinary budding villi, were yet so different in some of their characters as clearly to have failed as villi, and which, nevertheless, were not recognised as hydatidiform cysts. The following case is an example of this kind:—A woman, who thought herself ten weeks pregnant, aborted suddenly. The decidua and chorion were undoubtedly of an earlier date than ten weeks; it was judged from the appearance of these structures that the embryo had perished four or five weeks before the expulsion of the ovum; the decidua had retained some connexion with the uterus to the last. Viewed with the naked eye, the villi of the chorion were dull yellowish white; they did not float out freely; they presented numerous nodular enlargements on the sides and extremities. With an inch glass the enlargements were seen more clearly: they were then seen to be pear-shaped bodies, springing from the sides and ends of the villi by *very narrow* pedicles, altogether resembling hydatids which have shrunk from being kept a day or two. With a quarter-inch these enlargements

were found to contain granular fat, they were opaque, some quite dark. No embryo was discovered. These bodies did not possess the characters of healthy villi, and the great disproportion between their slender stalks and the diameter of the bodies themselves, as well as of the villi to which they were attached, precluded the idea that they could ever assume the function of villi. We will not, however, do more than express a conjecture that these bodies were in course of degeneration into hydatidiform cysts. In addition to the peculiar appearance of these bodies, the villi generally were affected with fatty degeneration. This change may possibly have been the first morbid process, and the cause of the destruction of the embryo, and of the abortion. It is one of those numerous cases which mark the imperfection of our knowledge of the pathology of the ovum. The appearances observed may, however, justify a suspicion that, if these hydatid-like bodies were not really budding villi in the incipient stage of cystic degeneration, they were examples of some other hitherto unrecognised form of degeneration.

But, quitting this doubtfully intermediate state, let us pass on to the clearer forms of cystic degeneration of the chorion. A very clear account of this affection has been given by Mettenheimer.* This very careful observer objects to the comparison made by Cruveilhier of hydatid placenta to a bunch of grapes. He thus draws the distinction:

“Whilst in the grape-bunch there is a central trunk and branches, the latter giving off twigs, each of which bears its berries, in the hydatid chorion the central trunk is wanting, the centre of the whole vegetation being a bladder (the chorion) on whose walls a new generation of cysts is formed, each one of which has in like manner the property of developing one or many daughter-cysts. Berry grows out of berry, and the stalks do not unite berries with principal stems, but berries with berries, and lastly with a central mother-cyst.”

We may here observe, that Madame Boivin† gives a very correct drawing of the common aspect of the hydatid placenta, exhibiting the manner in which the cysts are connected with each other. In this drawing it is seen that the cysts are not attached individually by stalks to a central stem, but that, in many instances, series of cysts are connected together, as it were, by one string, one cyst supporting a second, and this in its turn a third, and so on.

Mr. Paget,‡ also basing his views upon the observations of Mettenheimer, regards the hydatidiform placenta as an example of cystic development. He says:

“A part, or even the whole of the chorion is covered with pellucid vesicles, with limpid contents, borne on long, slender, and often, branching pedicles. The cysts are usually oval or pyriform; their walls are clear, or have minute opaque dots; they may be either simple, or may bear others projecting from their walls. Dr. Mettenheimer has found that the minute dots besetting these cysts are ‘villous processes,’ exactly resembling those of the natural chorion, and growing from the walls of the cysts, either outwardly or into their cavities. In these villi he traced the development of cysts. In their natural state, they may be described as filiform or clavate projections, and composed of dimly-granular substance, in which are embedded minute nucleated cells. In this cystic disease, vesicular bodies may be seen scattered among the cells in the villi, which bodies are distinguished from the cells by their pellucidity, their larger size, and double contours; but from the

* Müller's Archiv, 1850.

† Nouvelles Recherches sur la Mole Vésiculaire.

‡ Lectures: Medical Gazette, June, 1851.

cells to these every gradation may be traced, so as to leave scarcely a doubt that the vesicles are derived from cells deviating from their normal characters. . . . The whole process may, therefore, be probably thus described: Certain of the cells in the proper villi of the chorion, deviating from their cell-form, and increasing disproportionately in size, form cysts which remain connected by the gradually elongated and hypertrophied tissue of the villi. On the outer surface of the new-formed cysts, each of which would, as it were, repeat the chorion, and surpass its powers, a new vegetation of villi sprouts out of the same structure as the proper villi of the chorion. In these begins again a similar development of cysts; and so on, *ad infinitum*. Each cyst, as it enlarges, seems to lead to the wasting of the cells around it; and then, moving away from the villus in which it was formed, it draws out the base of the villus, which strengthens itself, and forms the pedicle on which the cyst remains suspended."

To this account we think it interesting to add Mettenheimer's appeal to the admirable researches of Dr. Hodgkin, for the purpose of drawing more especial attention to the relation subsisting between the cystic disease of the chorion and abnormal cystic development in other structures of the body:

"Hodgkin distinguishes two kinds of cystoid formation: in the first, the young cysts stand out upon the walls of the old ones, without any tendency to grow inwards or to become stalked; in the second, the secondary cysts grow inwards, as pear-shaped stalked growths, from the inner wall of the mother cyst, developing again new progeny; and so forth. In the bladder-moles, it appears, from my researches, that there is a third kind, which exhibits the reverse of the second of Hodgkin. In this kind, the secondary cysts grow in villus-form, from the outer surface of the cysts."

It is not consistent with the plan of this article to dwell much upon the bearings of the diseased conditions of the placenta upon obstetric practice; but the following observations will not be out of place:

Dr. F. H. Ramsbotham, in considering the subject of adherent placenta after delivery, says, "At other times again, but very rarely, the remaining portions of placenta (after removing part by hand, &c.) become the nucleus for hydatinous formations, and more rarely still, I believe, they are actually absorbed." The question thus raised is one of great interest. But we are not aware that any unequivocal case exists, upon which the inference that a portion of placenta left in the uterus, *the portion extracted at the time of labour being free from cystic degeneration*, has subsequently become the seat of cystic disease, can be firmly based. It does not seem very probable that this disease ever *begins* at a late period of gestation. It seems essentially an affection of the young chorion. Beginning thus early, one of the first and surest effects is to destroy the embryo while this is still very small in size. It is consequently found, in the great majority of instances, that when this disease exists no trace of an embryo can be found, or if found, that it is exceedingly minute. Is there a case recorded in which the placenta was found affected with hydatidiform degeneration, even partial, at the normal term of gestation, and concurrently with the birth of a fully-developed living child? We put our doubt in the interrogative form because, although we consider it at variance with the known course and history of the disease under consideration that such a case should occur, we are not willing to hazard an absolute denial. But, assuming it to be in the highest degree improbable that the chorion of the placenta approaching maturity should become the seat of cystic degeneration, how much more improbable is it that a *portion* of mature placenta, remaining attached to the uterus after delivery, should

then take on a new cystic life of this kind? What is the condition of a fragment of retained adherent placenta? The foetal vessels with the villi of the chorion are torn across, and the foetal portion of the placenta—that portion which is the seat of cystic disease—may fairly be said to be organically destroyed. The only portion of the placenta which preserves life is the maternal portion, the decidua or uterine element; and this is not susceptible of cystic degeneration. Whether the difficulties we have raised be valid or not, it must at any rate be admitted that the doctrine enunciated by Dr. Ramsbotham requires to be substantiated by facts rigorously analysed. The interest of the question at issue will appear from this: If it be true that a portion of retained mature placenta may, subsequently to delivery, undergo hydatidiform growth, then one conception will account both for the product of the delivery in the usual course at which a child was born, and a portion of placenta brought away, and for a subsequent delivery at a remote period from the first, at which a hydatid mole was expelled. If it be not true that a portion of placenta so retained can undergo this change, then it follows that, in any case in which a hydatid mole may be expelled after an interval more or less remote from an ordinary labour, *a fresh conception has occurred*. The medico-legal bearings of the case are then of the greatest importance.

One other observation we take the opportunity of adding. It is clear from the account we have given of the histology of the cystic disease of the placenta, that Mettenheimer and others regard it as a disease of the chorion, originating in an abnormal development of the cells observed in the villi. This is equally clear from the figures given by Mettenheimer. But these cells are regarded by Goodsir, and those who follow him, as decidua or maternal. It would thence follow, that the cystic disease of the placenta is not an affection of the foetal element, but in reality one of maternal origin. We are not aware that any pathologist has advocated this view. It therefore appears to us that we possess, in the history of the cystic disease of the placenta, a confirmation of that view which regards the cells of the chorion as an integral part of the foetal portion of the placenta.

Although it is in the highest degree probable that fatty degeneration of villi of the chorion may take place as a primitive condition, we defer the minute consideration of this affection until we take up the diseases of the placenta that may be traced to the mother or embryo. We will simply observe in this place that, according to our observations, a considerable amount of fatty degeneration of the chorion is commonly found concurrently with the hydatiginous degeneration. Case X., recorded in the reviewer's second paper in the 'Medico-Chirurgical Transactions,' is an instructive example of this kind. In most cases it is probably secondary upon the physiological destruction of the villi produced by the hydatiginous change. But independently of any hydatiginous affection, and independently of any morbid influence transmitted from the mother, or of failure of the developmental force in the foetus, it seems not an unreasonable conjecture, that in like manner as by an abnormal excess of cystic developmental force the villi may pass into hydatiginous degeneration, so by an opposite defective developmental power the cellular chorion may pass into atrophy, and fatty degeneration may follow.

(To be continued.)

Robert Barnes.

PART SECOND.

Bibliographical Record.

- ART. I.—1. *A System of Instruction in Quantitative Chemical Analysis.*
By Dr. C. R. FRESenius. Second Edition. Edited by J. LLOYD
BULLOCK, F.C.S.—London, 1854.
2. *A Course of Practical Chemistry, arranged for the Use of Medical
Students.* By WILLIAM ODLING, M.B., F.C.S., Professor of Practical
Chemistry and of Natural History at Guy's Hospital.—London, 1854.
3. *Anleitung zur Qualitativen und Quantitativen Zoochemischen Analyse.*
Von E. VON GORUP-BESANEZ. Zweite Auflage.—Nurnberg, 1854.
Guide to Qualitative and Quantitative Zoo-chemical Analysis.
4. *Anleitung zur Qualitativen und Quantitativen Analyse des Harns.*
Von CARL NEUBAUER.—Wiesbaden, 1854.
Guide to the Qualitative and Quantitative Analysis of the Urine.

THESE four works on chemistry are adapted to special ends, and are therefore very different in scope and execution. The masterly work of Fresenius, it is scarcely necessary to say, treats of the whole subject of quantitative chemistry in the broadest way. It is not necessary to do more than announce the appearance of the second edition, which has evidently been most carefully prepared by Mr. Bullock.

The little work by Dr. Odling is one of more modest pretensions, though in its way of not less use. The preface informs us that it contains an outline of the course of practical chemistry as annually carried out in Guy's Hospital. Inorganic, toxicological, and animal chemistry are discussed in as many sections. We are rather surprised to find under the last head no notice taken of the new modes of analysing urine, by using known quantities of test solutions; and although Dr. Odling may consider that this did not enter into his plan, it would have been a useful addition.

Gorup-Besanez has attempted to do for medical what Fresenius has done for general chemistry. He has given a comprehensive account of the modes of determining the presence and the quantity of the various constituents of the body, and has had especial reference to the wants of the physician. He has also given a very excellent short account of the various animal principles.

The author of the last work, Dr. Neubauer, is assistant to Fresenius, and, under the direction of that able chemist, has compiled a very perfect code of instructions for determining, according to the most

approved and recent methods, the amount of the urinary constituents. We do not observe anything novel in the work. The method of Liebig is employed for urea, although all other accurate plans are also given. Liebig's plan for chloride of sodium, Breed's for phosphoric acid, &c., are all carefully detailed. Three plates of urinary sediments are added, from Funke's Atlas.

ART. II.—*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., Physician to the Hospital for Sick Children. Third Edition.—*London*, 1854.

WHEN two editions of a medical book on a special subject are exhausted in six years, it is a sure sign that that book has supplied a want, and has supplied it well. In the present edition of his well-known lectures, Dr. West has taken every pains to support the reputation they have already acquired. Many alterations are made in the text, and in foot-notes, and the bulk of the volume has been increased by sixty pages. A larger number of cases, too, has been brought to bear on every doubtful point, and the latest authors on every subject are carefully referred to. It is almost superfluous to say, that this work should be in the library of every one engaged in the treatment of the diseases of children.

ART. III.—*A Practical Treatise on the Diseases of the Eye.* By WILLIAM MACKENZIE, M.D. Fourth Edition.—*London*, 1854.

THE fourth edition of this standard work will no doubt be as fully appreciated as the three former editions. It is unnecessary to say a word in its praise, for the verdict has already been passed upon it by the most competent judges, and 'Mackenzie on the Eye' has justly obtained a reputation, which it is no figure of speech to call world-wide.

ART. IV.—*On the Topical Medication of the Larynx in certain Diseases of the Respiratory and Vocal Organs.* By EBEN. WATSON, M.D., Lecturer on the Institutes of Medicine in the Andersonian University, &c.—*London*, 1854.

TOPICAL medication to the interior of the larynx, by means of a sponge, was used by Sir Charles Bell, and afterwards by Trousseau and Belloc, but was first brought prominently forward by Dr. Horace Green, and we may add also, by the author of the work before us, for he was one of the first in this country to put Dr. Green's statements to the test, and he has written several valuable papers on the subject.

Much discussion has taken place as to whether Dr. Horace Green succeeded in really introducing the sponge into the larynx. Dr. Watson tries to prove that the entrance into the larynx can be readily accomplished, and he does most certainly show that bodies larger than the sponge have accidentally passed into the trachea, that the dimensions of the larynx in the dead body are such as to admit the sponge, and that

in the dead body the passage can be accomplished. But we do not see that he has been more fortunate than others in adducing absolute proof that the larynx has been entered during life; and, although we do not deny that this can be sometimes done, we are very certain that it is an operation of great difficulty, and that in the great majority of cases, the operator has simply passed the sponge down the pharynx and œsophagus. In his remarks on this point, Dr. Watson appears to us to have been really unwarrantably uncivil to Mr. Erichsen, who, in his late work on 'Surgery,' has expressed doubts of the possibility of the operation.

Whether or not the sponge enters the larynx, the fact remains that the application of caustic to the upper part of the throat, to the epiglottis, and it may be to the interior of the larynx, is of the greatest service in many troublesome and serious diseases. Dr. Watson prefers the nitrate of silver, but he has used also the hyposulphite of soda and silver, and has found it sometimes useful. The strength of the nitrate of silver varies from ten to sixty grains in an ounce of water.

In acute cases Dr. Watson's experience is thus summed up:—

"In acute laryngitis in which there is no false membrane, the local application of solution of caustic, varying in strength inversely in proportion to the intensity of the inflammation, may be employed with more or less speedy benefit.

"During the exudative stage of true croup, the stimulant application to the part affected is injurious, but when the disease begins to yield to antiphlogistic and other treatment, it may assist in the cure.

"There is reason to believe that, in many cases of croup, there is an active inflammatory stage prior to exudation, in which the disease may be checked by topical and other means, appropriate to such cases.

"(Edema glottidis, whether occurring as a primary disease, or as a complication of other morbid states, is always speedily relieved, and in some cases effectually cured, by the application of strong solutions of the nitrate of silver to the œdematous organ."

In chronic laryngitis and aphonia, the use of the caustic is illustrated by some very interesting cases, but the utility of the remedy is so thoroughly admitted in similar cases, that it would be useless to delay on the point.

Dr. Watson has employed the topical medication of the larynx in whooping cough. This was entirely a suggestion of his own, and it has been attended with the greatest success. He employs rather a weak solution, and touches at first only the pharyngeal membrane; then, after some few applications, he passes the sponge into the larynx. By this remedy alone he is able to give, by putting together M. Joubert's cases and his own, the following most favourable report on the plan:

Cured in two weeks	96 cases,	or	57·4 per cent.
„ in three to four weeks	61 „	or	36·5 „
Resisted the treatment	9 „	or	5·3 „
Died	1 „	or nearly	0·6 „

True spasmodic asthma has been treated in the same way, and with very great success. Although Dr. Watson believes that there is spasm

of the bronchi, as well as of the larynx, in this disease, he thinks that if the latter be overcome, the bronchial spasm soon yields.

In the so-called stomach and hysterical coughs, in laryngismus epileptica (2 cases), and in the laryngeal complication of pulmonary phthisis, the topical medication has been by Dr. Watson, as by others, employed with excellent results.

We recommend this very practical and well-written work very cordially to our readers.

ART. V.—*A Treatise on Diseases of the Lungs, having especial reference to Consumption.* By ANTHONY WILLIAM CLARKE, M.D.—London, 1854.

THE author informs us in his preface, that “the sources of his information have been various,” and “that nothing will be found in his book which will not be verified under the test of trial.” He also states that “independent in circumstances, he commits his brain-child to the world, satisfied if the amiable reader approves.”

It requires greater amiability than we possess to welcome this very feeble and decrepid “brain-child” in the way its parent seems to expect. We have read the greater part of the book carefully, but we can find little that can have sprung from Dr. Clarke’s brain, except at second hand. Page after page is filled with the most familiar facts, and the “experience acquired in a practice of tolerable extent” has just sufficed to make a book, which any man with a few good treatises before him might have compiled. We are sorry thus to speak of a gentleman who is probably a worthy and respectable practitioner, but we cannot speak otherwise, and yet speak truly.

ART. VI. — *Principles of Comparative Physiology.* By WILLIAM B. CARPENTER, M.D., F.R.S., &c. Fourth Edition.—London, 1854.

To call Dr. Carpenter the most excellent of compilers, though no small praise, would be doing him but scant justice. He is much more than a compiler, for he can not only select and combine, but he can investigate and discover. To say that Dr. Carpenter collects the facts which other men have pointed out, is merely to say that he has accomplished one of the inevitable conditions of the proposed work; but to assert that he does no more than collect, is to overlook the numerous original observations he has made himself, and the novel results which he deduces from the observations of others.

We have already had occasion to point out that Dr. Carpenter possesses in a high degree two mental qualities seldom found combined—the industry which collects, and the genius which discovers. In scarcely any former work have these powers been so marked as in the present. In no work that we have ever seen have the phenomena of life been discussed so broadly and lucidly, and in none have so many facts been put more happily and fairly into their appropriate places.

The work is, in many respects, a new one; it consists of only half the former edition, for the author found he could not do justice to his subject without enlarging the volume to an excessive degree. He has preferred, therefore, making separate works of the Comparative and the General Physiology, and the volume containing the latter subject has yet to appear.

We should devote a longer space to this work than we can give it here, did we not feel that no review is really necessary, for it will be read by all physiologists, and, from its comprehensive nature, it is little adapted for an analytical summary. When the other volume appears, however, we shall take the opportunity of including this volume in a comprehensive review on the general position of physiological science.

The work is most beautifully and copiously illustrated, as usual in all Dr. Carpenter's productions.

ART. VII.—*Transactions of the Pathological Society.* Volume V.
London, 1854.

IN announcing another volume of the 'Transactions of the Pathological Society,' we can scarcely do more than repeat the opinion so often expressed, of the extreme value and interest of the facts brought before the society. A marked feature of the work now consists in the reports, which are made on the exhibited specimens by gentlemen especially conversant with those means of investigation by which the nature of the morbid change can be best discovered. In this way some admirable original statements are called forth, and specimens are investigated much more minutely than could possibly be the case if the exhibitor alone examined them. This plan is about to be carried to a greater extent, and the society proposes to investigate, by combined observation, certain obscure or imperfectly known conditions in pathology. We shall look forward with the greatest interest to the results of this plan, and we are confident that in a little time they will not disappoint our expectations.

ART. VIII.—*A Disquisition on certain Parts and Properties of the Blood.*
By DAVID TOD, M.R.C.S.—London, 1854.

MR. TOD's work cannot be considered a common one. We are not certain that we ever before met so many novelties in so small a space. We had not got through twenty pages before we found that the blood-corpuscles, or hæmatozoa, as Mr. Tod calls them, are animalculæ, that they feed on and digest albumen, that they possess great generative powers, which vary, however, according to the time of year, so that we suppose they have their rutting season, and that they turn, or can turn rather, into spermatozoa. Further on (p. 197) we are told, that man was originally generated by a gradual change occurring in the hæmatozoa of a particular class of extinct animals, this change being the result of what botanists call a "sport;" and without entering into any long analysis, we may simply state that the entire book is filled with statements equally remarkable.

Our readers may suppose that this work is a "sport" of Mr. Tod's, but we assure them this is not the case. Our author is in sober earnest, and illustrates his opinions by some of the most singular experiments and processes of reasoning we have ever had the fortune to read. He has perfect faith in his own views, announces them with complete gravity, and although we are afraid he must abandon all hope of convincing any one, he may feel quite certain that he has produced a book which will rank among the curiosities of medical literature.

ART. IX.—*An Expository Lexicon of the Terms used in Medical and General Science.* By R. G. MAYNE, M.D. Parts II. & III.

PART III. of this excellent work takes us almost to the end of the letter H, and we anticipate that the author can have little difficulty in concluding his work in the six parts, as originally proposed. We shall examine the whole work critically when finished, and shall now only repeat the very favourable opinion which the perusal of the first part led us to form.

ART. X.—*Neurologische Untersuchungen.* Von Dr. RUDOLPH WAGNER. Göttingen, 1854.

Neurologic Researches. By Dr. WAGNER.

THIS volume consists of a series of papers presented by Dr. Wagner during the last seven years to the Royal Society at Göttingen, and published in their 'Transactions.' The subjects of the papers (28 in number) are very various, and embrace pretty nearly the whole subject of Neurology. Almost all the facts, however, have passed into the domain of established physiology, since the last paper (on Bernard's Discovery of the production of Diabetes by wounding the floor of the fourth Ventricle) is dated in 1852.

Still this volume will have a lasting interest for all who desire to have a connected account of the discoveries of its celebrated author.

ART. XI.—*Lectures on Polarized Light* By the late JONATHAN PEREIRA, M.D., F.R.S., F.L.S. Second Edition. Edited by the Rev. BADEN POWELL, M.A., V.P.R.S.—London, 1854.

THE great power of research, and the unusual facility in using the materials at command, which were Dr. Pereira's pre-eminent mental qualities, are manifested as much in this small volume as in the gigantic work on *Materia Medica*. The edition before us is much enlarged, not only by materials left by the lamented author, but also by interpolations by the editor. It is scarcely necessary to say that it is a clear yet comprehensive treatise on the subject, and is likely to remain for some time what it now is, a standard work on Polarized light.

ART. XII.—*Summary of New Publications.*

WE have already noticed the majority of the works published up to the end of November which have reached us, and a very brief summary will suffice for the rest.

The thirty-seventh volume of the 'Medico-Chirurgical Transactions' is rather thinner than usual, and although there are some papers of great interest in it, it does not, we think, equal its immediate predecessors.

In *Medicine*, the concluding part of Wunderlich's extensive work* has appeared. Dr. Tanner's compendium of 'The Practice of Medicine' has reached a second edition. Although the descriptions of the different diseases are extremely short, the little work may be useful to a busy practitioner.

Dr. Davies has published a second edition of his 'Lectures on the Physical Diagnosis of the Diseases of the Lungs and Heart.' The most important addition appears to be an investigation into the frequency of venous murmurs among strong, healthy men, such as the Foot Guards. In fifty picked men of the Coldstream Guards, between the ages of 21 and 27, Dr. Davies found a venous murmur in the neck, either on one or both sides, in no less than forty-four. He confirms, then, the statements of Wintrich. The remainder of the volume has been very carefully revised.

Dr. Ballard has written a very elaborate work on a single symptom—viz., 'Pain after Food.' We shall review it in our next number.

The causes and extent of the epidemic of yellow fever which decimated New Orleans in 1853 have been recorded by Dr. Fenner, a well-known writer on this subject. The sanitary condition of New Orleans is described with care; and without denying the occasional contagion of yellow fever, Dr. Fenner is a warm advocate of its propagation in other ways, through the medium of a vitiated atmosphere.

Another American work, relating to a different department of pathology, will also be found to be interesting to a special class. We refer to Dr. Pliny Earle's treatise on 'Bloodletting in Insanity.' A great number of authorities are examined for and against this practice, and the result is, that Dr. Earle believes that insanity *per se* is a contra-indication for bloodletting, but that there may be conditions of plethora, or tendency to apoplexy, which demand it.

A second edition of 'What to Observe at the Bedside and after Death, in Medical Cases' has been issued. We have already reviewed the first edition, and shall therefore content ourselves with this simple announcement.

Almost simultaneously, two manuals of 'Pathological Anatomy' have been published: one in this country, by Drs. Handfield Jones and Sieveking; and one in Germany, by Dr. Förster,† of Göttingen. Both are works of a high order; and we anticipate for the English treatise a wide circulation in this country.

After the review on Electricity was in type, we received Duchenne's long-promised work. It is a bulky volume of 926 pages; but its pith has been already anticipated in the review published in this number. If necessary, however, we shall refer to it hereafter.

In *Surgery*, a large work on 'White Tumours of the Articulations'‡ will be noticed in the next review on Diseases of the Joints.

Mr. Skey has written a short pamphlet on 'The Relative Merit of Lithotomy and Lithotritry,' in which a very decided preference is given to

* Handbuch der Pathologie und Therapie.

† Handbuch der speciellen Pathol. Anat. Göttingen, 1854.

‡ Traité des Tumeurs Blanches des Articulations, par le Dr. J. Crocq. Bruxelles, 1854.

the latter. The subject is too important for us to discuss here, but we shall examine it thoroughly in an early number.

A pamphlet of interest has been written by Mr. Ward—‘On Strangulated Hernia;’ and is based on the experience of three years at the London Hospital. During this time, 242 cases of hernia were admitted, and 69 were operated on—viz., 43 femoral, 22 inguinal, and 4 umbilical; of this number, 21 died. In the cases of femoral hernia (39 of which were in women), the sac was not opened (Luke’s operation being usually employed) in 29; the average period of strangulation was slightly over thirty-four hours; 4 cases died, or 13·85 per cent. In 13 cases, the sac was opened; 6 died, or 46·1 per cent.; the average time of strangulation being fifty-eight hours. This would appear to show that one operation is vastly superior to the other; but Mr. Ward points out the difficulty of comparing the two classes of cases; and although he evidently inclines to Mr. Luke’s operation, hesitates to draw the strong inference which might at first sight be drawn from the above facts. Many other interesting points are discussed in the pamphlet, which, we have little doubt, most of our readers will peruse for themselves.

We have room only to enumerate the numerous other works on various subjects which have reached us, and to some of which we must return. Mr. Child has published three lectures on ‘Injuries incidental to Warfare,’ but has neither done justice to his subject, nor to himself. A second edition of Mr. Hunt’s pamphlet on ‘Syphilitic Eruptions’ would have been more useful had the cases been given in greater detail. ‘A Discourse on Medical Botany,’ by Earl Stanhope, is an interesting and well-written theme on the benefits to be derived from an assiduous attention to the medicinal virtues of plants; and a paper by Mr. Lizars, on ‘Tobacco,’ is a violent tirade against the use of the “grateful weed,” and, like most tirades, is only one-sided. The ‘Watering Places of England,’ by Mr. Lee, has passed into a third edition; a concise account is given of all the places of resort in this country. In a pamphlet entitled ‘Harrogate and its Resources,’ some very valuable analyses, by Professor Hoffmann, are given at length. The ‘Book of Prescriptions,’ by Mr. Beasley, is one of real value to practical men, as it contains numerous formulæ gathered from all sources, and embracing almost all the possible combinations in prescribing. The fifth part of the ‘Micrographic Dictionary’ brings us down to the letter C; there is no falling off from the excellence of the previous parts.

We reserve for special review some other works:—viz., Mr. Henry Lee’s ‘Pathological and Surgical Observations;’ Mr. Swan’s work on the ‘Brain and Mind;’ Dr. Bucknill’s prize treatise on ‘Insanity;’ and two German inaugural dissertations of considerable merit—viz., one on the ‘Adenoid Tumour of the Female Breast,’* and the other on the ‘Tumours which occur in the Course of Nerves.’†

* Das Adenoid der Weiblichen Brust. Von A. Weber. Giessen, 1854.

† Ein Beitrag zur Pathol. Anatomie der Geschwülste in Verlaufe der Nerven. Von F. Kupferberg.

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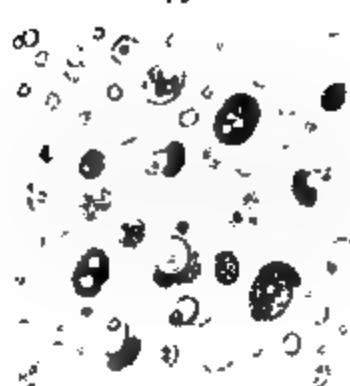
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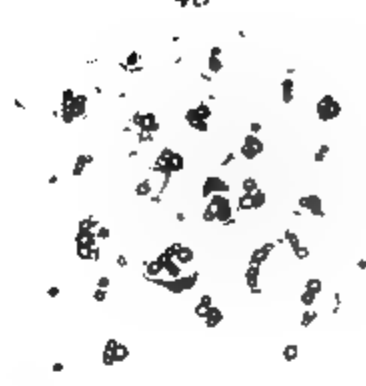
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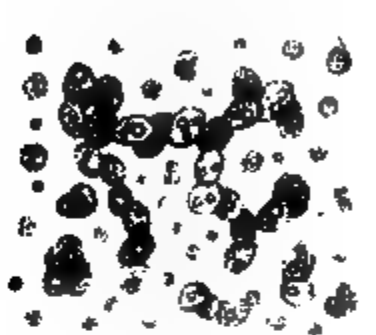
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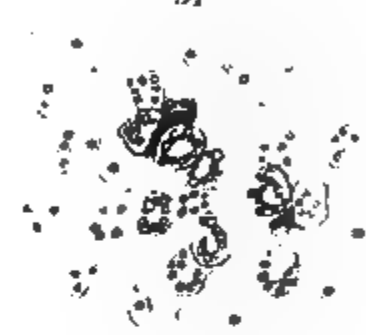
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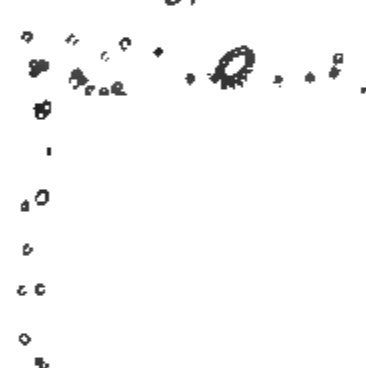
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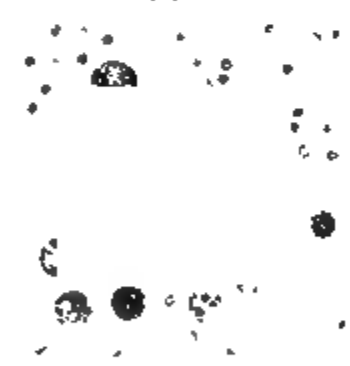
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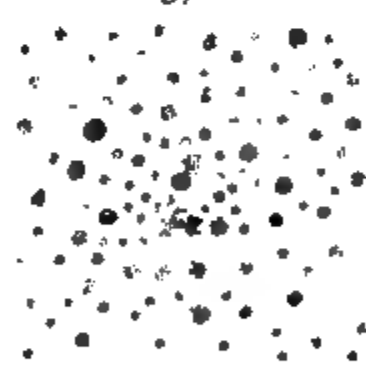
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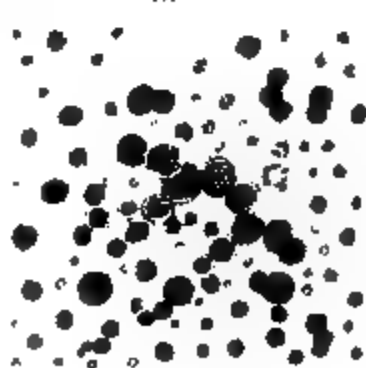
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PART THIRD.

Original Communications.

ART. I.

The Blood—its Chemistry, Physiology, and Pathology. By THOMAS WILLIAMS, M.D. Lond., Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea.

(Continued from No. 25, p. 207.)

HUMAN physiology is only a solitary planet in the orrery of organic science. Detached from the mutually dependent and reciprocally illustrative units of the system, it becomes incomprehensible. Viewed as the *summa philosophia*, the pinnacle of organization, it is the *terminus* on which converge a thousand paths of elucidative research. Every inferior animal successfully dissectionized, uplifts some veil, dissipates some mystery, unlocks the long-chained cavern of some new springs of revived thought, in its utilized reference to the science of the human organism. Who does not hear, in this methodological principle, the voice of admonition? Does it not point the way? Does it not exhort the child to walk first? To the philosopher does it not speak, begin where nature is intelligible? Examine the whole animal kingdom as an indivisible multi-une machinery; regard the consecutive members of the series as stages in the growth of one animal; contemplate it first in its living totality. Does it not bear the impress of a dynamic, as of a material unity? Does it not look like one body obeying the dicta of one life-power of commensurate stupendousness? Yes. But where is the painter so gifted as, even in faint and dubious outline, to reduce to visible form a conception so splendid, a picture so grand? Was it not traced by the master-hand of the great physiologist of antiquity? In his '*Περὶ Ζῴων Ἱστορίας*,' did not Aristotle forefigure the true principles of animal classification? Did he not pile Ossa and Olympus on Pelion, in order to scale the battlements of his mythic heaven? Did he not superimpose the zoophyte on the plant, the star-fish on the zoophyte, the fish on the mollusc, and the mammal on all, in order to make a man? Inanimate matter grows into the vegetable form, and the latter rises to the excellence of an animal: *that* is the philosophy of Aristotle. It is the primæval germ of all later conceptions as to the "unity" of organization. It is, indubitably, prophetic of the tendency of modern science. This very sentiment, in its unavowed essentiality, haunted the secret aspirations of Cuvier. Though repudiating the possibility of linking the countless members of the organized world into an ascensive chain of linearly or circularly successive sequences, he unconsciously superscribed his monumental '*Règne Animal*' with the presiding

conviction of his mind—his secret *regulus philosophandi*, “*distribué d’après son organisation!*” Goëthe and Oken, Geoffroie St. Hilaire, Lamarck, and an army of less distinguished thinkers, have since preached upon the same cosmogonic text, upon the doctrine so monosyllabically expounded by Ben Jonson:

“ Nature doth first beget the imperfect, then
Proceeds she to the perfect.”

Plato and Socrates shaped into immortal form an identical idea. The scheme of creation, the archetypal idea, existed in the mind of the Creator anteriorly to the first act of zoo-genesis: such was *their* philosophy. They pretended, as the modern world-creators have fondly pretended, to have discovered the peep-window commanding a perfect view of the wondrous panorama. The author of the ‘*Vestiges*’ professes to have seen the workman at his cyclopean work; to have caught him handling the primitive, plastic clay, the formless *nebulae*; then to have tracked him in his artful evolutions, until at length his mystic labour issued in the apparition of the *summa*, the Caucasian type-man! These visions are profane, because they falsify the true *method* of nature. Such is the indignant criticism of the clear-minded Professor Sedgwick. It is intolerant to argue in such a manner. What! Is it irreverent to catechise creation as to whence it came, or whither it goeth? Is it profanity to study the *method* of things? Then it is impious to observe the signs of things; to gaze, though with awe-stricken humility, upon the soulless phenomena of this mighty world! All hostile eloquence is vain. The human mind ever has yearned, is yearning, and will for ever intensely yearn, after a perfect knowledge of the *reason* and the *meaning* of visible things. To reveal the order of causation is the sweetest fruition of the finite intellect. The passion will never cease to burn.

Modern science has shaped this great cosmogonic controversy into three grand parallel arguments: 1. The geologic; 2. The embryologic; and, 3. The zoologic. Many individual links of these chains respectively remain to be forged, by discoveries yet unaccomplished; but every living philosopher must really feel that there runs the golden thread of *unity* severally through these series. The conviction cannot be parried away: it reappears, like an all-pervading divinity, in every insignificant event. It must be true. The appearance of living beings in geologic time is the most vulnerable point in this beautiful tripartite argument. It is vulnerable only when contemplated from the insecure eminence of presumptuous theories. Let it be granted that the vertebrate *did* appear on the theatre of objective realities before the invertebrated animal: what does it disprove? Most certainly not the *serial* method of creation—not the consistent homogeneity of the principles of organization. Though only man may at this moment flourish on the scene of this sublunary world, can it be argued that the Diatomian was not *intended* to be lowest of all animal forms—that is, was not the normal continuation downwards of that organized series whose uppermost members appeared first on the theatre of created beings? Though the highest fish may have preceded the lowest in the palæontological scale, it only proves that the highest was not formed out of the lowest; it does not militate against the doctrine of *linearity* in the march of organization. If the alleged facts—viz., that

the geologic series are contradictory of the idea of progressive development—be true events in nature, they really only demolish the hypothesis of the transmutation of species. If the archetypal scheme was *mentally* perfect from the beginning, the arrangement was not disturbed, though the fish may have anteceded the cephalopod. Let it be conceived that the physical conditions proper for birds occurred *before* those consistent with fish-life, it does not annihilate the Platonic idea that, in the unwritten plan of cosmos, the fish was normally predestined to stand below the bird. It only erects into loftier eminence the orthodox argument of *volitional* creation; but it lends support in essence to the *principium* of spontaneous generation. A species is evolved only when a certain definite assemblage of material conditions is realized. Inorganic forces are transmuted into the “vital principles,” in accordance with the modern doctrine of “the convertibility of forces.” Such “conditions” conspire to produce a result. The conditions are the *final* secondary causes. The word “spontaneous” is an inapposite expression. It invests conditions with creative, volitional spontaneity. The inference is a vitiated, corrupt *lemma*, unfairly planted on splendid premises. In all nature, living and unliving, do not one class of events conditionate other events? Do not the latter rest on the substratum of the former? “Spontaneous generation!” It most certainly never was intended that the mind should *terminate* in the word “spontaneous.” If it be signified, though only by conventional implication, that all “conditions,” all “final and secondary causes,” are swayed, directed—aye, created, by a fore-going, fore-being primal Power, it does not contravene the *principia* of “spontaneous generation,” it does not destroy the *idea* that the Creator should act, not by partial but by general laws, should *create*, that is, through the instrumentality of directive “conditions.”

The plan of the animal kingdom is unquestionably founded on the idea of consecutive progression, not *necessarily progression upwards*. The march may be *downwards*, in the direction of simpler organisms; or, laterally, into dependent, but divergent lines. The lowest members of this progression, *when present*, are the simplest, the highest, the most complex. It is palpably true of the vegetable kingdom. Though not capable of proof in all its details, the *principle* of ascensive seriality is true, also, in palæontology. The argument of serial evolution is, emphatically, cumulative. Every discovery adds to its cogency. The word *comparative anatomy* originated with Condorcet, the French metaphysician. He saw in it deep, unreachd meaning. In persuasiveness, it is every day increasing. The “serial homotypes” of Sir Charles Bell and Professor Owen are the riper fruit of the same far-extending conception; without it, neither comparative anatomy, nor comparative histology, nor the vital study of comparative physiology, can have any pretensions to a secure rank in the ennobled hierarchy of the sciences. But let all this be most earnestly explained. The alleged progression of grades in the zoological, or geologic, or embryonic scale, involves one question; continuity of type in organs and their histological elements totally another. The latter thought is new, the former is as ancient as Plato, and Aristotle, and Socrates. The fish bears not the faintest semblance to the likeness of the bird. The two animals are irreconcilably diverse; but in ultimate constructional

elements, the liver of the former is identical with that of the latter! Postpone the details of the argument. Throughout the extant kingdom of animals, there prevails a recoudite conformity of *structural principles*, which theorists cannot, at this age of the science of observation, dare to dispute. But has this law of *histological gradation* ever yet received a definite expression in language? Never. No physiologist has succeeded in up-raising the shapeless mass into the luminous eminence of an undeniable verbal proposition. It will be accomplished. If species meet in *genera*; if genera, amid manifold outward diversities, centre in the type of the *order*; if orders converge in *families*, and families range into sub-kingdoms; if, in the growth of the mammal embryo, zoological types be assumed, though only transiently and foreshadowingly; if, in the fossiliferous series, there be traceable a chain, though its links be numerously broken, and its linearity interrupted; if there be, indubitably demonstrable, a serial homotypism in "limbs"—the outward and visible members of the body;—why should there not be graven on things more deeply hidden—the internal vital organs—the impress of the same seriality?

But the critic, arrogant in the fog of a little German lore, will exclaim—"Why! the 'teleology of organs' conveys a full expression of such an idea. The fin of the fish is the teleologic antecedent of the arm of man—the same organ, modified according to the exigencies of special demands: *that* is the 'teleology of organs!'" It is *not* the idea of seriality which is now to be propounded. The doctrine of histological development lies at the foundation of all phenomenal externalities. It preserves the constructive symbol of unity beneath the deceptive exterior of varieties. It is the living clue of the Maker's finger, pointing the path through a world of wonders. Let it be travelled, but travelled with the humility of earnest science.

Every vital organ is the sum of component units, which arithmetic cannot count. The units, marshalled in a fore-planned order, issue in a premeditated action—the function. In the totality of the organ, the constituent integers are lost: just as the several organs forfeit individuality in the unity of the entire animal body. The doctrine which recognises a singleness of intention in the manifold elements of which the body of an individual being is fabricated, concedes in essence the doctrine which sees, in the entire sphere of animality, a constancy of purpose, a conformity of architectural style. Think of a four-footed animal without lungs, or of whose organization a liver formed no part! Is not the mind instantly seized with the idea of its impossibility? The supposition of an animal destitute of an organ essential to the *sum* of the organism, is as offensive to science as that of a planet in the orrery which, in its revolutions, sets at nought the physics of Newton. The sense of the *necessity* of a certain and definite *order* in events, a consistent conformity in the method of things, is as strong in the former example as in the latter. But all this is contemptible from its familiarity. It is one of the platitudes of vulgarized knowledge. Yes. But there are depths below this charted surface into which the plumb-line of science has never yet descended. Can the geologist make answer to the question, *why* the first mammals of the earth, the famed marsupialia of Stonesfield, appeared at a particular spot in geographic space, and a particular epoch in geologic time? Rea-

soning from final causes, he could only say, the "conditions" favourable to marsupialian life culminated in that period. Does such an observation add one single substantive fact to the treasury of real knowledge? Not in the least. It is the shadow without the substance of reply. Ask, in the phraseology of Dr. Alexander Braun,* *why*, at the age of six or seven years in human childhood, the phenomenon of rejuvenescence of the teeth occurs? Physiology is impotent in presence of such a question. The conjectural reply would speak of "conditions," advancing nutrition, period of growth, &c. The querist remains unsatisfied. The teeth of the second dentition are of a higher order of growth than those of the first. The permanent mark an advance in organization upon the temporary. But the former were not evolved by a transformation of the latter. The temporary were not first subjected to a reducing crucible, in order that their incandescent dust may be moulded into the permanent growth. In the structure of both tiers, there are exhibited conspicuous affinities. The identity of the *plan* on which both series were constructed admits not of a moment's denial; and yet both arose out of a common centre, from a common basis, as perfectly independent growths. The second in seven years after the first. This is not "rejuvenescence." The first did not *renew* the term of its life by, and in the evolution of, the second; nor was the second the product of the dissolution of the first; it was not a *redivivescence* of its former, but a new creation. The word "rejuvenescence" is admissible only as a poetic designation of a large class of natural phenomena. It is doomed to mystify and misguide, as a philosophical expression. It is chargeable with the *suggestio falsi*. With what consistency can it be said that the lungs of the frog are *the* gills of the tadpole rejuvenised? In fact, the doctrine of rejuvenescence is essentially synonymous with that of the transmutation of species. If one organ can grow out of or can be transfigured into another, there can be no valid reason for rejecting the hypothesis of the transmutability of species.

What is the difference between the idea of rejuvenescence as applied to the lungs of the amphibia, or to the teeth of the child, and to the *neider-blatt* and the *hoch-blatt* of the Berlin botanist? If the cotyledons are rejuvenised (reproduced) in the high leaves, the act is one of transmutation. But are not the phenomena susceptible of a more intelligible, if not more exact, analysis? Could the act of the inflorescence in a plant, which is the crowning event of the year's cycle of actions, be accomplished, if *not preceded* by all those under which the leaves are produced? Could the permanent teeth be evolved without being antecedent by the temporary, or are the external gills *necessary* preparations to the development of the lungs in the amphibia? Are the caducous branchiæ the *progenitors* of the lungs? Are they not a single factor in an assemblage of agents conspiring to generate new physiological conditions, amid which lungs are perfected, if not formed? These physiological positions are really parallel. To accept the idea of rejuvenescence is to receive a euphonistic but hollow word; it presents to the mind nothing that is substantially tangible; it awakens a beautiful but bodiless vision. What more than a graceful fiction is there in the assertion that in cosmical transmutations fishes prepared the earth for, and were repro-

* Botanical and Physiological Memoirs, Ray Society, 1854.

duced in, birds, the second vertebrates in the geologic series? This is not severe science; it is sickly sentiment, because worthless.

The phenomenal sequences which occur in a single vital organism during the progress of growth, are not readily comparable with those which mark the advance of a series. In the case of the single individual, one event is traceable into connexion with the next. The thread of causation can be followed; such events are parts of a whole. The presence of the permanent organs of the body *imply*, with all the force of a physiological necessity, the preparative foregoing of the disused parts of the foetal phase of life. Here there is an intelligible dependence, as well as a sequence, of events; but in individuals of a series, the *connexion* of events cannot be established. Who can venture to affirm that the fishes of the first were *parentally* related to those of subsequent periods? In the extant scale of animals, it is easy to suppose the existence of birds without reptiles, or reptiles without fishes. *But when the series is completed*, the symbols of consecutive conformable seriality are indubitably discernible on each successive link. These symbols are not written merely upon the outward surface of the entire animal body, but upon the inmost elements of each constructive organ.

Contemplate the kingdom of animality under the character of a triune system. In animal organization three classes of parts are distinguishable; they are anatomically distinct, but physiologically inseparable. The system of the *exterior solids* is descriptively, but not chronologically, the first; that of the fluids is the second; that of the *interior solids* is the third. Let three distinct, but parallel, lines be carried throughout the zoological chain from man to the sponge; these lines will be unbroken! Unbroken constructively, histologically! They break only where they terminate. Each of these lines is divisible into links or stages. The stages or gradations of one line *coincide in place and time with those of the two others!* The viscera, the productive glandular organs of the body, constitute one series; the fluids, the next; the organs of animal life (Bichat) the third. The visceral series is resolvable into several component lines, of coincident progression; the fluid and that of the exterior solids are reducible probably to the same number of lesser but co-ordinate series.

At the present stage of this exposition, it is quite immaterial to inquire what may be the order of precedence among these three systems of parts; when that question shall have been satisfactorily answered, another *principium* will have been added to the true laws of organization. Faithful demonstrative history must precede the mental act of interpreting. The *modus in rebus* is an after discovery, the product of judicial induction. The *fact* that along the paths of organic progression, whether tracked downwards in the direction of forms of life, which successively simplify, or upwards in that of those which ever multiply in completeness, certain great steps, signifying ascent or descent respectively, are periodically encountered by the anatomist, is a remarkable but clearly-written chapter in the science of comparative anatomy. But the signals of ascent or descent of *one* of the three classes of parts of which the sum of animal organism consists, run parallel with similar signals of change in the other constituent systems of the animal frame. What is the physiological

meaning of this *coincidence*? Has this question ever before been clearly propounded in physiology? Never.

It is not solitary *events* in the lineal families of living beings which demand to be recorded in history. The *desideratum* in philosophical physiology is to know *why* certain grand movements in the *standard* of the animal organism are experienced at one and the same time and place by several of its organological elements. Without such information as that implied in this inquiry, comparative anatomy can never attain to the consummation of a science, to a knowledge of the *material* laws which preside over the process of organogenesis. Thick events crowd into *nebulae*. The clear sight of gifted genius is required to resolve the mist into its intelligible elements. The *fact* that an event does really happen is first to be achieved; demonstrate first, then generalise.

What signify the following illustrations? The *Amphioxus* is the lowest vertebrate; it is furnished with every vertebrate organ but the spleen. This fact, stated as an isolated event, has no significance; it means nothing; it has no value in science. *Connect it with this extraordinary fact, that the blood of the Amphioxus, though corpusculated, is colourless!* Now travel, but with circumspect and vigorous logic, along that path of thought, towards which this single but remarkable illustration so significantly points. Is the blood colourless *because* the spleen is absent? Standing apart, this question admits of no certain answer. Present it in a reversed form; is the blood colourless in any known animal in which the spleen is present? It is.* Then the predicate of the syllogism is logically inevitable, therefore the spleen is the generator of blood-pigment. The question demands a far more extended examination.

Every animal body is composed of two distinct and separate classes of solid organs. 1. Those of animal life; 2. Those of organic or vegetative life (Bichat). The blood stands intermediately between these two classified moieties of the organism. The viscera make the blood, and the blood makes the solid systems of animal life. Whether this relation be wholly or only partially true, it was formerly argued at length that every advance in the animal series which occurred in the nervous and muscular and sensual systems, was marked by a contemporaneous change in the chemical and morphological characters of the fluids. A new meaning was thus imparted to known events. A novel doctrine of animal seriality was unexpectedly established. It became incontrovertibly conspicuous that the sequences of organic development were directed by immutable laws, lying deeper than the surface of things. But is it possible that the jurisdiction of such laws can be limited to the systems merely of animal life? Far from it.

The physiological connexion which subsists between the visceral systems of the organism and the fluids, throughout the chain of organized nature, is a perfectly untravelled tract in comparative physiology. It is thickly planted with the choicest fruit-trees. It is a virgin garden, whence may yet be drawn votive offerings acceptable on the altar of science!

* It will be afterwards proved that the colour which occurs in the nutritive fluids of the vertebrate animal is totally dissimilar, if not chemically, assuredly morphologically, from that which characterises, with one or two exceptions, the blood of invertebrated animals.

The *exterior solids* of the body consist of the muscle, nerve, skeletal, tegumentary, and sense-series. Every index of standard change inscribed on either of these series, histologically, is *preceded by equivalent* marks of change in the system of the fluids. It is erroneous to suppose that the nutrimental, ministering fluids of the body represent a *single* incomplex organ. They are constituted of manifold elements. They perform different and varied functions. They should, both in number and office, represent a congeries of organs. The system of the floating corpuscles would denote a separate organ, the albumen another, the fibrine a third, the salts a fourth, &c. There is traceable in the agency of each principle or element a linear individuality of direction.

It follows, from this separateness and independence, that one or more of the elements may be withdrawn without destroying the physiological unity of the remainder. The same law precisely is applicable to the instances of the solids. The increment or decrement of the vital organism on the standard scale is respectively accomplished in one of two modes. 1. Increment is effected by the *creation* or the superaddition of a *new* organ to the systems of those already present, decrement being the converse subtraction; or, 2. An organ, or principle, or element already existing, is raised in the series by the homogeneous involution of its constituent parts; the added portions being more or less structurally identical with the pre-existing substratum. This is simple growth, *to which, however, there is an organic limit*. For the latter mode there are no marked variations of type; the degree only is multiplied. This law of graduated progression, by the superaddition of similar or dissimilar elements to a typical basis or nucleus, affects as deeply the organic principles of the fluids as the grosser organs of the solids. Fibrine, in the form in which it *first* appears in the scale of the fluids, is as different from fibrine in the phase of its highest preparation, as the lowest muscle-cell is from the highest. Albumen also exhibits a similar scale of graduated composition. It is of the utmost importance to the future progress of physiology to understand that *secreted products* differ with the machinery by which they are elaborated. This is self-evident. A simple organ, reacting on simple fluids, must obviously produce results of corresponding simplicity. It is materially and chemically impossible that the bile of the lowest animal can agree in composition, either in the quality or number of its constituents, with that of the highest. The incomplex, non-nucleated bile-cell of the echinoderm, or the annelid, in presence of the blood of the mammal, could not so marshal and rearrange the affinities concerned as to produce echinodermal or annelidan bile. Discordance between the means and the end jars on the reflective mind at every step. What conclusion does this common sense reasoning constrain? It seems irresistibly evident that an isolated, detached, single event *cannot* happen in the history of the organic elements. A new organ is invested with new functions. It supposes creative antecedents.

Arbitrary events, like arid facts, are utterly infertile. Until utilized, they are useless lumber in the archives of knowledge. The blood-proper system, as formerly announced, first occurs in the zoological scale at the echinodermata. *Why* does organizing nature, at this particular step in the ladder, enact this grand effort of a new creation? Is not the *creating*

of a *new organ* as great a marvel, a miracle, as the fiat of a *new animal* into being? Yes, most reverently! But stop!—Is this act of creating a *new living organ* illustrative of the doctrine that a something has arisen out of nothing? and that only by the interposition of the invisible will of an invisible Artificer? Human reason turns away with sorrow from the impious littleness of such a question. Is such an event marked by the occurrence of no other *connected* events in the organism? If it be not, physiology is not a science, and organization is not under the governance of the principle of typical progression. The antecedents of a material event must be material. A grand organic power cannot arise and exist in the living body, without leading to sequences also material. Every phenomenon of growth or evolution must be the *product* of anteriorly operative conditions. What conspired then, in the instance of the echinoderm, such that a new system of fluids came into existence? It is at this class that the visceral cavity, charged with the chylaqueous fluid, is first closed into a shut, independent space. At this stage the open fluid or phlebenteric series terminates. The biliary system augments in proportions. It lines the large tracts of digestive surfaces presented by the cæca. Every portion of chyme in these cæca must traverse the living walls osmotically in order to gain the splanchnic chamber. In this passage it blends with the product (bile), and receives the biochemic impress of a living solid. Thus is elevated its organic standard; thus, too, is raised its nutritive value.

It is capable of doing new work, of *creating* new systems. A changed arrangement in the apparatus of the *interior* solids, the digestive viscera, entails on the chylaqueous fluid an improved composition; the latter ministers to the apparatus of the *exterior* solids, and unexpected results are accomplished.

Let the foregoing example suffice to illustrate the methodological principle, which it is designed to follow in these investigations. Every phenomenon annunciative of an organic change in the anatomical place or chemical composition of *the fluids*, must have been preceded and conditioned by indications of altered standard in the productive apparatus of the *interior* solids, just as it is succeeded by symbols of advancement in the system of the *exterior* solids. But is not such a proposition liable to the charge of vicious circularity? If the viscera generated the fluid, and the fluids produce the muscles and nerves, &c., what formed the viscera? All physiological reasoning partakes of the faults of the circular logic. The cycles of phenomena are so labyrinthically interblended, that neither a detached beginning nor a non-re-entering result can be discovered in the mingled rounds of vital operations. The gastric fluid may be said to be the beginning or the end of a thick tangle of events. 1. It is produced by the blood—the end. 2. It constitutes the initial term of a succession of actions, of which the blood is the final product. This is undeniable. Henceforth, nevertheless, the organic sciences must march along the highway of demonstrative truth. To establish the *method* of sequence, to prove that events happen *in connexion* with others, is to enhance the value of all, and clothe them in the garb of a higher significance. It indicates a new path of investigation. It suggests a principle for the classification of phenomena, from which comparative anatomy will receive an ennoblement of rank.

The systems of the *interior or visceral solids* are reducible into several apparently independent and unconnected series. The *alimentary or digestive apparatus* enjoys the widest range of distribution: it begins at a lower point in the scale than any other system; it is present when the rest are either entirely absent, or present only in a dubitable rudimentary form. It will be the object of this investigation to determine whether the phases of mechanical type, of general conformation, which this apparatus discovers as the succession of classes are tracked upwards, are accompanied by co-ordinate symbols of elevation in the system of the fluids. Phenomena, hitherto deemed incomprehensible, because viewed in their unrelated isolation, will thus be drawn within the cycle of necessary and orderly sequences.

The *biliary system*, followed throughout the phases of its evolution, but with constant reference to the development of the fluids, will conduct to a knowledge of new phenomena in the serial history of organization. The liver is wide-diffused. It is said to be a conspicuous feature of the lowliest organisms. It must exert a preponderant influence over the generation and organic standard of the nutritive liquids. The search from this exalted point of view will lead to unexpected results.

The *organs of breathing* constitute another visceral element of universal presence. In the sphere of its distribution, and in importance, they rank next to the biliary. Examined in relation to the fluids, they will point to a new sphere of thought.

The *renal system*, in its reaction upon the liquid nutritive media of the body, offers a special but fruitful line of study. It has never been contemplated from the eminence of this novel conception. Insects and arachnida are the only invertebrate animals in which it exists. It is not present even in the cephalopod.

The *reproductive organs* fall scarcely within the category of the fluid-making series. They belong more properly to the sphere of the exterior solids. They are rather products than producers of the fluids. Every *germ-cell* (ovum) is histologically identical. The sperm-cells are as numerously varied as the *species* of the animal series. Unequals, added to equals, give unequals. The germ-cell of every species numbers the same constructive elements. The vitelline capsule and its granular yolk substance, the germinal vesicle, and the germinal spot, *are present in all*. Wherefore this uniformity? How is it that unequals produce equals? How is it that the zoophyte, at the furthest extreme of the kingdom, whose fluids are of the least complicated order, can form an ovum which discovers an identity of structure with that of the mammal? Does not this illustration at once destroy the doctrine which claims for *the fluids* a graduated scale of Development? It does so apparently, but only apparently. An *ovum* is a nucleated cell. The simplest and lowest nutritive fluid is capable of generating so simple an organ; and only such elements of the fluids, in the instance of the higher animals, are withdrawn in the production of the ovum, as are required to form a simple cell. Familiar facts, concatenated under the guidance of a new theoretic scheme, acquire a new value.

The *spleen* is emphatically a vertebrate organ. Among the invertebrate it has no equivalent or representative; it is not required. It fulfils

an office for which no demand exists in the inferior half of the organized series. It bears an intimate relation to the blood-producing actions. As formerly stated, it is absent in the lowest fish—the *amphioxius*. Its absence is coincident with the non-existence of colour (hæmatosine) in the blood! *The correlation of events enhances their individual values!*

The *pancreas* is said to be present in gasteropods and cephalopods. No other class among the invertebrata is even supposed to possess this organ. It exists in every vertebrated animal. The truth is, that no representative of the pancreas can be found in the invertebrate organism. Let the philosophic reader pause to reflect over *the meaning* of these wondrous epochs in the history of *serial organo-genesis*. Are they not *creative epochs*? At successive stages has he not witnessed the “spontaneous generation” of a system of living organs which, in the chain of organized beings, had no existence before? Is he not startled into admiration? Does he not feel that a cloud is being banished from the horizon of his view? But let him well remember that these are not acts of creation *ex nihilo*. The building materials are first prepared, *the act*, the gross, visible consummation, is then accomplished. Nothing is here *transmuted*. No fusion or confusion of two beings or two organs in order to make a third. Nature’s method consists in the gradual accumulation of such physical circumstances as are essential to the production and maintenance of a new order of things. A complex assemblage of casual conditions are seen to converge upon an undeclared end. The end is at length announced, and the act of creation is accomplished. The march of the preparatory processes is obvious to the eye. The mechanism of the creative act is literally legible to the understanding.

Now, in what essential respect does the organic fact of the *first appearance* in the scale of serial organization of a new and independent system of living *organs*, differ from that of the creation in geologic time and place of a new genus or species of animals? Only in this: that in the former instance, the mechanism of the act is resolvable by the analytic power of science; while, in the latter, no approach can yet be made to a knowledge of the productive conditions. *Faith* in the uniformity of natural laws constrains the most reverent thinker to believe, that between the two acts of creation there must obtain an intimate analogy. It is impossible to imagine that events so analogous can be accomplished by irreconcilable modes.

The successive superimposition of elements or organs to the system of the individual in the upward march of growth, is really, in mode, and type, and essence, the same as the consecutive addition, in a conformable manner, of new members, independent beings, to a series of orderly types. In the instance of the specific chain, however, the cumulative method, which is necessary in the perfecting of a single being, from infancy to maturity, is not an irreversible method of creation. The highest or a middle member indifferently may appear first. Each member is a detached and independent individual, although constructed in intimate conformity with those juxta-posed. No two, though joined by similitude of model, are mutually convertible.

Imagine two ideal beings; in one is figured an invertebrate, in the other, a vertebrate animal. In what organic particulars are they diverse?

The organism of the invertebrate animal is compounded of a digestive, biliary, reproductive, and respiratory systems; in the case of the vertebrate type, *three* great classes of organs are superadded to the number of those required to form the simpler model, the renal, pancreatic, and splenic series of organs suddenly rise into existence. Could such material machinery breed arbitrarily in the vacuous womb of nihilism? Are they a mystic progeny without progenitors? Why is it that science has never moulded into intelligible language these most natural questions? Is there not in this *mode of working* distinguishable, in the clearest manner, the principle of progression, of typical seriality, of rising from things little to things great, from lower to higher standards of organic mechanism? There is, beyond all doubt. But it remains to demonstrate, by reference to the acknowledged data of descriptive anatomy, what the master demonstrators in that science have never suspected, that a new viscus added to the body, or an old one modified, is the symbol of a cycle of changes necessarily flowing from, and consecutive to, that one act of creation. Pursued under the brilliant light of this novel methodological conception, the details now to be studied will add another triumph to the first principles of organic science.

Let first be studied the serial history of the *digestive system in relation to the fluids*. In the stomach, the first and the last acts of nutrition meet. In its simplest phase this organ is a bag, perforated at the bottom. On the walls of this bag *all* the visceral or gland organs (except the reproductive) of the body are commonly supposed to be situated. It is in direct communication with the splanchnic cavity, which lodges the nutritive fluid. The glands which are distributed over its surface consist of cells filled with oleous molecules. These cells are *not* nucleated; they should be described as glandular capsules. In them are generally supposed to be united two independent offices, that of the gastric juice and that of the liver. The model of this form of digestive system occurs in the hydroidan and actinian zoophytes. All the vital preparation which the fluids in these classes are required to undergo, consists in their admixture with the secretion derived from the walls of the stomach alone. Nothing else is added to the blood-making apparatus. The fluid enters at once into the cavity of the body. It is there aerated. It is then qualified to react upon the solids for their nutrition. Could so simple a vital machinery, viewed apart, considered alone, ever receive a consistent physiological solution? It is by calculating it as the last and lowest member of a descending series, or the first and simplest point of an ascending line, that the *meaning, the possibility* of its simplicity can be shaped into the consistent form of a reasonable problem.

The fluids consist only of a weak limpid solution of albumen. They display only the faintest tendency to corpusculatation. Now, reflect upon these marvellous phenomena, first in the chronologic order of their occurrence, then in their reciprocal connexions:—(1) a simple system of interior solids;* (2) simple fluids; (3) simple exterior solids. That is the

* I have re-examined with great care the reputedly biliary character of the pigmented cells which are distributed over the *internal* surface of the stomach in *Hydra* and *Actinia*. Various species of the latter were subjected to analysis, with special view to the solution of the question discussed in the text. The tissues are most transparent, and therefore most favourable, in *Lucer-*

march of events. Catechise more minutely the first members of the triad. The stomach alone, of all the inextricably complex organs afterwards added, in the higher grades of the scale, to the system of the interior solids, is here present. Is that literally the case, or is the gastric open-bottomed sac of the zoophyte the *whole alimentary system* of the higher animals in epitomized miniature? Does so simple an apparatus in secret and invisible littleness, comprehend not only the liver, pancreas, spleen, &c., but the small and large intestines, and their associated glands? If so, physiological or serial simplicity can only mean minuteness of size. The lowest and simplest organism, in the conventional sense, must be synonymous with the "highest in miniature." This illustrates the confusion of ideas, the transmutation of thought, by which the progress of discovery is retarded, through lack of clear definition as to the sense in which words are to be employed. Attempt another interpretation of the facts. Nature attains the ends of "simplicity" of structure, by gradually and cautiously subtracting the inessential from the essential elements of an organ. The nuclear type is preserved. When this last is withdrawn, the last teleologic idea, as well as the substantive reality, of the organ disappears. But is there not a point, in the descending scale of animality special and proper to each separate element or system of the organism, at which, the process of deducting non-essentials having been carried so far, the last *essential* remnant, the archetypal essence of the organ, finally and completely vanishes from the scene? or, reversing the direction of the serial march, a point at which every organ has a *beginning*? Examine, again, the stomach of the zoophyte from the vantage eminence to which the mind has been raised by the preceding discussion. If every organ concerned in the formation of the nutritive fluids, in the case of the higher animal, were centralized, though only in the guise of their initial rudiments, on the walls of the actinian stomach; if one nucleated cell performed the office of a liver, another represented the pancreas, another the spleen, &c.; it follows, with undeniable certainty, that the fluids resulting from the elaborative secretions furnished by such a compound organ, would correspond *in quality*, in the number of their elements, with the fluids of the *highest animal*. Equals produce equals, complex conditions determine complex results. This is really the doctrine taught by every living systematic physiologist. The mind is chained by preconceptions as false as they are confused.* Why should not an integral *organ* of an individual

varia Auricula, not uncommon on the coast of Swansea. I have arrived at the conclusion not without thought, that when the alimentary system *terminates* at the base of the stomach as exemplified in the Hydroid, Asteroid, and Helianthoid Anthozoa, a biliary system *does not exist*, has not been created. It will be explained at a future time, that in the lowest radiata it is extremely difficult to define the difference between the true gastric glandules, which in their anatomical *locale* are strictly limited to the stomach, properly so called, and the biliary glandules, which as constantly are distributed over the walls of that segment of the intestine which succeeds the stomach in an antero-posterior order. If a *liver* could grow in or on the parietes of the stomach, nature would approve herself guilty of an incomprehensible outrage upon the constancy of her own principles. Every organ has an inviolable *anatomical locality*, just as every species has its *serial place*. To cause a liver to grow on or in the stomach, would be the same organic victory *precisely* as to transmute a species!

* The "homology of organs" is nothing but "ascensive development" of organs. The *anatomical place* of the organ is never transferred. Amid a thousand variations in the outward physiognomy of organs, the law which demands *constancy in the relative position* of parts remains absolute.

being, just as much as a species of a genus, have a *beginning* in the organized series? The first appearance of a new individual or group of beings in geologic chronology is signalized as a mystic era, annunciative of the omnipotence of Creative Power. The first appearance of a *new organ*, as wonderful and perfect as an example of organic mechanism as any separate individuality, is viewed with indifference, and reluctantly chronicled in the annals of science. In gazing at events *at present* utterly inapproachable to the human understanding—the occurrence of species *in time*—the unresolved nebulae of cosmic chronology—he culpably overlooks the *comprehensible* phenomena of creation; comprehensible because legible in their attendant conditions. In the instances of new organs, the steps of the creational processes are literally traceable, *seriatim*. The stomach of the zoophyte is then the *first form* or shape under which the alimentary system of the interior solids begins in the animate series. But let it be emphatically stated, it is only the incipient representative of the stomach proper; it is not a compendium of all the alimentary machinery of the higher animals. If it be only a stomach it follows, with irresistible certainty, that the stomach proper is the first created link in the alimentary chain—the first letter in the continuous series of the alimentary alphabet—that the duty performed by it must be the lowest expression of the function discharged by any other stomach-proper, not of the entire digestive system of more completely organized species. Here, then, “simplicity” of office is synonymous with *singleness* of action. Singleness of structure is the obvious inference!

The method of evolution, as it applies to the alimentary system, consists in the successive addition of *new* segments antero-posteriorly from the stomach to the *colonic* intestine. The distinction between the small and large intestines is not clearly defined in the invertebrata. This is the serial law which presides over the development of the digestive canal. Let it be further considered if, in the zoophytes, the stomachal extremity or segment only of the digestive canal be present, it is only the *gastric* agency which can be exerted upon the fluids. The product of such agency is the correlate of that of mere stomach digestion in the higher animal. This method of explaining the low type and inferior standard of composition displayed by the lowest order of fluids, is at variance with no known fact in serial organization. When the true natural history of the proximate principles shall have been written, when it shall have been proved by chemical demonstration, that there obtain, even in albumen and fibrine, *grades* of vitality and composition, the question will be answered whether the *finished* nutritive fluid of the zoophyte is the equivalent only of the chyme of the vertebrated animal. Let this question be waived for the present. The proposition has been established that in the first and lowest class of animals, in which the material conditions of existence are the simplest, and therefore most intelligible, the *triad systems of the organism bear towards each other a definite, direct, and constant ratio!* The lowest pattern of visceral apparatus engenders the lowest type of fluids, and the lowest standard in the fluid series determines the most degraded class of exterior solids. Can it be otherwise?

The *alimentary system of the medusae* presents an unambiguous advance upon the former. Cæcal prolongations are superadded to the central

gastric sac; a new organic place is created; a biliary system arises to occupy that place, yet not in an unequivocal form. The gastric canals of the medusæ are not the counterpart of the visceral cavity of the actinia. The former is anatomically continuous with the stomach, the latter is a separate chamber. In the gastro-vascular canals of the medusan, the contained fluid receives new impulses of growth. In the cavity of the body of the actinia it is at its last stage; it can rise no higher in composition; it was necessary to superadd the gastro-vascular cæca to the central stomach in order to create an anatomical place for a new system of organs—a biliary apparatus. The fluids could not be raised in standard without a resort to some such expedient. The fusion of organs is as contrary to law, as abhorrent from true science, as the transmuting of species; a new organ must, therefore, have a new locality; it is provided, a new agency is introduced, what are the physiological consequences? The fluids are raised in vital and chemical composition! The degree in which the fluids are elevated is proportionate to the advance which may have occurred in the elaborative solids; the sequence is necessary and co-ordinate. How simple, how perfectly comprehensible the *mode* in which the act of creation is accomplished! In zoophytes and medusæ the alimentary system contains, bounds, the nutritive fluids. In all animals above this limit, these fluids escape into separate and distinct recipient canals or cavities. The medusan organism consequently marks the upper limit of true *Phlebenterism*. The physiological sense implied by this word is far more clearly expressed by the word *gastro-vascular*. About the former designation there hovers an elegant Grecianism, about the latter plain common sense.* By the word *Phlebenterism*, it is meant that the cæcal tubes appended to the stomach prepare, contain, aerate, and distribute the nutritive fluids of the body. In the MEDUSÆ all these functions severally and collectively are undoubtedly discharged by the gastro-vascular canals. But the word was never intended to be applied to this class of animals. It was invented only to express the anatomical peculiarities which distinguish the alimentary system in certain classes of gasteropod molluscs, exemplified chiefly by the Eolidan and Doridan families. Between the gastric processes of these molluscs and the corresponding diverticula in the Asteridæ among the echinoderms, Pycnogonidæ among the crustaceæ, Planariæ and Clepsinidæ among the annelida, the Trematodæ and Cestodea among the entozoa, there obtains this radical difference. In all the latter examples, the cæca bear the biliary organ *in their walls*; in the Eolidæ and Doridæ a separate liver is provided.

The tubular appendages into which the stomach in these nudibranchians is multiplied cannot, therefore, prepare the fluids further than what is implied in the act of digestion. In minute anatomical structure the digestive cæca of the Eolidæ and Doridæ are consequently not homologous with, although analogous in figure to, the corresponding appendages in the digestive system of the Asteridæ, Planariæ, Pycnogonidæ, &c. The resemblance is only anatomical. Physiologically, they are irreconcilable.

* The word *Phlebenterism* originated with M. Quaterfages. While engaged in the pursuits of natural history, in company with M. Milne Edwards, in the years 1843 and 1844, he published a paper in the *Comptes Rendus*, for July 15th, 1844, in which he noticed a group of gasteropod mollusca, for which he proposed the distinctive name of *Phlebentera*.

The Phlebenterata of M. Edwards and M. Quaterfages *fail*, then, in the alleged function of sanguification. This fact is in itself enough to prove that their fluid contents cannot be a *completed* nutritive medium. But the very idea of Phlebenterism is destroyed by this irrefragable fact—in every example (the medusæ and zoophytes excepted) of supposed Phlebenterism, whether drawn from the lowest crustacea, echinodermata, annelida, entozoa, or mollusca, the gastro-vascular appendages *float in and are surrounded externally by a layer of fluid*. In the nudibranch molluscs and Pycnogonidæ, it is true-blood, because circulated by a heart in a definite orbit of movement; in the Planariæ, Trematodæ, and Asteridæ, it is a real chylaqueous fluid, because, though occupying the same anatomical position, it is *not* circulated by a heart. It moves to and fro under the rhythmic contractions of the alimentary diverticula.

Suppose the contents of these gastro-vascular cæca to be a nutritive fluid, while in such situations, how is it to be aerated? Necessarily, if at all, *through* the stratum of fluid by which each cæcum is embraced externally. Though this precise method of reasoning against the tenableness of the Phlebenteric theory has not been followed by Mr. Hancock and Dr. Embleton, they object on a ground quite as valid, that a true and very exalted vascular system exists in those very molluscs which M. Quaterfages has signalized as the most degraded.

But are those families, already enumerated, which are characterized by a ramifying alimentary system, coequal in organic standard because they are similarly constituted in the special feature of a vasiformly subdivided digestive apparatus? This question recalls the mind to the point from which this discussion originated,—that the organic standard of the fluids is directly determined by the serial grade of visceral solids. In the zoophytes and medusæ *all* the blood-making processes, inclusive of the respiratory, occur in the digestive system. No single sanguiferous action takes place external to or beyond the limits of this apparatus. *This* is true Phlebenterism. Nothing beyond and above this limit is unmixed Phlebenterism.

Let the law be here remembered, already emphatically proclaimed, that the simplest type of the system of the *interior solids* is coincident with the lowest standard in the fluids. View the phenomena again in their organic connexions—Do they not arm the philosophic anatomist and the vital chemist with a *novum organon*?

Proceed to a higher grade in the series—to the *Echinodermata*. An extraordinary event occurs: the fluids are impounded in a new, closed, and independent cavity,—a cavity which had no previous existence in the chain of animal life! Is this striking episode a causeless, arbitrary circumstance? Does it stand apart as an unconnected novelty in the organism? Impossible! It is raised in chemical and vital character as compared with the Phlebenteric fluids of the medusæ and zoophytes;—but *why* and in what manner? Examine the blood-productive solids. To an incomplex gastric sac, cæcal tubular appendages are superadded. A true biliary system then arises, but *not* before. These cæca cannot be a part of the stomach, properly so called. The presence of liver-glandules on their parietes is conclusive against their gastric homology. They are the foretype, in ambiguous rudiment, of that glandular blood-making

segment of the alimentary system of the vertebrate animal which comprehends the small intestines.*

The partitioning of the digestive from the fluid system gives to the latter individuality. It is the mark of organic elevation. No incident, in a dependent cycle of actions, can transpire without co-incidents or consequents. What are they here? A liver is added to a stomach—two blood-making organs! The fluids indicate, express the event. They betray the signs of new productive agencies. They acquire more definitely organized corpuscles. They experience an augmentation of proteinised principles. If they did not, no visible consequences would flow from the superaddition of a liver, though simple, to a stomach, itself of the lowliest type. The fluids are consequential. They denote the *second* member of the organic triad,—the second element in a tri-partite individuality. The fluid occupying the peritoneal cavity in the Asteridæ is the *initial phase* of the true chylaqueous system. *Everything* in the Asteridan organism corresponds with, and explains this initiality in, the fluids. The simplest biliary system is joined to the simplest stomach. If, under such circumstances, the fluids were organically complex, would not *reason* be outraged? In the ways of living, as of unliving Nature, there is reason. In the laws of the former, as of the latter, there is uniformity! Cannot, then, the physiologist foretell results, the conditions being given? If he cannot now, in this age of history, he will at a future!

But there is another event to relate:—*A blood-proper system intrudes itself on the scene.* Why? Because the productive, creative conditions are realised. The result intended *must* follow!

Here is another *act* of creation!—Could a true-blood system arise and live in a zoophyte or medusan organism? If there be *method* in Nature's creative scheme, it could not. Then, in serial organogenesis there is a *designed beginning* to *organs*, or systems of organs, as in serial zoogenesis to *individuals*,—to integers of individuality as to species!

Move higher. In the *Sipunculidan genera* the alimentary system augments in complexity. The fluids rise in a similar ratio! The corpuscles of the chylaqueous fluid acquire colour-pigment; the fluid becomes more thickly albuminized; a blood-proper trunk assumes a separate consequence; the nerve and muscle systems rise in importance! Can the force of these demonstrations be rationally opposed? Can the *facts* on which they rest be denied? Then does not the physiologist see that events, *creative "miracles"* in the history of organization, are really nothing but *necessary sequences*! One link necessitates as it conditions the next. In Nature there is no solitariness of acts, no capricious, licentious singularity. Her operations, even the most hidden, are bound by the adamant chains of inviolable law.

More onward still,—the *entozoa* forefigure the annelida. Every class of

* For some years I have noted the instances in which I have succeeded in discovering the presence of food in the discal sac, the true stomach of the star fish. In no single instance could it be proved that particles of *undigested* food passed into the interior of the cæca. Each of these appendages is guarded by a sphincteric apparatus which, pylorically, excludes every thing but *prepared* chyme from the chambers of the cæca. In the cæca the *second* act of digestion occurs, not the first. These facts are advanced in support of the doctrine that Nature does not locate organs in the organic machinery indiscriminately; the *place* of an organ is as absolutely fore-ordained as its anatomical elements, its specific structural type.

animals in the organized series stands on a more or less separate basis, forming the bottom of a secondary diverging line, resting on one common to all. The entozoa are not definable as the natural continuation of the Sipunculidæ, and yet they constitute the lowest members of the annelidan series. In the scheme of the animal kingdom there is discernible no *literal* linearity. Here, accordingly, the wave of progress again descends. The blood-proper system present in the echinoderms disappears. The chylaqueous system resumes its exclusive prevalence. This descent is accompanied by the marks of degradation in the system of the visceral solids. The biliary apparatus sinks in anatomical characters. Its identity is scarcely determinable. The fluids exhibit inferiority. The alimentary system betrays faint traces only of the distinction afterwards to be declared between the stomach and intestines. The liver-system is of the humblest order. Look at the *annelida*: the stomach-proper is individualized; the intestine is stratified by a higher form of biliary system; an exalted type of chylaqueous fluid occurs; an unambiguously developed blood-system, provided with self-acting contractile vessels, appears on the stage. In the measure and standard of these successive phenomena, is there not inscribed the impress of proportionality?

The planariform entozoa and annelida are parallel in these particulars: both are destitute of a blood-proper system;* both exhibit a *ramified* alimentary system; in neither has the biliary system received a separate form, it is distributed over the parietes of the digestive cæca; in both, the nutritional fluids are of the lowest type. But the more highly-organized orders of the family of annelids denote a striking complication of machinery: a separate and independent blood-system arises; the fluids thicken, from the increased amount of the proteinized principles; the floating corpuscles mark an improved standard of structure; and, finally, the character of the exterior solids is raised. These events follow one another with an inviolable parity of march. Are they not inevitable sequences?

The *alimentary and biliary systems in the lowest mollusca* include still the entire sum of the blood-making solids. It is by their anatomical characters, by their serial degree, that the standard of the fluids is to be determined. The stomach is dilated, such that it may be distinguished from the intestine. The liver in every genus is situated upon the walls of the intestine. In the genera *Ascidia*, *Dendodroa*, and *Cynthia*, the liver is said to be absent.† This is an error: it exists, in all these genera, in form of a layer of glandules in the intestinal wall. In some orders, this

* The reader is referred to the 'Annals and Magazine of Natural History,' November, 1853, for a full discussion of the question which relates to the fluids of these classes. The views of M. E. Blanchard, which ascribe a complex blood-proper system to the parenchymatous entozoa and Planariæ, are there shown to be an utter misconception of what exists in nature. The beautifully painted system of bloodvessels which grace the last edition of the 'Regne Animal' afford an overdrawn view of the passages, bearing in these classes nothing but a chylaqueous fluid. In the chapter in the 'Annals of Natural History,' to which I have referred, I now think that I have assigned too large a share in the office of respiration, and probably too high an organic standard, to the *corpuscles* of the chymous fluid contained in the interior of the digestive cæca, in the trematode entozoa and planariform annelida. It is, however, certain that the contents of these cæca in some instances do undergo aeration. In *Aphrodita aculeata*, so express are the provisions for bringing these cæca into advantageous contact with the external water, that the question can scarcely be disputed.

† Article 'Tunicata,' Cyclop. Anat. and Physiology, by Rupert Jones.

segment of the intestine is thickened by the focculent development of the liver. It is the phase of the organ which precedes its centralization into a separate individualized form.

In *Chelyosoma*, the "caecal tubes" distributed over the walls of the stomach are described as the liver.* If this be true, there can be no foundation for the law of *anatomical place*, by which organs are governed in development and distribution. The author's own dissections assure him that there prevails but *one* type of biliary apparatus in the tunicate mollusca. It constitutes a coating to the anterior extremity of the intestine. It discovers higher characters than those of the liver system of the annelida, which invariably occupies this situation. The distinction between the stomach and intestine in the annelida is only faintly traced. In the fact of the individualization of the stomach, the tunicate molluscs surpass the annelida. If the former are superior to the latter in the serial features of the alimentary systems, it may be predicated with certainty that they are superior in other characters. Nature's career is inflexible. What are they? A complex organ of propulsion—a heart—is added to the apparatus of the circulation. By this single provision, a chylaqueous is transformed into a true-blood system of fluids. The duplicity of the fluid system of the annelids thus disappears. A circulation of blood is now first established, in the fulness of its physiological meaning. The cardiac centre has arisen. The fluid circulates with orbital regularity. The second and subsidiary system—the chylaqueous—has ceased in the series. The blood-proper is the only medium of nutrition. In the echinoderms and annelids, this system is provided with no trace of a muscular cardiac centre. Catalogue these symbols of advancing seriality. *Tunicata*—stomach and intestine separate and distinct—biliary organ unchanged in type, advanced in degree—chylaqueous fluid disappeared—a heart created—a true circulation of corpusculated blood established. Is not the *mechanism*, are not the *rules*, of organic creation perfectly orderly, and traceable to the last *material* conditions in this example?

An elevation in the standard, an increment in the working power of the visceral solids, simple, and two in number only though they be, necessitate an advance in a similar ratio, not only in the type of the apparatus of the fluids, but in the composition of the fluids themselves. The further the analysis proceeds, the more inconvertible does this law of parity of progression, in the component systems of the organism, appear. The *acephalous molluscs* contribute decisive facts to the cumulative argument. The stomach and intestines are more obviously differentiated. The liver swells in mass, is multiplied by tubuli, the heart rises in structure and dimensions, the bronchial apparatus assumes preponderant proportions, the measure of the respiratory office is augmented—an advance is accomplished in the standard of the fluids.

The digestive organs of the *gasteropod molluscs* are highly developed. The stomach is completed, formed. The intestines lengthen by convolutions. A new advance in the visceral solids is, at this stage, effected; a *salivary system* is superadded. Here, again, is the *first* unequivocal appearance of an *additional* integer in a pre-existent organism. Wherefore so tardy? Because the fluids required by an incomplex machinery were

* Article 'Tunicata,' Cyclop. Anat. and Physiology, by Rupert Jones.

themselves so little raised in the scale of vitalization, that a mere stomach and liver sufficed for their production; but in order to the gradual accretion of new parts to the sum of the body, the standard of the fluids could not be elevated, save by the provision of *additional* organs. The object could not be accomplished by the involution of the old. A *specialty*-acting chemical power has now become essential—a salivary system is *created*. The liver assumes an enlarged and isolated character, and a symmetrical position. A heart subdivided into auricular and ventricular cavities—a distinctly-recognizable approach towards the organic definition of the vascular system. *Fibrin*, in an unambiguous form, occurs in the blood. The system of the *exterior* solids, as explained in a former paper, betrays the unequivocal signs of growth. But to the organism of the gasteropod molluscs a renal apparatus is also added in a decided form. This system is only doubtfully present in the acephala. Does not the physiologist perceive, with greater and greater brightness of intellectual vision, that, as the blood rises as a chemical compound, as its albumen and fibrin increase in relative amount, as its morphological elements betray the signs of higher organization, new productive elements, organ after organ, accessions to the blood-making capacity of the system of the *interior solids*, are being made at a corresponding ratio?

Extend the survey to the families of the *articulated series*. In *insects*, effective masticatory appendages initiate the digestive system. Mark the complex subdivisions which now occur—a muscular oesophagus—a crop (*ingluvies*)—a muscular gizzard (*proventriculus*)—a stomach (*ventriculus*)—an intestine—foreshadowing the vertebrate segments of the ileum and colon.

Is there not in all this a profound significance? *Every organ* which succeeds the digestive system in the order of occurrence of physiological acts, bears the impress of the exalted type of the digestive machinery. It is the centre of the system of the interior or blood-making solids. Around it cluster salivary glands, Malpighian or biliary tubuli, affecting a *pyloric* situation, a renal system connected with the cloaca. This machinery of visceral solids is the physiological basis upon which rest the extraordinary superstructures of the respiratory and circulatory systems of insects.

The *crustacea* contribute confirmatory evidence to the consistency and truth of the great rule of serial organization, which it is the object of this memoir to inculcate. The *alimentary canal* is subdivisible into a gastric segment with its intricate appendages; the stomach is completely organized; the intestine, though short and straight, is similarly formed. The liver in this class enhances in organic value. In the lowest crustacea it constitutes a glandular, follicular stratum, enveloping the intestine; in the highest it consists of two glandular masses, composed of more or less remote cæca bound together. The renal organs of this class are imperfectly developed. Connect the preceding facts with the history of the fluids. They are inferior in standard to those of insects—why? The visceral apparatus is less completely organized. The heart, however, is well formed, subdivided into cavities; vessels are constructed, highly-organized corpuscles float in the blood, and fibrin is one of its proximate constituents.

Indulge for a moment in the luxury of a retrospect over the historic

sketch now summarily drawn. The lowest invertebrate animal is provided *only* with a stomach. It is the organic centre of the entire body. Every other element, fluid and solid, is measured and apportioned by this primal organ. The fluids occupy the lowest extreme of the scale. A liver is next added. The fluids rise one stage. An intestine is appended to the stomach; the liver augments. The fluids multiply in the number of their constituents, improve in the quality, and increase in the amount of the old ingredients. Additional chemistry is provided in the salivatory and renal organs. The products of the agency of the latter are announced by indications of advance in the standard of the fluids.

One great sanguiferous process remains to be considered before completing the survey of those phenomena of progression in the invertebrated animals, which prove incontestably that *organs*, as well as *species*, obey the grand creative ordinance, that all things living shall *rise* from what is simple to what is complex, from what is low to what is high. The *respiratory system* differs from the ordinary glandular organs in this extraordinary feature, that it knows no variations or differences of *quality*. The stages of *its* progression are those only of degrees. Can this be said of any other sanguiferous gland? The question cannot be confidently answered. Secretions, products, graduate in composition as organs progress in structure. But there must be a point at which *bile* is bile, or *not* bile. If it be bile, the cell which formed it is a *liver-cell*. The bile of the echinoderm may be composed of only *one* or *two* of the numerous ingredients of which the same secretion in the vertebrate animal is constituted. It is less by varying the essence, the basilar constituent of an organic product or of an organic apparatus, than by adding to, or withdrawing from, the number of the component elements, that Nature's workmanship rises or falls in the scale. Of respiration only thus much can be stated—the lowest nutritive fluid absorbs oxygen, and emits carbonic acid; the highest only does the same. The difference lies in the amount. A simple fluid, composed only of a very dilute solution of albumen, demands the lowest grade of the respiratory action. How could it be otherwise? Carbonic acid is the product of the molecular metamorphoses of organic tissues and organic principles. If these latter are of the most degraded order, this especial function must be at its minimum.

The reasoning admits of no opposition. Then in writing the serial history of the respiratory organs, nothing but variations (endless!) of proportion and outward shape have to be recorded.

This absolute principle with reference to the function of respiration may, however, be propounded. Whatever be the external form of the organ upon which this function devolves, the amount, the *meter* of the function, is directly proportional to the organic standards of the fluids. Complex fluids evolve a large amount of carbonic acid, and take in a corresponding value of oxygen; simple fluids a small. That is the *serial law of respiration*.

Given the standard of the viscera, to determine the composition of the fluids? Given the fluids, to estimate the measure of the respiratory function? The problems are practicable. They breathe, however, the accents of a science which sleeps to glorify a future age.

Advance to a general view of the conditions which pertain to the for-

mation of the fluids in the *vertebrated series*. Every genus of invertebrate animals begins at a low species, and rises to a high. Every genus of vertebrate animals does the same. Fishes have their lowest and highest types—birds have theirs—mammalia have theirs. The animal kingdom does not represent *one* straight, continuous series, travelling and multiplying along one rectilinear path. The true classification of animals will be eventually founded upon this principle. One highway traversing the distances, intervening between the lowest and highest species, will be defined. From this *common* highway—common, that is, to species and genera—secondary by-roads, ending in unconnected extremities, will be found to diverge.

This figurative conception expresses the principle on which the animal kingdom should be classified. The vertebrated are a natural continuation, in every philosophic sense, of the invertebrated series. The same arithmetic laws preside over the multiplication and development of the blood-making viscera. The principle of progressive serial development is indubitably legible in the organs and the chemical processes which precede and elaborate the organic, proteinised principles of the vital fluids. The fluids are the sum, the product, of the organic actions by which they are preceded. The interior solids are the seat of such action. This new principle in the science of organization, in its reference to the kingdom of the invertebrata, reposes now on the firm basis of demonstration. The phenomena of gradation, rightly interpreted, has insensibly expanded into the *law* of organologic creation. If such a law be true of the invertebrata, is it conceivable that it can be untrue of the vertebrated series? The vertebrate organism begins at the limit at which the invertebrate ends. The architectural principles on which the former fabric is built are the direct and natural continuation of those observed in the case of the latter. The interval which removes the fish from the mammal is not the less real because it is less wide and conspicuous than that which divides the sponge from the cephalopod. In both examples the interval is traversed by a *graduated scale*. The principle of gradation stamps its impress upon the most subtle elements, and governs the deepest penetralia of the vertebrate as of the invertebrate organism.

The method of tracing upwards from the lowest type the history of *individual organs of the body*, gives to the law which regulates their increase in the line of the ascending genera a new force. Each organ has its own history. It has been ably written in the works of the anatomists of this age. There is, however, an unwritten history which remains to be added to the literature of organic science. A change in the outward form, the type, or the dimensions of an organ in the living animal body, cannot, from the introverted reciprocity which pervades all, occur as a solitary event. It must have been preceded by preparatory occurrences; it must have been succeeded by derivative sequences.

This chapter, when given to science, will enrich substantially its annals. Compare any single organ of the vertebrate animal with the corresponding organ, when present, of the invertebrate. The difference is remarkable. But compare the entire visceral solids of the one with those of the other. Look at them respectively as multipartite engines designed to accomplish a definite purpose. What disparity of power!

Now place in juxtaposition the products of the actions of these engines severally—the fluids of the invertebrate by the side of those of the vertebrate animal. Is the disparity less striking? It is scarcely possible to conceive that the *constructive idea*, in two things so widely dissimilar, can be the same. It is so. One is the prolongation of the other *plus* a few superadded elements.

The bright era will dawn upon the science of living beings when the physiologist, from the examination of the fluids, will be armed with knowledge to deduce the nature, the number, the dimensions, the structure of the productive viscera; and conversely to foretell the constitution and vital standard of the blood from a consideration of the viscera. In this communication it is only practicable to give to these views the form and shape of verbal propositions. The details of demonstrations are, for the present, postponed.

The blood of the fish differs neither less nor more from that of the cephalopod than the visceral solids of the two animals from one another. It cannot be otherwise, if the fluids owe their formation to the agency of the interior solids.

Excluding the lancelet, whose organs generally are inferior to those of myxinoid fishes, think of the *new elements* which at the fish are added to the machinery of the organism.

Though straight and short, the stomach and intestinal canal are capacious. It is here, for *the first time* in the zoological series, that the liver assumes a separate existence, and becomes internally complicated by a portal and arterial circulation, and a system of excretory ducts. Now the pancreas and the spleen first show their presence in the machinery of organization. It is only at this limit that the renal apparatus has acquired an evident importance. A new and hitherto unknown element of the animal body occurs also at this stage—the *absorbent system*. What *must* be the issue of these several complex acquisitions on the side of the elaborative organs, this skilfully contrived involution of parts and superimposition of specially created structures? The first and most unambiguous result is inscribed on the characters of the fluids. The proteinised principles are raised in amount, and probably improved in quality; colour (hæmosine,) in a novel manner and place, has appeared. A higher class, a more finished order of floating corpuscles, enters on the scene. Is the ellipticity of their figure a character of degradation? But, withal, the heart still retains the impress of relative inferiority, and the *respiration is aquatic*.

In the *Reptilian organism*, what are the signals of elevation? The Batrachia exhibit a long and convoluted alimentary canal, but the stomach is only slightly individualized.

This part of the alimentary canal augments more and more, from the Batrachian, through the Ophidian and Saurian, up to the Chelonian orders. As the gastric segment dilates, the intestinal canal shortens. In reptiles, though the liver is relatively large, the product of its action is small, for it operates upon blood which is semi-arterialized. The size of an organ is frequently inversely as its texture; a large and loosely-structured organ does not really present so multiplied an operative superficies as the small but densely-textured. The *pancreas* betrays indications of higher com-

plexity, as measured by that of fishes.* The spleen rises to the consequence of a pronouncedly-developed organ; the heart acquires an additional auricle, and the first trace declares itself of the presence of a second ventricle. It is here that the grand transition is accomplished from aquatic to the atmospheric method of respiration, in the vertebrated series. Evidences of a more elevated standard are discernible in the fluids. The proportion of fibrin is undoubtedly augmented; the corpuscles are larger, more efficient as instruments for the generation of pigment; the specific gravity† progresses upwards. The entire apparatus of the exterior solids attests the thickening, advancing composition of the fluids.

In *birds*, the march of organological acquisitions proceeds. The whole digestive apparatus multiplies; its several constituent divisions are involved in the advance. To the œsophagus is appended a crop; after the crop follows a second dilatation, the proventriculus (the gizzard), the liver, the pancreas; to the system of the blood-making organs is added the spleen. The heart is perfected as a hydraulic mechanism; its contractions increase in frequency. The blood changes for the higher, in several respects; its fibrin more quickly coagulates; the corpuscles lessen in ellipticity of figure, and lose their nuclei. Respiration is quantitatively augmented. By comparison, the *sum* of the visceral machinery is multiplied unquestionably.

The *mammalia* crown the countless members of the consistent series thus cursorily and hastily sketched. Every individual organ here attains its maturity of structure; the arithmetic aggregate is greater, the component parts are more elaborately formed. The economic principle of the subdivision of labour is prosecuted to the utmost practicable extreme. On this far-up level stands *the human frame*, the perfection of animality.‡ It is here that the prerogative of the erect attitude is first conferred; it is here that a compactly-structured heart first drives the blood against gravity—a symbol of superiority. Discoidicity, in the figure of the mammalian blood-corpuscles, supplants the ellipticity of those of the lower vertebrata. In this class, the blood is more complex, of higher specific

* See the excellent article 'Pancreas,' by Dr. Hyde Salter, Cyclop. Anat. and Phys.

† In a future paper a tabulated series of observations will be given, by which the principle will be established, that the arithmetic sum expressive of the specific gravity of the fluids, conducted on the entire scale of the zoological series, will prove of unexpected value in elucidating the fact that the blood, viewed merely as a chemical solution, rises more and more in density as the scale is followed upwards from the lowest to the highest animal. Inferences deducible from such tables of facts will reflect corroboratively upon the reasoning, founded upon totally different evidences, which I have endeavoured to pursue in this and the preceding memoirs.

‡ The material fabric of the living body, animal and vegetable, is raised in standard by a progressive increase in the *number* and complexity of the parts or organs of which it is composed. The arguments pursued in this and the preceding memoir will, I trust, place this great theory of organization on a secure eminence. Can a wide-stretching law, which rules over the very elements of the ponderable organism, be untrue of its *imponderable moiety*? If so, there can be no constancy in Nature. But, in truth, the *phenomena* which express a *progressive ascension* from low to high, in what is trans-material, psychological, in living beings, are as unequivocal in clearness and significance as the signs which mark the advance of substantive individualities. The conclusion in the two cases is founded upon the same induction. The analogy is full and complete; it cannot be overthrown. Carry the eye downwards along that deep descending file of vitalities which connects humanity with the zoophyte. It may perform the journey by one of two separate yet dependent routes, the physical and metaphysical. As the organism rises in architectural finish, that immaterial principle, of which it is the tenement, rises in the same exact proportion. In the argument as developed in the text, it has been proved that product (the secretion) of the physiological activity is chemically complex if the producing organ be anatomically complex; incomplex if the latter be structurally simple. Why

gravity; the component viscera are weightier, relatively to the weight of the entire body. The mammalian characters culminate in man. In this solitary *genus*, material and immaterial organizations reach the summit of refinement. On the subtlest and profoundest constituents of the collective system of interior productive solids is graven the impresses of the most finished elaboration; the middle system of the nutritive fluids are here, too, the theatre of the *most* complex chemistry. It is here, consequently, that the machinery of the exterior solids attains its most perfect and most beautiful development.*

The human hand, the faultless instrument of the human mind, the consummate symbol of *the completed scheme* of nervo-muscularity, *could not* be grafted on the organism of any inferior member of the series. That phase of bio-chemical science is fast approaching which will establish the prediction, that not only the visceral apparatus and the exterior solids, the system of the nerves and that of the senses, as compared with the corresponding elements in the bodies of all other animals, wear, indeed, the insignia of supremacy, but that the nutrimental media by which the "noble column," standing upright in express image of its Maker, is sustained, will yield to the micro-chemical philosopher of another day *proofs*, sublime in their wide-spreading and deep-penetrating significance, that *they also*, as integral parts of that privileged column, are clothed in the habiliments of *the same* supremacy.

should it, how could it be otherwise, if there be truth in demonstrative reasoning with reference to the *products* (psychological manifestations) of the activity of the nervous system, itself the sequence of a labyrinth of anterior activities? Could the human intellect sit enthroned on the contracted cerebrum of the fish? The question is ridiculous and incongruous. Could the liver of the radiated animal secrete bile, having the same chemical composition as that which is normally produced by the same organ in the cephalopod? The point is parallel and illustrative. In fact, violate by *any* supposition the principle of progressive seriality in the *psychology* of the animal chain, and the mind is landed in caricature and absurdity. The *human mind* itself is governed by the same law of progressive ascension. Unwind the tangle of this law in the history of nations, and the reality of its governance becomes undeniable!

* I trust that the *dependent physiological events* which, in the argument developed in this memoir, have been traced as a continuous line of light throughout the entire animal kingdom, will hereafter lead to the appreciation of *correlative dependent pathological events* in the study of the diseased conditions of the fluids. The fluids are not physiologically isolated, how can they be so pathologically? If albumen and fibrin, &c., cannot arise and augment spontaneously, causelessly, it is highly probable that they cannot suffer disease primarily, causelessly, and spontaneously. Themselves being results, products, their morbid states are extremely likely to be also results or products of anterior disease. If on the entire scale of the animate series there be legible the bright characters of the all-pervading law—that *as the antecedent solids, so the consequent fluids; that as the antecedent fluids, so the second great system of solids*—it must follow that the same exact order must preside over the development of pathological phenomena. *Humoral pathology is a practical impossibility.* It is too partial and exclusive to embrace the truth. The science of pathology must rest on a wider basis. What does the *dogma* of humoral pathology really signify? That the fluids *per ipse* are pathologized! That effects can happen independently of causes! That causes can assume an operative form without ending in effects! I am deeply convinced that the modern revived phase of the humoral doctrine of disease is as partial and false as its archaic prototype. Exclusivism is the emblem at once of narrowness and ignorance. It is the obstructive bane of modern pathology. It ignores that inviolable reciprocity, that intimate neutrality of connexion, which federates into *one organism* the fluids and solids of the body. Morbidism in resultant fluids, it is impossible reasonably to conceive apart from disease in the causal solids. No single fact has as yet been recorded in pathology which proves that any one or more of the constituent principles of the blood may suffer disease *spontaneously*. If the antecedent processes executed by the blood-producing solids be normal, the result *must* be normal. Healthy fibrin, in its chemical place in the fluids, is under the sway of forces which, *if normal*, render the *dis-order* of its constituent atoms impossible; the disorder must be *communicated* to it down along a descending path of anterior changes; it is then diffused from it throughout the entire system of the exterior solids. No single advance can be made in the science of pathology but under the torchlight of discoveries achieved in the domains of physiology and organic chemistry.

Now reflect. A moral remains to be pointed. The tale is now to be adorned with one application of the principles of organogenesis, which it has been the hope of this paper to unfold. It is startling; first, from the awe-striking profundity of the region into which it conducts the human intellect; next, from the perfectly *natural and necessary manner* in which it logically flows from the successive and cumulative series of demonstrative propositions precedingly enounced. If it be permissible, even within the domain of *positive* philosophy, to rise, by fair reasoning, from the known to the unknown, from the peopled earth to a "plurality of peopled worlds," from the mortal to the immortal, from matter to spirit, from man's ways to God's, on the wide-opening wings of that imperious "Analogy" which enabled the divine genius of Butler to convince the sceptic, to open a path into the arid recesses of the mind of the most rigid mathematician, along which there *now* enters therein a luminous and beautiful "ray," whose far-stretching line spans athwart the nubial chasm which divides extant existence, conscious and demonstrable, from a bright and lovely futurity, *else indemonstrable* by the *ars instrumentalis* of human reason, the following analogical argument is also permissible.

In the most simple organism everything is simple; not the fluids only, but *all* the solids, productive and resultant. In the next organism above the lowest, *three classes of signs* mark an advance, though it be only *one step*, in its chemical and physiological characters. Ascendingly, when any given element or principle is *added* to the fluids, it may be predicated with entire certainty, that a correlative addition has been made to the productive solids, and that a proportionate advance *will result* in the exterior solids. Descendingly, reversely, *one* class of signs read exclusively would be utterly unintelligible; *all*, viewed in their connexion, open to the eye of the philosopher a *splendid prospect*. When Nature is about to add a liver, or a spleen, or a pancreas, or a cerebrum, to the system of the vital machinery already constructed, is it *her method* to perform the act causelessly, by the mystic interposition of a terror-striking, because wilful, capricious, *unsystematic* "Fiat?" Most reverently and most assuredly not. Her footsteps are most legible, most orderly, and, when accomplished, most logically *necessary*. The physiologist, in travelling upwards along the track of progressional creation, can *even now* foretell the appearance of a new element in the growing and involving machinery of the living body, with a confidence which is rivalled only by the axiomatic certainty with which the astronomer predicts the return of a comet, or the eclipse of a planet. *But let him travel downwards*. He successively leaves behind him organ after organ, principle after principle, element after element, until at length he arrives at the deep-down confines of *morphological organization*. Now rest for a new thought. Is the inquiring, restless, divine human intellect to *terminate* at this unexplanative, unsatisfying limit—a limit *at which God's creative processes are not limited*? What! are *rule and method*, are laws and processes, are constancy of direction and certainty of result, are logical sequence and necessariness of succession, which *everywhere else above this limit* are palpable in the creational workings of the Maker and Ruler of this illimitable universe, to be fooled, to be abandoned, to be reversed, to be contradicted at this particular, arbitrary, unmeaning confine, the reputed, but not the *real*, beginning of the atomic and elemental movements which

end, result, in the organized form of matter? *Credat Judæus!* It is as impossible as that to-morrow the earth will cease to revolve. Philosopher! adventure downwards still, but reverently, for thou art in the presence of the same Almighty whose footprints hitherto thou hast tracked with certainty and truth. Descend; cross the *unferried passage which divides organic from inorganic matter*. Art thou not on the same road? Most certainly. It is the clear and obvious continuation of that luminously-defined path by which thou hast descended from man to the "Amœbæa." Is thy eye to be here blindfolded? Art thou here to be bereft of reason? Why shouldst thou think, reason, and *observe* hitherto and no further? No! Be profoundly assured that thou hast the same just grounds for inquiring, analyzing, inferring, observing, &c. *below* this limit as above it. It is one of those legitimate spheres of study into which its All-knowing and All-seeing Author has *invited* the human mind. Be not afraid. "Elements and principles," inorganic, cosmic elements *move upwards*, combine and recombine, until, in the ascending march, organized *form* is attained in fearless and unswerving compliance with *those very laws and processes* by which they rise from the zoophyte to man. *Generatio equivoca!* The phrase is nothing but a hollow, senseless sound. Let it henceforth be put calmly aside. The march of thought, of inquiry, and of discovery will proceed. And the day, oh! glorious morn! *will* dawn on human science, when *the creation of species in time and space*, the appearance of a new being, a recently-constructed individuality, on the theatre of visible, palpable existence, will be as clearly, circumstantially, and minutely explained by human science, as it now defines the material conditions which lead to, render necessary, the *creation* of a new system of living parts, a new organ in the chain of serial organization. Spontaneous, equivocal generation! Let it henceforth be called *creation by rule, by conditions*. The act of *initiating* is the same as the act of *continuing*. The doctrine which contends for the *reasonableness* of the atomic, elemental movements, which result in the "spontaneous creation" of a new species in geologic time, is now the opprobrium of human science. The time will come when it will be its brightest and highest glory.

(To be continued.)

ART. II.

The Pathology of Insanity. By JOHN CHARLES BUCKNILL, M.D. Lond., Physician to the Devon County Lunatic Asylum.

WHATEVER differences of opinion may be entertained respecting the causation of insanity when the excitant has been of a moral nature, the following propositions will scarcely be disputed.

Granting that the brain is either the organ or the instrument of mental power, and that physical causes are capable of producing insanity, in all cases so originating, an abnormal physical condition of the brain must be, and can only be, the cause of the abnormal condition of the mind.

To express this in more formal terms, we may say, that a physical agent acting upon a material substance can only produce a physical result. Blows, sun-strokes, poisons as of fever, are physical agents, which, acting on the brain, are capable of causing, and frequently do cause, insanity.

Insanity, therefore, in a considerable number of instances, is the expression of a physical condition of the brain. If the intimate physical state upon which it depends can be demonstrated, it will be for the metaphysicians to prove that all cases, howsoever produced, are not referable to similar conditions. The presence of a sufficient cause for certain phenomena having been ascertained, it is neither logical nor necessary to refer to the influence of other supposable causes. Physical causes are capable of producing syncope, moral causes also are capable of producing it; but since it has been proved by Dr. Burrows, in his work on the 'Cerebral Circulation,' that all physical causes operate by diminishing the pressure of blood in the cerebral vessels, it is fair to assume that all moral causes operate in exactly the same manner. To prove this assumption true, however, would not be easy; and theories about the moral causes of syncope, and the failure of spiritual essences, might be constructed as easily and rapidly as children make edifices of cards. The analogy holds good with regard to the physical and moral causation of insanity. If a physical cause can be demonstrated, it will be against all rules of scientific research to trouble and obscure the argument with other supposable causes, the very nature of which renders them undemonstrable. But is any peculiar condition of the brain, producible by physical agents, and causative of the phenomena of insanity, capable of proof?

We believe that it is. That it has not yet been demonstrated will readily be granted; but that it is demonstrable, we hold to be an opinion in conformity with the confirmed achievements of medical science in relation to other parts of the animal body. It is a subject which is knowable, and not unlikely to become known as soon as we have discovered the proper mode of questioning nature. It has frequently been objected, that because we are never likely to discover the final cause of thought, it is therefore not probable that we shall succeed in detecting the pathological cause of insanity. Thus, the author of the excellent article on 'Mental Diseases' in the 'Dictionnaire du Médecin Practicien,' states:

"Le scalpel, le microscope, les réactifs pourront-ils nous montrer jamais le mécanisme des opérations de l'esprit? Sait-on pourquoi celui-ci est bon, celui-là méchant? Pourquoi l'un est spirituel, l'autre sot? Quelle est donc cette opiniâtreté à pénétrer les mystères de l'intelligence, lorsqu'on n'a jamais pu soulever le voile qui recouvre la vie organique? Jusqu'alors l'étude des formes et des manifestations nous a seule été permise. Il est probable que l'essence des choses, la cause première, nous échappera toujours."

Nothing, indeed, is more likely, for the *essence* of things, or final causes, are unknowable; whereas secondary causes are knowable, and are every day becoming known. If it were certain that the pathological conditions of insanity were rightly to be considered among the essences of things, well might we be disheartened at the prospect of fruitless labour in the attempt to unveil them. The essence of things has not been revealed to us, and is not discoverable by us. There is in nature a holy of holies, into which no high-priest of human faculties may hope to enter; are we therefore to desert the temple of science? Because we cannot reach the sun, are we to abjure the use of light?

Fortunately for the progress of medical knowledge, and for the welfare of the human race, the conditions of disease do not take rank among the

essences of things; they occupy a secondary and more accessible grade. We know as little of the essence of secretion as we know of the essence of thought: that is, we know nothing. We are utterly ignorant of the final cause why one set of cells separates from the blood, urine; why another separates bile, and so on. The essence of secretion, like the essence of thought, will in all likelihood be for ever hidden from us. But this reflection has not prevented the secondary phenomena of secretion from being discovered, and deviations from the normal conditions of these phenomena from being recognised as the conditions of disease. And is it not probable that we shall eventually discover the conditions of cerebro-mental disease, not in the inaccessible heaven of final causes, and dependent upon the essence of thought, but on the hill tops of natural phenomena, which are approachable by human patience and industry; not in the reveries of metaphysicians, but in the plodding pursuits of the pathological anatomist?

That investigations into the pathology of insanity have hitherto been somewhat unfruitful of results, need excite little surprise. Structural pathology, and its handmaiden, organic chemistry, have not long been cultivated with that diligence which has in many instances been rewarded with recent and splendid success. It is but yesterday that these sciences were in their nonage, and it would be absurd to object that they have not yet solved the most difficult problem which can be propounded. That diseases of the brain constitute the most difficult problem of structural pathology, there can be little doubt; and the principal reason for it may, perhaps, be found in the fact, that brain-substance bears no apparent relation to cerebral function. We observe a collection of minute cells and tubes, discharging certain functions, but of the most important of these, namely, the cells, we are as yet entirely ignorant of the origin, the development, and the decadence, of their arrangement, and their connexion with the tubes.

We doubt whether the most skilful microscopist, employing the most perfect instrument ever yet constructed, could recognise a single brain-cell, as such, distinguishing it from a cell of the same size taken from a gland. Yet we know it must have its capsule and its contents, its proper mode of development, its connexion by tissue or otherwise [its ependyma or stroma], with its fellows, with the capillaries from whence it derives nutriment, and with the nerve tube, from which it receives, or to which it communicates, impressions or influences. The cell may, at different periods, be dilated and contracted, like the corpuscular oxygen-carriers in blood. It may be filling itself, or growing from the capillary walls during sleep; and emptying itself, or decaying, during wakeful hours; it may lack its proper nourishment, owing to abnormal changes in the capillary walls, or of their fluid contents; it may contain its proper complement of phosphorized fats or other matters, or it may have become choked with cholesterine or other products of regressive metamorphosis, like the gorged fat-cells of the diseased liver or kidney; it may become drowned in serum, or entangled in condensed connective tissue. All these changes are possible, nay, even probable; but as yet we are unable to recognise any one of them.

The infinity of nature still defies the skill of man. Golden trophies

she drops for the ardent pursuer, but still she flees. The fact has been humorously expressed, as it relates to the minutiae of zoology :

" Each flea has its flea, with less fleas to bite him,
And less fleas have lesser fleas *ad infinitum*."

It is not less true in relation to the minutiae of pathology.

That the check which the microscopic pathologist has received before the neuroses will eventually cease, we entertain no doubt; although, hitherto, the labour expended in this field of science has been rewarded with a parsimonious hand. The main thing needed for the success, besides more powerful instruments and improved means of manipulation, seems to be, that the microscopic investigator should have under the eye, at the same time, both healthy and diseased brain. Appearances which can neither be remembered nor figured may be sufficiently evident as abnormal changes, if contrasted at the time with a healthy standard. A few granules more or less in each cell may make all the difference between health and disease; and this difference may be inappreciable, unless the cells in which it occurs are confronted, so that the observer may at the same time "Look here upon this picture, and on this." But is it necessary that investigations into the pathology of insanity should be postponed until the microscope is competent to reveal the minute alterations of nerve-cell and nerve-tube? The history of pathological research on other organs would indicate that it was not necessary. The changes wrought by disease in an organ have first been recognised, as it were, in the lump; the minute structural changes have revealed themselves to later investigation. Dr. Bright ascertained that dropsies with albuminous urine were accompanied by an altered state of the kidneys, easily recognisable in the mass, and without the aid of the microscope; and subsequently to his discovery, the minute structural pathology of these changes has been developed by several observers. Although the tendency of pathological discovery has been to development from the particular to the general, as relates to diseased conditions of the body at large; as relates to diseased conditions of individual organs; its development has taken place in the contrary direction—namely, from the general to the particular. May we not hope, therefore, that, as the coarse pathology of Bright's disease was recognised before the microscope had to any extent rendered its potent aid to the labours of the medical investigator; as the ruder facts of fatty heart were recognised by Laennec and others long before Dr. R. Quain's classical paper made this disease one of the best known in the range of medical science; in like manner, some coarse but important observations, relating to the pathology of cerebro-mental disease, may be established before the microscope brings its tardy help to elucidate the intricacies of cerebral organization?

It must be admitted that, for many years, the brain in the mass has been diligently questioned for the cause of insanity; and that the results, if not entirely negative, have been unsatisfactory. The brain of an insane person is very generally found to have undergone marked alterations, but these have not, apparently, been different from those found in persons who have died of other diseases without mental alienation. For instance: of two persons affected with the same amount of cerebral hæmorrhage, and apparently much in the same locality, one becomes paralysed, with some impair-

ment of mental powers perhaps, but without insanity; the other becomes insane. As yet, the pathologist is unable to trace the cause of this difference of symptoms. Esquirol, after having examined the brains of many hundreds of persons dying insane, was led to the conclusion, that no cerebral changes had been observed by him in such cases which were not also to be found in others where no insanity existed. The same observation has been made by other experienced pathologists, both in France and in this country; and it is impossible to look through any detailed list of post-mortems of the insane without feeling convinced that the special characters of cerebral changes have hitherto escaped observation; for that there are special changes, no exact reasoner can for one moment doubt.

Having, ten years ago, been placed in charge of an institution providing for the treatment of 450 insane patients, I have examined an average of 30 persons dying insane every year. During the first six years of this period, the only impression made on my mind by these examinations was, that Esquirol's observation had been founded on truth. I gradually, however, became aware of this leading fact—that the brains of all persons dying insane, except those of some epileptics, presented well-marked appearances of deficient or degraded nutrition: they were all more or less atrophied; and the majority of them were atrophied in a far greater degree than I had ever observed to occur in the brains of persons not dying insane.

Knowing that it would be useless to add to the existing accumulation of barren observations; and that nature having returned negative answers to inquiries put in one manner, it would be absurd to expect more positive ones unless the method of interrogation was varied; and the microscope having been employed diligently, but in vain; I bethought me of the most feasible plans for determining in what manner, and to what extent, the nutrition of the brain was altered. For this purpose, it appeared most desirable that the actual weight of the organ should be compared with that which would have been its weight in a state of health; and the indications of what this would have been were sought for in alterations of its specific gravity, and in the amount of shrinking from its bony case.

In the annual 'Report of the Devon Lunatic Asylum,' published at the end of the year 1851, I gave a list of examinations in which the capacity of the cranium, the weight of the brain, and the specific gravity of the cerebrum and the cerebellum, was marked. The specific gravity of the nervous centres has since been investigated by Dr. Sankey, Dr. Oliver, and others. Dr. Sankey's elaborate and interesting paper on this subject, published in this Journal in January, 1853, is particularly valuable, as affording means of comparison, his observations having been made upon persons not dying insane. I have myself published a second and a third series of observations in the 'Lancet' of December, 1852, and in the last two annual reports of this institution.

The question of specific gravity, however, although an important one, I have always felt to be subservient to that of atrophy of the brain, as indicated by shrinking of its substance, or by loss of specific gravity without shrinking. Variations in the specific gravity appeared likely to indicate that which the microscope would not show—namely, whether, as in degeneration in the nutrition of other organs, an excessive deposit of

albuminous material ever took place in the brain, rendering its specific gravity higher than usual; or whether, on the other hand, its specific gravity might not become lighter in consequence of fatty degeneration.

I think it highly probable that the rare and curious condition known as hypertrophy of the brain, may depend upon abundant interstitial deposit of albuminous material; and that the pathology of at least some cases of epilepsy may be owing to a less degree of the same condition. Careful organic analyses can alone solve this question with certainty; but in the meantime, the increased density and specific gravity of the brain in epileptic cases, and the analogy of similar pathological changes in other organs, render the opinion provisionally tenable.

The belief expressed in my paper above referred to, that many cases of diminished specific gravity are owing to the conversion of the proper brain substance into fatty matters, has of late received confirmation and authority from the researches of Henle and Meckel into the degeneration of the contents of the brain-cells into cholesterine, forming what they call violet-fat (*spek-violett*), from its blue reaction with iodine and sulphuric acid, and which Henle believes to be cholesterine arising from metamorphosis of the fat molecules of the nerve-cells.

In the paper above-mentioned, I stated that "I believed the investigations there recorded would establish the existence of two kinds of cerebral atrophy,—namely, positive atrophy, and interstitial or relative atrophy, which may or may not be coexistent. By positive atrophy I wish to indicate an actual shrinking of the brain, and by relative atrophy an interstitial change, wherein the active cerebral molecules suffer diminution, and inert materials are deposited."*

In the two first series of post-mortem examinations published by me, I endeavoured to show the amount of positive atrophy by comparing the external measurements of the cranium and its capacity with the weight of the brain. Professor Sharpey, however, suggested to me that the more simple and complete method would be to compare the capacity of the cranium for water, with the amount of water which the brain would displace. This plan I have since adopted, and have found it satisfactory. I have not, however, discarded the old plan of measuring the cranium, believing that an extensive series of accurate cranial measurements of insane persons will be interesting, and may eventually prove useful.

It will be needful to describe with some minuteness the method I have adopted, to ascertain the amount of positive atrophy, or the degree in which the brain has shrunk, in each instance, from the cranial parietes.

The brain, including the medulla oblongata, is slowly immersed in a vessel of convenient size and shape, which is filled with water up to the level of a capacious spout placed at an acute angle with the sides. Before the brain is so immersed, the contents of the ventricles, and any serum which may be in the sub-arachnoid tissue, are allowed to escape through several long incisions. The organ is not allowed to remain immersed long enough to imbibe water, which it is capable of doing in large quantity, as proved by the experiments of Nasse. As it descends in the vessel the water it displaces escapes from the spout, is caught and mea-

* Lancet, Dec. 25, 1852.

sured, and affords a criterion of the actual bulk of the brain. (See 18th column of Table.)

The capacity of the cranium* is obtained by a somewhat more troublesome process. It is well known that one of the older physiologists employed millet seeds for this purpose; Sir W. Hamilton used sand: but neither of these methods would be feasible in the recent subject. The plan I have adopted is as follows:—The foramina at the base of the brain are carefully plugged with tenacious clay—that used by statuaries for modelling answers best; a small triangular piece of the frontal bone is removed with the saw: the calvarium is readjusted to the base, the dura mater being left attached. The space left by the attrition of the saw in removing the calvarium is filled up with clay, and a narrow bandage, with clay spread upon it, is made to surround the cranium three or four times, covering this space. If this manipulation has been carefully done, the cavity of the cranium will now be found as tight as a bottle. Sixty fluid ounces of water having been measured, a sufficient quantity to fill the cranial cavity is now poured from it by means of a funnel through the orifice in the frontal bone, taking care that the stream does not wash away the luting of the foramina. The fluid which remains after having filled the cranial cavity, is measured, and being deducted from the sixty ounces, gives the amount employed. (See column 19 of Table.) Thus, if nine ounces and two drachms remain, the capacity of the cranium was fifty ounces and six drachms; and if the amount of fluid displaced by the brain was forty-five ounces, the amount of atrophy was five ounces and six drachms. To this must be added half an ounce occupied by the luting, giving the actual amount of atrophy as six ounces and two drachms. Of course this examination is made before the chest is opened.

It is proper to observe, that although the above process was satisfactory in the majority, there were a few cases [and those of the most interesting, on account of the extensive atrophy they presented] in which a difficulty was experienced in draining off the effused serum from the brain. In most instances the serum effused under the arachnoid readily flows away through incisions made for that purpose; but in some, where the effusion has existed for a long period, the sub-arachnoid cellular tissue having become hypertrophied and tough, the serum contained within its meshes cannot be entirely removed. To this cause must be attributed some of the discrepancies which may be discoverable in the accompanying table.

The brain of a healthy subject fills the cavity of the skull; so that the water it displaces, and that requisite to fill the cranial cavity, are very nearly equal. At the base of the cranium of a healthy adult, killed by

* Mr. Paget, in his *Lectures on Surgical Pathology*, vol. i. p. 78, says: "The hypertrophy of the skull, which may be called concentric, is that which attends atrophy with shrinking of the brain, in which there is diminution of its bulk. . . . The thickening is attended by remodelling of the inner table," &c. . . . "At whatever age, after complete closure of the cranial sutures, shrinking of the brain may happen, this hypertrophy of the skull may be its consequence." It is certain that this change is a very rare result of atrophy of the brain in the insane. It only occurred once in the 63 cases recorded in the table: and, in the course of my pathological experience among the insane, I have not met with it more than three or four times. Such skulls are not uncommon in museums, from which source, perhaps, Mr. Paget has derived his examples. An hypertrophied cranium is a preparation easily made, and which lasts for ever. There must be some other cause than atrophy of the brain to which the production of concentric cranial hypertrophy must be referred: perhaps often to venereal affection.

accident, it is rare to observe more than a few drachms of serum; and the sub-arachnoid space contains only enough serum to keep it moist.* It will be important to ascertain what amount of divergence from this standard may occur in old age, and in various wasting disorders without insanity. At the present time I have no opportunities for such observations. Where insanity has not existed long, and has not been intense, the brain is sometimes found to have shrunk in a degree scarcely appreciable.

In epileptic cases, where the disease has not produced any considerable amount of dementia, the brain is not found to have shrunk. The brains, indeed, of persons dying with epileptic mania are found to present no appearances of disease; the deviations from health which we are able to observe being attributable to the mode of death. In these cases our faulty and insufficient methods of investigation must bear the blame of failure.

In the 11 cases in the Table in which insanity was accompanied by epileptic seizures, the average amount of shrinking was only $3\frac{3}{11}$ ounces; the average of the whole number of cases being $5\frac{1}{4}$ ounces. In 6 of the 11 epileptic cases the amount of atrophy was inconsiderable, averaging less than two ounces; although, in several of these, the accompanying loss of mental power was great and of long duration. Cases of exaggerated dementia, in which, if uncomplicated, a large amount of cerebral atrophy might be confidently looked for after death, when of epileptic origin, often present a brain apparently well nourished and perfectly normal, or marked only by changes attributable to the mode of death. It must be acknowledged that the pathology of insanity caused by epilepsy is distinct from that of other forms, and that it can scarcely be said that a reasonable hypothesis has been as yet formed respecting it. In the examination of 33 brains of epileptics, I have only once found a spicula of bone projecting from the cranium, and once only a tumour: this pressed on the nodus encephali, and was mainly composed of plates of cholesterine.

The Table contains the measurements of 13 patients whose age exceeded sixty-five years, and in these the average amount of the cerebral atrophy was $8\frac{1}{8}$ ounces, or more than 50 per cent. above the average of the whole number. In 2 cases only was the atrophy less than three ounces, and in both of these cases the form of mental disease was mania, and the mental powers had suffered little loss.

The most common form of mental disease in old age is primary dementia, or an exaggerated condition of the state so well known under the name of second childhood. Cerebral atrophy is a constant concomitant of this form of mental decay, and may be always looked for, in

* Majendie entertains different opinions on this subject, and believes that a considerable amount of serum always exists in the sub-arachnoid space, serving the mechanical purpose of preventing concussion of the cerebral substance, as the liquor amnii preserves the fœtus. He describes four places, which he calls confluxes, where the cerebro-spinal fluid, as he calls it, collects in considerable quantities. The principal of these are formed by the reflexion of the arachnoid at the base of the brain. (See *Récherches Physiologiques et Cliniques sur le Liquide Cephalo-Rachidien*, 1812.) It is true that in perfectly healthy brains a small quantity of fluid, rarely exceeding half an ounce, is found at this spot, bathing the roots of the cerebral nerves. Majendie examined and measured the fluid from the spine and the brain together, obtaining it by tapping the lower end of the spinal theca. From observations I have made on the relative quantity of fluid to be obtained from the theca of the spine and cranial cavity, I am persuaded that the greater portion of the fluid in Majendie's experiments came from the former source.

extent varying with the loss of mental power which has occurred before death closes the scene.

Why, in some aged persons, the cerebral centres fail in the power of appropriating due nourishment at a comparatively early period, and while the digestive, the circulatory, and the respiratory systems are still healthy and vigorous, may be owing to hereditary predisposition, or to their having been subjected to more work, or to ruder shocks of emotion, than common.

That cerebral atrophy in aged persons is not dependent upon failure of the functions of alimentation and general assimilation, is shown by the fact, that persons suffering from such atrophy are for the most part well nourished, as relates to the body at large. That some cases of cerebral atrophy do depend upon defective alimentation is more than probable. When persons have been starved to death, cerebral symptoms allied to mania have never been wanting. Whether these have been owing to the influence of decayed matter not eliminated from the brain, in consequence of new material not being supplied; or, as Liebig thinks, in consequence of commencing oxygenation, or eremacausis of the cerebral molecules,—they prove that the encephalon suffers speedily and seriously from want of a due supply of nutritive material.

But it may be asked, in reference to senile dementia, whether a gradual decay of the great nervous centre is not an inseparable concomitant of advancing age, and whether the failure of its functions is not the certain and necessary cause of death, in default of other causes which may all be called accidental? When the golden bowl of life is not broken by chance, is not the nervous tissue the silver thread which must give way under the tension of age and the implacable shears of destiny? The constant decadence of mental power in advancing life, and the annihilation of mind where the course of life has been greatly prolonged, indicate that such is the fact. The repute for wisdom possessed by age is perhaps mainly due to the decay of the passions being more rapid than that of the intelligence. If a man does happen to pass into the second century of his existence, sound in wind and limb, heart-whole and hungry, his mind never fails to give way. The Nestors of history are more than mythical,—they are impossible. And the proverb, that “the strong man dies upwards,” is untrue; the strong man, like the aged oak, decays first at the top. The period when the decay commences is very variable. Southey relates a good story of a traveller arrested by the pitiful sight of an aged man sitting in the porch of a farm-house and weeping bitterly: in reply to an inquiry as to the cause of his tears, the old man said he wept because his father had beaten him. Curious to ascertain the truth of so strange an assertion, the traveller entered the house, and encountered a still older man, but hale and vigorous. He acknowledged that he had beaten his son, and that it served the latter right, for he had been plaguing his grandfather,—pointing to a mindless centenarian in the chimney corner. The grandson was in his dotage, the grandfather was mindless, but in the middle term of this strange family group mental decay had scarcely commenced.

The greatest amount, however, of cerebral atrophy observed in any case, was unconnected with advanced age. The case was so remarkable and instructive that I shall venture to give a few of its details.

EXAMINATION OF THE ENCEPHALIC ORGANS IN SIXTY-THREE CASES OF INSANITY.

Patient's number.	Sex.	Age.	Form of mental disease.	Supposed cause of mental disease.	Duration of mental disease.	Apparent cause of death.	Measurements of body.		Measurements of cranium (scalp removed).				Thickness of grey matter of cerebral convolutions in 16ths of an inch.	Specific gravity of cerebrum: tubular neurine.	Specific gravity of cerebrum: vesicular neurine.	Specific gravity of cerebellum.	Weight of brain and meninges oblongata (avoirdupois).	Displacement of water by brain in fluid ounces (apothecaries).	Capacity of cranial cavity for water (fluid ounces, apothecaries); half an ounce added for lining.	Amount of atrophy shown by difference between displacement and capacity.
							Height.	Breadth from one acromion to other.	Circumference.	Between meatus and over vertex.	Root of nose to occiput, protuberance.									
1196	M.	28	Dementia	Blow on head ...	3 years	{ Apoplexy, with } convulsions	5 3 1/2	15	1 9 1/2	13 1/2	14	1 1/16	1.034	1.040	1.045	3 0 1/2	44 1/2	48	3 1/2	3 1/2
155	F.	37	Idiotcy	Malformation ...	Congenital...	Heart disease	4 7	13	1 8	13	13 1/2	1 1/16	1.043	1.034	1.043	2 0 1/2	37 1/2	40 1/2	2 1/2	2 1/2
809	M.	18	Epilepsy	Epilepsy	4 1/2 years	Peritonitis	5 9	14	1 8 1/2	12 1/2	14 1/2	1 1/16	1.034	1.041	1.041	3 3 1/2	49	49 1/2	2 1/2	2 1/2
1121	F.	23	Melancholia	Unknown	11 1/2 months	Phthisis	5 4	13 1/2	1 8	12	13 1/2	1 1/16	1.041	1.034	1.041	2 8 1/2	39	41 1/2	2 1/2	2 1/2
523	F.	77	Mania	Unknown	7 years	Phthisis	5 1 1/2	14	1 8 1/2	12	13	1 1/16	1.040	1.048	1.035	2 13	43 1/2	45 1/2	2 1/2	2 1/2
1186	F.	67	{ Chronic mania, } with dementia	{ Death of hus- } band and son	5 yrs. 9 wks.	Old age	4 10 1/2	13	1 8	11 1/2	12	1 1/16	1.032	1.032	1.041	2 8 1/2	37 1/2	43 1/2	6	6
1133	F.	30	{ Mania: general } { paralysis	{ Reduced cir- } cumstances	11 months ...	General paralysis ...	5 3 1/2	12	1 8 1/2	12 1/2	12 1/2	1 1/16	1.034	1.034	1.040	2 9 1/2	40 1/2	47 1/2	7	7
377	F.	33	Idiotcy	Malformation ...	From birth	Apoplexy	4 8	11 1/2	1 7	11 1/2	12 1/2	1 1/16	1.043	1.038	1.039	2 7 1/2	36 1/2	40 1/2	4	4
498	F.	41	Epileptic mania ...	Puberty	Unknown ...	Epilepsy	5 2 1/2	15	1 8 1/2	12 1/2	13	1 1/16	1.041	1.040	1.041	3 1 1/2	45 1/2	51 1/2	6 1/2	6 1/2
255	M.	57	General paralysis ...	Blow on head ...	Unknown ...	Convulsions	5 6 1/2	15	1 10	13 1/2	14	1 1/16	1.041	1.041	1.041	2 13 1/2	43 1/2	52	8 1/2	8 1/2
147	F.	53	Mania	{ Disappointed } affections	Unknown ...	Bronchitis: phthisis	...	12 1/2	1 8	13	13 1/2	1 1/16	1.035	1.035	1.035	2 11 1/2	41	44 1/2	3 1/2	3 1/2
1129	M.	66	Dementia	Fever	4 yrs. 8 mos.	Softening of brain	5 6 1/2	14 1/2	1 9 1/2	13	14 1/2	1 1/16	1.036	1.032	1.032	2 14 1/2	43	56 1/2	13 1/2	13 1/2
1127	M.	21	Epileptic mania ...	Too rapid growth	4 years	Epilepsy	5 5	14	1 8	11	13 1/2	1 1/16	1.042	1.041	1.042	2 0 1/2	42	44 1/2	2	2
1181	F.	38	General paralysis ...	Dissolute habits	Unknown ...	{ Paralysis with } epilepsy	5 5 1/2	13	1 7 1/2	11 1/2	12 1/2	1 1/16	1.042	1.041	1.042	2 9	40	42 1/2	2 1/2	2 1/2
1067	M.	24	{ Melancholia: } deaf and blind	{ Reduced cir- } cumstances	1 year 7 mos.	Phthisis	5 7	15	1 8 1/2	12	12 1/2	1 1/16	1.044	1.035	1.049	2 14 1/2	44 1/2	52 1/2	8	8
1064	F.	60	Melancholia	Unknown	2 1/2 years	Paralysis	5 2	13 1/2	1 8	12	13	1 1/16	1.036	1.033	1.042	2 11	39	40 1/2	1 1/2	1 1/2
1197	M.	27	Idiotcy	Malformation ...	From birth	Phthisis	5 5	13	1 9	13	13 1/2	1 1/16	1.040	1.038	1.042	2 15 1/2	40	44 1/2	4 1/2	4 1/2
869	M.	37	Mania	Unknown	5 yrs. 3 mos.	Results of apoplexy	...	13	1 9	12 1/2	14	1 1/16	1.033	1.035	1.034	2 7 1/2	37 1/2	53 1/2	15	15
1223	F.	55	Mania	{ Prostitution: } intemperance	5 months ...	Results of apoplexy	5 7	13 1/2	1 8	13	13 1/2	1 1/16	1.041	1.035	1.041	2 7	37	43	5	5
517	M.	35	Epilepsy	Epilepsy	2 months ...	Phthisis	5 8	15 1/2	1 8 1/2	12	13	1 1/16	1.041	1.039	1.040	2 13 1/2	41 1/2	48	6 1/2	6 1/2
1040	F.	75	Chronic mania	Unknown	2 months ...	Bronchitis	4 10	14 1/2	1 8 1/2	12	12 1/2	1 1/16	1.039	1.039	1.041	2 12	41	47 1/2	6 1/2	6 1/2
431	F.	13	Epilepsy	Fright	7 yrs. 5 mos.	{ Bronchitis and } epilepsy	4 3	10	1 8	12 1/2	11 1/2	1 1/16	1.039	1.039	1.039	2 12	42	44	2	2
712	M.	49	Melancholia	{ Anxiety abt. } business	4 yrs. 1 mo.	{ Dysentery and } heart disease	5 6 1/2	15	1 8 1/2	12	13 1/2	1 1/16	1.036	1.038	1.036	2 13 1/2	44	44	0	0
488	M.	72	Dementia	Not known	Congenital...	Chronic pleurisy ...	6 6	14	1 10	13	14 1/2	1 1/16	1.036	1.036	1.036	3 7	53	58 1/2	5 1/2	5 1/2
310	M.	34	Epilepsy	Not known	Unknown ...	Epilepsy	5 4 1/2	14	1 9 1/2	12 1/2	14 1/2	1 1/16	1.049	1.041	1.041	3 11	56	58 1/2	2 1/2	2 1/2
763	F.	40	Mania	Intemperance ...	4 yrs. 4 mos.	Phthisis	5 7	14 1/2	1 8 1/2	11 1/2	13	1 1/16	1.041	1.037	1.041	2 11	42 1/2	43 1/2	1	1
701	M.	68	Mania	Not known	7 years	General paralysis ...	5 7	16	1 9 1/2	13 1/2	13 1/2	1 1/16	1.042	1.033	1.042	4 0	40 1/2	55 1/2	8 1/2	8 1/2

111

J. N., No. 869, was a blacksmith, aged 37, and was admitted into the asylum on the 24th of March, 1851, after having been subject to paroxysms of mental excitement, alternating with depression, for two years. He remained in a state of chronic mania with enfeebled faculties to the 29th of September, 1853, when the gardener brought him in from work, because there was something the matter with his hand. On examination, the fingers of the right hand were found to be flexed; on being extended they remained so. There was ptosis of the right eyelid, and other symptoms of an apoplectic seizure. He was cupped in the nape, and appeared relieved; but from this time he gradually lost all mental and bodily power, had much difficulty in swallowing, and became extremely emaciated. It was thought impossible that he could live into the new year. He did, however, to the surprise of every one, survive to the 7th of July last. During the latter months of his life he had several deep and extensive bed sores. One of these was remarkable. It was on the outer side of the right knee, and was $3\frac{1}{2}$ inches in diameter. At the bottom of it the articular cartilage of the femur was exposed to view for the length of an inch; there was no evidence of the active inflammation so general when large joints have been opened, the cartilage looked healthy, and there was an abundant discharge of synovia.

He died on the 7th of July last, in a state of extreme emaciation, having lived for many months without the power of speaking or moving, and, it is to be hoped, without that of feeling. The measurement of the body could not be taken, as the knees were drawn up over the stomach, and the legs could not be extended. The remains of a sanguineous effusion, which had become fibrinous and tough, was found *completely enveloping the cerebrum*, and extending about two inches down the spine. It was situate in the cavity of the arachnoid. Over the vertex and the sides of the brain this membrane or clot was nearly half an inch thick, and was of the colour of venous blood; in the petrous fossæ it was also thick, but had become yellow. It did not extend over the cerebellum, but the effusion from which it was formed had found its way into the lateral ventricles, as these contained masses of dark red fibrin. Between the visceral arachnoid and the brain there was a large amount of serous effusion. The viscera of the chest and the abdomen were in a very healthy condition. The cranial cavity required $52\frac{1}{2}$ ounces of water to fill it. The brain displaced only $37\frac{1}{2}$ ounces, so that the atrophy of the organ was equivalent to 15 ounces. The brain, in fact, retained little more than two-thirds of its normal size.

The pathology of this interesting case is sufficiently clear. The patient was already the subject of cerebral atrophy when the cerebral hæmorrhage took place, otherwise the enormous amount of cerebral hæmorrhage which the above-described fibrinous remains prove to have existed, must have produced effects which he could not have survived many hours. As it was, the blood only displaced so much serum, driving it into the spinal recesses, and the brain escaped a pressure which, under other circumstances, must have been speedily fatal.*

* This opinion coincides with the third inference drawn by the late Dr. Sims from his cases in the first of his instructive papers in the nineteenth volume of the *Medico-Chirurgical Transactions*, p. 313: "That in persons whose brain has long contained a large quantity of fluid, with or without any trace of previous apoplectic extravasation, and who have suffered a first

The mischief done to the brain was sufficient to interfere with the innervation of the whole body, and all the functions suffered in their turn. The case affords an instructive instance of slow death from deterioration of the nervous centres, the remaining viscera being healthy. In cases of chronic mania, of dementia following mania, and of primary dementia, the amount of cerebral atrophy may generally be calculated upon by the enfeeblement of mental power. In all these forms of disease I have invariably found some amount of atrophy, and I have, for the most part, found this amount to correspond with the degree of mental decadence estimated with its duration. The first of these conditions, that is the degree, it is impossible to tabulate; the second, that is duration, it is not easy to show accurately in a tabular form. The sixth column of my Table represents the duration of disease from the period of the first appearance of symptoms, as nearly as it could be ascertained. These symptoms may have been slight for years, and grave for a short time only before death. In the first case, for instance, the duration of disease is stated to be three years; but until a few weeks before death the symptoms were only an inconsiderable loss of memory and of mental powers; the duration of the active symptoms was brief, and the patient was only in the asylum nine days. It must, therefore, be confessed that a comparison of this column with that showing the amount of atrophy will not be satisfactory. The details of each case, as shown in the case-book, would alone be sufficient to prove the extent of correspondence between the amount of cerebral atrophy, and the degree and duration of mental decadence. But it must not be thought that extensive atrophy is only found where the mental symptoms have been those of impairment or loss of function. It is not inconsistent with much mental excitement, or with numerous delusions, but such excitement is powerless, and the delusions are transitory and puerile. Whether *measurable* atrophy exists in the early* stages of acute mania and melancholia, the data I possess do not prove; although they are amply sufficient to demonstrate that the cerebral conditions upon which these forms of insanity depend, tend to pass into an appreciable and measurable shrinking of the brain substance, unless the healthy cerebral action be speedily restored. Is not this tendency sufficient to attest that mania and melancholia, however acute their symptoms, depend upon a perverted nutrition of the encephalon?

attack or a subsequent attack of fatal apoplexy, it is probable that the brain, from this cause, is rendered less impatient of injury from extravasation of blood." The two cases, however, to which he refers only survived the apoplectic attack eighty and thirty hours.

* That excellent mental pathologist, Dr. John Webster, has published, in the last number of the *Psychological Journal*, a series of 40 post-mortems made at Bethlem, which greatly confirm the views here advocated. Most of the dissections are those of recent cases, and yet "*serous infiltration of the pia mater*" is described as occurring in them, with few exceptions. Some of the chronic cases were undoubtedly good illustrations of cerebral atrophy. Thus, in No. 7—"In hospital six years and fifteen weeks. . . . The pia mater was infiltrated with clear serous fluid; and the convolutions, apparently compressed, were separated from one another, so as to allow the end of the finger to be inserted between." In No. 21—"In hospital twenty-five years and four months. . . . There was great and general infiltration, with clear fluid of pia mater covering hemispheres. Several convolutions considerably shrunk, leaving intervals occupied by infiltrated pia mater." In No. 26—"In hospital thirty years. . . . Pia mater was considerably infiltrated. Convolution of hemispheres shrunk, so as to leave in several situations, intervals filled by infiltrated pia mater." Dr. Webster would have added greatly to the value of these details if he had appended to them some brief account of the symptoms during life. They stand at present like fractions without an enumerator.

phalon?—a condition which may pass into that of health on the one hand, or, on the other, into that of chronic degradation and atrophy? All the causes, even of the acute forms of insanity, point to an interference with the due nutrition of the brain. Starvation causes raving delirium; mortification produces muttering delirium; the poison of typhus, which, perhaps, more than any other agent, interferes with the nutrition of all parts of the body, throws the mind into a state between melancholy and stupidity, and not unfrequently causes maniacal excitement. It is highly probable also that even the moral causes of the acute forms of insanity do not operate in a different fashion. If anything is positively known of the brain and its functions, it is that it expends its powers during the waking state; and that it is nourished, and its powers are recruited, by cell-growth or otherwise during sleep. But the moral causes of insanity are especially those which “murder sleep,” and thus the conditions of due cerebral nutrition are prevented. During the prodromic period of threatened insanity, opiates often act like a charm. Acute madness from moral causes rarely, if ever, comes on without preceding sleeplessness, that is, without a state in which the cerebral structure is for a long time undergoing waste without repair. After insanity has become established, the power of sleep is often regained without benefit: the balance of cerebral exhaustion and nutrition having been upset, is not always so easily righted; the sleep gained being only sufficient to repair the last losses of the organ, and not those which have been accumulating; it may be able to meet current expenses, but not to pay off old debts. And this may account for the fact, that chronic lunatics are not, on the whole, bad sleepers.

But although the production of sleep does not ensure the recovery of a patient labouring under acute mania, with as much certainty as it does that of a patient with *delirium tremens*, it is nevertheless true that the most satisfactory and rapid recoveries from acute mania take place in consequence of the early and powerful influence of “nature’s sweet restorer.” There is no better guide in the prognosis of a recent case of acute mania than the occurrence of deep, long, and refreshing sleep, or the persistence of restless insomnia. The one gives the brain a chance of regaining an equilibrium between nutrition and excrementation; the other plunges it down the hopeless descent of regressive metamorphosis.

The progress of such deterioration may be rapid or slow. It would, at the present time, perhaps, be rash to venture an opinion how soon it may be measured. It appears to me highly probable that many instances of cerebral disease which have been designated by the ambiguous term of serous apoplexy, are, in reality, owing to interrupted nutrition of the cerebral substance and rapidly proceeding atrophy. We have seen that, if the decay of the nervous functions takes place slowly, the life of an individual may be prolonged for a considerable time after sensation, voluntary motion, and cerebral power have been almost annihilated. But when the interference with cerebral nutrition has been more sudden, the checked innervation of the body is accompanied by more acute symptoms, greatly resembling certain cases of apoplexy.

No. 1125, a woman, aged 49, in consequence of grief at the death of her husband, suffered during six years from occasional maniacal excite-

ment of mild character; in July, 1853, she suddenly became paralytic over the whole body; the loss of power over the lower extremities was most conspicuous, but all the voluntary and the semi-voluntary muscles appeared to participate. She had great difficulty in swallowing, and her speech was unintelligible. The paralysis did not preponderate on either side of the body. She slowly recovered sufficiently to move about without help, and to articulate with distinctness. In the second week of August of the present year the symptoms returned, and in a few days she became completely paralytic over the whole body. The paralysis affected the abdominal muscles, the bladder, the orbicularis palpebrarum, the muscles of the pharynx, and those of the tongue. The tongue lolled out of the mouth; even the muscles of respiration were partially affected, the breathing being slow and shallow. For several days the pulse was good; it then failed. A careful examination of the body (which was well nourished) showed all the organs to be healthy, except the cerebro-spinal centres, which were much atrophied.

In the wards of this asylum I have seen many similar cases, and a still greater number wherein the symptoms of nervous decay have been less rapid in their progress, and less unmixed with those of disordered action of other organs. The loss of innervation from atrophy of the brain shows itself more frequently by causing some other weak organ to give way, than by purely nervous symptoms. Either a diseased heart, with which, however, a man with healthy brain might have lived many years, ceases to beat, from failure of the nervous function; or lungs or extremities become gangrenous; or inflammations of low type establish themselves. The immediate cause of death, in fact, may be found in various parts of the body, the remote but primary cause being impaired or debilitated nervous function.

The rapidity with which cerebral atrophy marches towards a fatal termination, is rarely so great as in the last instance cited, or so slow as in the former one. The deaths which, in asylum obituaries, are to be found attributed to such causes as "gradual decay," "failure of powers of nature," "decay of nature," are, we believe, mostly due to cerebro-spinal atrophy, and its train of symptoms.

Sir Henry Holland, in writing on what he calls deficiency or defective production of nervous power, says—

"I find in my notes two or three singular cases, in which there existed what could only be interpreted as a deficiency of nervous power, without any obvious bodily disorder, except what this deficiency produced, and unconnected with any aberration of mind, but testified by a general torpor of all the functions of both. In the most remarkable of these cases, where the symptoms coming gradually upon a vigorous frame of body, lasted for months, all the voluntary movements of walking, speaking, eating, &c. were in a sort of abeyance—the mind inert, as if unable to force itself into any effort of thought or feeling, the circulation very feeble, and great torpor of the natural functions. The cessation of this state was as gradual as its commencement, and as little explained by any obvious cause."*

These cases strongly resemble those I have referred to, and were probably owing to a temporary and removable impediment to the due nutrition of the cerebro-spinal axis, occurring in persons with a healthy assortment

* Chapters on Mental Physiology, p. 279, note.

of other viscera. Those met with in asylums arise from a more grave and deeply-seated deficiency in the nutrition of these all-important organs.

With its full composition of causes we are at present unacquainted. At the pathological origin of atrophy of the brain, it is, at present, difficult even to guess. There are many reasons for believing that it is unconnected with constitutional tuberculosis. The insane, as a class, are not more liable to phthisis than the sane, perhaps less so. In Sir A. Morrison's paper 'On the Statistics of Insanity,' he shows that during thirty years he has treated 6779 cases of insanity in different public institutions; that of these 1440 died, of whom only 164 died of pulmonary consumption. Tubercle is very rarely deposited in the brain after childhood. I have myself only found tubercle in the brain in one solitary instance, this being an idiot boy, who died of tubercular peritonitis. Again, insane patients who die of phthisis, do not present a greater amount of cerebral atrophy than might have been anticipated from, or that corresponds with, the amount of mental decay. Of the 11 patients in the Table who died of phthisis, the average cerebral atrophy was $3\frac{1}{4}$ ounces, or $1\frac{1}{2}$ ounce below the average of the whole number. In the few phthisical cases I have examined, in which the mental symptoms have been recent and moderate in degree, the brain was found to have undergone only a slight degree of atrophy. No. 763 in the Table presents a good example. She was four years four months insane, but retained much mental power. The cerebral atrophy was only 1 ounce. As far, therefore, as the individual is concerned, there appears to be no connexion between tuberculosis and cerebral atrophy. But if the question is somewhat widened, the answer may be different, since there can be little doubt, that persons belonging to consumptive families are more liable to insanity than others. An hereditary and radical fault of nutrition, which, in some individuals of certain families, develops itself in the form of pulmonary consumption, produces in others a tendency to the cerebral changes which we are discussing. Also the modes of life which in some produce phthisis, in others produce insanity, dependent upon faulty nutrition of the brain substance; and I feel myself justified in making the assertion, that the hygienic, if not also the therapeutical, measures which are most useful in preventing or relieving tuberculosis, are the most efficient in the prevention or the treatment of insanity. A young lady of weakly constitution and sensitive mind is compelled to battle for a livelihood as a governess, and if distressed by adverse circumstances, the chances are great, that if she does not become consumptive, she will become insane. An artizan, condemned by the exigencies of a large family to work hard, to fare badly, and to breathe the tainted air of the crowded workshop, or the more crowded lodging, is exposed to the same alternative; the probability being, that if the brain is more excitable or impressible than common, the radical fault of nutrition will manifest itself in that organ; but if otherwise, that the lungs will give way. In either case, an early change of circumstances, with country air, cheerful occupation, nutritious food, tonic remedies, as the shower-bath, and, above all, sufficient rest, are often found capable of checking the progress of symptoms, whether they be cerebral or pulmonary, and not unfrequently of effecting a cure. These considerations col-

lectively are more than enough to establish a strong analogy between the most common pathological conditions of insanity, and of that peculiar disease of the system (tuberculosis) which no one doubts to be dependent upon perverted nutrition.

Cases of insanity undoubtedly exist which are, in the first instance, attributable to causes acting by the production of inflammation of the brain, or its membranous envelopes; but these exceptions confirm rather than invalidate the rule, insanity being in them secondary to, and consecutive upon, the inflammatory symptoms, and dependent upon the nutrition of the brain being interrupted or vitiated by the inflammatory changes. The analogy we have before used will again serve to explain our meaning; since it is universally admitted that inflammatory diseases of the lungs are frequently succeeded by the development of tubercular deposit. Bronchitis and pneumonia are attended by groups of symptoms, distinguishable, but not altogether dissimilar to those of phthisis; so also the symptoms of phrenitis are distinguishable, but not altogether unlike those of insanity. The means of diagnosis are far more complete in the one instance than in the other; but at the present day, the pathologist who is unable to distinguish between phrenitis and mania will scarcely be thought a first-rate craftsman. That a purely inflammatory condition of the brain may be one stage of insanity, is as true as that an inflammatory condition of the lungs may be the earliest stage of pulmonary consumption—that is, strictly speaking, it is not true; but the inflammatory condition may be, and frequently is, a preceding but distinct disease, to which true insanity is consecutive. That inflammation is more frequently followed by defective nutrition in the brain than in the lungs, is no doubt owing to the latter organs having a far greater power of repairing pathological injuries than the former. Pericarditis is a frequent cause of fatty degeneration of the heart; and it must be considered an established rule, that a common source of perverted nutrition is the change effected in the intimate structure of organs by inflammatory action.

Opinions on the ultimate nature of the nutritive defect which results in cerebral atrophy and insanity, must necessarily be speculative, since the ultimate nature of nutrition itself is unknown to us.

Its apparent and exciting causes may be classified as follows:

1. In predisposed persons it may depend upon poverty of blood, since it is producible by deficient food and by diseases interfering with the alimentative processes; and since an analogous train of symptoms occurs during starvation.

2. It is probable that in other cases it may depend upon some derangement of "the intimate connexion between the nervous and vascular systems, through which their most important functions are performed." Because it is sometimes found to be accompanied by extensive disease of the minute cerebral capillaries, the coats of which can be shown to be subject to fatty or earthy decay.

3. A third class of cases would appear to be producible by the molecular change effected by blows or violent concussions, and followed by atrophy, owing to some process as yet unknown to us. Atrophy of a testicle from a blow, without inflammation, presents an analogous instance.

4. Another class of cases are those following inflammation, and perhaps also following frequent or long-continued congestion. The basis of inflammatory action is an abnormal state in the mutual relationship between the blood and the tissues. That this state effects changes in the tissues, which, if not speedily repaired, must be followed by conditions of degraded nutrition, is proved by the pathology of every organ in the body. The brain certainly offers no exception. The capillaries become blocked-up, or their coats become spoiled for the purposes of nutritive regeneration of the tissue.

It also appears probable that, during inflammatory or congestive conditions, albuminous matter or serous fluid may be effused by the capillary network into the intimate structure of the brain; thus separating its vesicles and tubules from the capillaries, and preventing the due nutrition of the elements of nerve-structure. For this form of atrophy, we have formerly suggested the prefix of *relative*, as it may exist where there is no shrinking of the brain; atrophy with shrinking being termed *positive*. The two, however, may, and frequently do, co-exist.

5. The most numerous class, however, is that which depends upon want of rest, and the especial period of nutrition of the brain—namely, sleep. Want of refreshing sleep I believe to be the true origin of insanity, dependent upon moral causes. Very frequently, when strong emotion tends to the production of insanity, it causes, in the first instance, complete loss of sleep. In many cases, however, the power of sleeping is not lost, but the quality, so to say, of the function is perverted, the sleep being so distracted by agonizing dreams that the patient awakens jaded rather than refreshed. I have known several instances in which patients becoming convalescent from attacks of acute mania, have distinctly and positively referred to frightful dreams as the cause of their malady; and it is probable that a certain quality of sleep, in which dreams exciting terror and other depressing emotions more forcibly than waking events are likely to do, is not less adverse than complete insomnia to the nutritive regeneration of that portion of the brain on whose action the emotions depend. In such a condition, it is highly probable that the very portions of the brain which most need a state of rest are even, during the sleeping quiescence of other portions, more wastefully engaged in the activity of their functions than they could be in the waking state. The mainspring of insanity is emotion of all kinds; this, stimulated by phantasy, and emancipated from the control of judgment, during harassed sleep, may be more profoundly moved than at any other time. Bichat considered sleep to be a very complex state, in which it was possible for the cerebral functions to be in very different conditions of quiescence or activity: “Le sommeil général est l'ensemble des sommeils particuliers;” and he considered that dreams represent the active or waking condition of certain of these functions during the repose of the others. In this manner, a patient, some one or other of whose emotions has been profoundly affected, may continue to be sleepless, as far as the activity of the particular emotion is concerned, although he may by no means be the subject of general insomnolence; and this consideration will afford what seems to be a fair explanation of the exceptional cases to the rule, that the moral causes of insanity act by preventing the due nutrition of the brain, as it occurs during sleep:

" His slumbers—if he slumber—are not sleep,
But a continuance of enduring thought."

The only cases of insanity which are not thus reconcilable to this rule, are those which arise from the gradual development of one mental faculty, by frequent exercise, until its power becomes preponderating and excessive. Such cases may and do arise without loss or deterioration of sleep. But are they rightly classed as cases of disease of the mind? They seem to be instances of irregular development of the mental faculties, but not of morbid change; and, in the opinion of the writer, they cannot truly be considered instances of disease, a term which implies morbid change.

Delirium tremens presents an instance of real insanity (*plus*, an affection of the spinal nervous system), in which the intimate relationship between the symptoms of mania and those of insomnia are remarkably apparent; and cases of unmixed mania are numerous in which this relationship is not less intimate. Mania follows sleeplessness; if sleeplessness is obviated by appropriate remedies while mania is threatening, the explosion of the latter is prevented; and even after the invasion of maniacal symptoms, if continued and refreshing sleep is procured, these symptoms are frequently cut short. The relationship is unquestionably that of cause and effect. In the clinique of hospitals for the insane, cases do undoubtedly occur in which the cerebral symptoms are exaggerated after sleep; but the sleep in these cases is not continued and refreshing; it is broken sleep, sufficient only to recruit the sensory-motor energies of the patient; sleep in which the portions of the brain implicated by disease do not participate. Such cases are often, also, of a mixed character, between mania and phrenitis, in which the cerebral congestion produced by sleep has a prejudicial influence upon those portions of the brain tending to an inflammatory condition: for it must not be forgotten that the brain is a large and complicated viscus, and that different portions of it may at the same time be in very different pathological states. Congestion, inflammation, regeneration, nutrition, or degenerative atrophy, may all co-exist in the brain, as in the lungs or any other complex organ.

If the above views are correct, they will support the following propositions. That the brain substance alternates between three distinct physiological states.

1st. The state of sleep, which is complete if it embraces all parts of the brain. During this state, and this state only, nutritive regeneration of the cerebral organism takes place; and this regeneration will be universal or partial, according to the general or partial rest of the cerebral functions.

2nd. The state of wakefulness, in which the functions are alert, but not active. Like the strings of an instrument stretched up to concert pitch, but not vibrating. This state is always partial, because one or other of the cerebral functions is constantly in activity during wakefulness. It may be assumed as doubtful, but not improbable, that any portion of the brain whose function is alert, but inactive, during that time neither undergoes reparation nor decay. That it is in a condition of histogenetic equipoise.

3rd. The state of functional activity. This state, like the last, must ever be partial as relates to the cerebrum at large, and even to that portion of it which is concerned in the discharge of the intellectual and emotional

functions. That different mental states can succeed each other with wondrous rapidity is well known; but there are many cerebro-mental functions which cannot co-exist, although they may alternate with facility; others are liable to become preternaturally active, to the exclusion of that moderate and alternating activity which is especially conducive to mental health. During functional activity there is an exactly equivalent degree of histogenetic waste of the portion of the cerebrum concerned, a waste only to be repaired by nutritive regeneration during the first state, or that of sleep.

The cerebrum at large, therefore, may be in two opposite conditions; first, that of waking functional activity, accompanied by its equivalent of waste, and marked by equivalent decay of power and addition to the excrementitious phosphates; and secondly, that of functional repose, during which the waste is repaired.

But a portion of the cerebrum, discharging a particular function, may be in three different states: those of rest and of activity, and an intermediate state, which may be called that of quiescence, in which it is ready to act, alert, but not active. It may be active during the repose of the greater portion of the cerebrum in sleep, it may be quiescent during general cerebral activity in waking hours.

This difference between the brain at large, and any portion of it discharging a special function, will explain those otherwise anomalous cases in which insanity, from moral causes or intellectual overstrain, has taken place without any apparent loss of sleep. These apparent exceptions are not so in reality, and the rule remains intact, that functional activity of a cerebral organ implies equivalent waste, which, if not repaired during sleep, becomes permanent, and morbid decay of the organism takes place, soon to be followed by appreciable atrophy.

It is the pathological fashion of the day to attribute all diseases to defect or perversion of nutrition. This appears to me to be an error, at least in the use of terms: and in science an error in terminology is not unimportant. I must, therefore, be excused for taking some pains to explain, that whilst so generally attributing insanity to changes accompanied by diminution in the bulk and weight of the organ, I do not consent to view all these changes as dependent upon disease of nutrition alone. The history of the life of an organ is the aggregate of the history of its component cells. In the history of a cell there are three stages: that of its growth, that of its decay, and the intermediate one of its functional activity, which is dependent upon the first, and which causes the third, of which the third is, indeed, during health, the exact equivalent. Now, nutrition is as unlike decay as income is unlike expenditure. The one may be a measure of the other, but it is not necessarily so, and in the cerebral organisation I do not believe that it is so. The cell of a gland undergoes nutrition until it is mature, when it bursts, and its activity and decay are complete and commensurate. By following an analogy not sufficiently close to be trustworthy, the history of the cerebral vesicles has been assumed to be precisely similar. Professor Carpenter says, "In the vesicular tissue which constitutes the essential part of the nervous centres, there are appearances which indicate that its peculiar cells are in a state of continual development; newly-formed ganglionic vesicles taking the

place of those which have undergone disintegration."* He elsewhere likens their development and death to those of the epidermic cells. But, notwithstanding this high authority, we possess no actual knowledge inconsistent with the belief that the cerebral vesicles have a more durable existence, that the changes affected by healthy functional activity are exhaustive but not destructive, or that the same cell may not repeatedly pass through the three vital processes of functional activity, waste, and regeneration. Normal changes may give place to abnormal ones in either of these three states, and insanity, with shrinking of the brain, may thus be dependent upon the defective nutrition, excessive function, or disproportionate and irremediable decay of the vesicular neurine. Insanity following starvation or inflammation affords instances of the first; insanity caused by the storms of passion, or by intellectual overstrain, illustrates the second; and the insanity of old age, when the elements of the body tend more strongly to form chemical compounds than to perpetuate the organic ones, may serve to exemplify the third. To use the term, disease of nutrition, in all these cases would be to abuse it. The processes of life are nutrition, functional action, and decay, and the vital changes may become abnormal, that is, disease may commence in any one of these states or stages.

The facts indicated in the Table already given, and the estimate of them which I have attempted to give, will, I trust, be thought sufficient to establish the law, that the symptoms of insanity are accompanied by pathological changes in the brain, the most prominent characteristic of which yet demonstrated is shrinking of its substance. The intimate changes of the organization which cause the shrinking, we have yet to learn, by the combined use of chemistry and the microscope. It is very probable that morbid degeneration of brain-substance, like that of muscular tissue, takes place by the running together of its organic elements into forms of hydro-carbon. It may be that the microscope will remain incompetent to detect undoubted indications of such a change in the whole of the encephalon, since death may be inevitable before changes can occur in the intimate structure of the whole organ grave enough to be appreciable by the assisted sight. But when portions only of the brain have undergone degenerative change, I have repeatedly and easily seen an abnormal abundance of oil globules; and in several such instances I have observed abundance of the peculiar crystals of cholesterine. The brain appears to afford no exception to the law, that one stage of regressive metamorphosis of animal tissues is that of fatty or oily compounds. The recent researches of Meckel and Henle, on the formation of spek-violet (a combination of cholesterine and other fats), during cerebral decay, afford strong confirmation of this opinion.

Of the specific gravity of the brain [a subject which I was the first to investigate in this country, and which has since attracted the attention of several excellent observers] it must be acknowledged that, although the large number of facts now collected do not appear in themselves to be very instructive, they at least prove the existence of great and constant differences in the condition of the cerebral matter, the nature of which must be demonstrated by other means. The average specific gravity of

* Human Physiology, p. 563.

the 63 cases in the Table is, white matter of the hemispheric ganglion, $1.039\frac{2}{3}$; grey matter of ditto, 1.037; cerebellum grey and white matter, $1.040\frac{1}{2}$. Dr. Sankey gives 1.0412 as the average specific gravity of white matter of seventy observations made upon persons dying without head symptoms. He gives 1.0346 as the average of the grey matter.* Meckel states that the specific gravity of the brain of the insane is less than that of the sane. MM. Leuret and Mitivié give the former as 1.028, the latter as 1.031. Dr. Aitkin, of Glasgow, has found considerable difference (1.025 on one side, 1.031 on the other) between the specific gravity of the substance on two sides of the same brain, in a case of chorea, a fact which we have not been able to verify in epileptic cases. Upon what do these differences depend? Surely they are at least sufficient to stimulate powerfully the labours of the microscopist and the organic chemist.†

I feel painfully the deficiency which exists in this paper, in the omission of observations respecting the amount of alkaline phosphates secreted in the urine. As proved by Dr. Bence Jones, in his papers in the 'Philosophical Transactions,' for 1846, the quantity of these compounds in the urine may be regarded as a measure of nervous decay. The large amount of phosphorus contained in the brain, and the wide range between the quantity of this substance stated by L'Heritiér to exist between the brains of the sane and the insane, the young, the aged, and the adult, sufficiently indicate that a knowledge of the conditions of cerebriic phosphorus would greatly elucidate that of mental pathology. I have, however, found it so impracticable to determine the quantity of this substance in the urine of insane patients, principally from the difficulty of collecting the secretion, that with regret I have been compelled to turn aside from a research so promising of curious and important results.

I must conclude with some brief observations, which appear needful to illustrate the tabular form above given. The breadth of the grey matter of the convolutions given in the thirteenth column was ascertained by

* Vide No. 21, p. 241.

† Since this article has been written, I have had the pleasure and the benefit of reading the papers of the late Dr. Sims, in the nineteenth volume of the Medico-Chirurgical Transactions.

These valuable papers contain nothing adverse to the opinion, that atrophy of the hemispherical ganglia cannot take place without a corresponding decay of mental power.

The fourteenth deduction is, indeed, to the effect—"That in phthisis, diseases of the stomach, and other emaciating disorders, the brain, also, sometimes undergoes a process of wasting." (p. 380.) Professor Todd adopts this opinion as regards persons who have been long bedridden, and those who have been habitual spirit-drinkers. If the cerebral physiology now current be true, atrophy in these latter cases, occurring without decay of mental power, may reasonably be attributed to shrinking of the corpora striata and thalami, and perhaps, also, of the cerebellum, owing to long disuse of their functions. It would not be easy, in a post-mortem examination, to distinguish between an atrophied condition of the hemispherical ganglia, the seat of the purely mental functions, and a similar state of the ganglia of sensation and motion. Doubtless, in the majority of cases of cerebral atrophy, all parts of the encephalon are implicated; but where the functions of the brain which relate to the body alone, or those which relate to the mind, are separately impaired, it will in future be desirable to look for those indications of partial atrophy we may be taught to expect by physiological science. It is not sought in this paper to maintain the proposition that cerebral atrophy never occurs without mental disease; the converse and more limited one, that cerebro-mental disease never exists for a considerable time without atrophy of the brain, forms the whole of its scope. It is, however, inconceivable that the portion of the brain, upon the due action of which the special functions which are called mental, depend, can be atrophied without those functions falling into an abnormal condition.

measuring, with a pair of hair-dividers, inspected by a lens, the average breadth of the least oblique, that is, the narrowest, sections, made by a perpendicular slice through the hemispherical ganglion. In five instances the breadth was $\frac{9}{100}$ ths of an inch, of which 2 were cases of epilepsy, 1 of general paralysis, 1 of dementia, and 1 of melancholia. In one of these cases the cerebral shrinking was as much as $8\frac{1}{2}$ oz., in another as little as 2 oz. In 4 of these cases the brains were above the average weight. In 38 instances the depth of the grey matter was $\frac{7}{100}$ ths of an inch; in the remainder it was $\frac{8}{100}$ ths.

The measurements of the cranium expressed in the 10th, 11th, and 12th columns were taken after the removal of the scalp, in order that they might not be vitiated by differences in the thickness of the scalp, or by the abundance or deficiency of hair. The difference between measurements so made and others made before the removal of the scalp is not great; the principal being that of the circumference, which never exceeds an inch. The difference in the other measurements does not exceed half an inch each. A cranium measuring 21 in. in circumference will give a weight of brain somewhat exceeding 3 lbs., dependent, however, upon the vertical development; a small circumference with a high vertex giving as much cranial space as a large circumference with a low one.

ART. III.

Scarlatinal Dropsy. By JOHN W. TRIPE, M.D.

(Concluded from No. 27, p. 224.)

WE shall now describe the various forms of dropsy. These may be divided into two varieties; (1) *dropsy without albuminous urine*, (2) *dropsy with albuminous urine*; which latter may be further subdivided into (a) *the acute*, and (b) *sub-acute*.

1. Simple dropsy, or that with non-albuminous urine, is by no means so common as either form of the other variety, and supervenes usually within a few days after the disappearance of the scarlatinal rash. Its duration is usually less than that of the albuminous forms, the longest case I ever met with having lasted but four weeks, the average duration being about half that period. The danger attending it is slight; and, although fluid may collect in one or other of the serous cavities, usually of the peritoneum, yet I have never seen inflammatory visceral disease; coma, convulsions, or other symptoms of uræmia, occur, and have therefore termed it simple dropsy. In the case which lasted for nearly a month, the urine was tested almost every day, without albumen or any other abnormal constituents of the urine being discovered, save an abundant deposit of lithates, and a rather large quantity of disintegrated epithelium. In other cases a stray fibrinous cast or two have occasionally been met with. The symptoms attending this variety of dropsy are very similar to, though of much less intensity than, those characteristic of the other forms. Its ordinary course may be described as follows:—The patient becomes dull, indolent, and disinclined to move about, loses his appetite, complains of increased thirst: and his tongue, which had just

re-acquired its ordinary aspect, becomes slightly coated with a white fur, especially towards its base; and there is little, if any, increased heat or dryness of skin. In a few days the face gradually assumes the peculiar leuco-phlegmatic appearance so indicative of the disease; the mucous membrane of the lips, gums, and conjunctivæ become very pale; and the features look a little bloated (the contour of the face becoming fuller), but do not pit on pressure. The pulse is slightly, if at all, quickened, has no sharpness, but indicates diminished power, being soft and weak. In a day or two the eyelids become œdematous, and the effusion gradually involves the face, the extremities, and the rest of the body; and, after remaining for a period, varying from a few days to two or three weeks, gradually disappears. As before stated, the disease rarely, if ever, extends over a period exceeding one month from the invasion of the dropsy.

The next variety, which includes all forms of scarlatinal dropsy with albuminous urine, is of far more importance than the preceding, as all cases included in it arise from a diseased action, or from morbid alterations, of the kidney. As just stated, there are two sub-varieties—viz., the acute and sub-acute; but as the symptoms vary in degree only, being more intense in the acute than in the sub-acute, they will be included in one description: premising, however, that in the acute variety there is always more or less blood in the urine, and that it is attended with greater danger than the other.

The premonitory symptoms are tolerably well-marked, and consist in the persistence of the febrile symptoms beyond their ordinary duration, or in their reappearance after having ceased at the ordinary period. The child ceases to play about, becomes capricious and dull, his appetite diminishes, his eyes look heavy, and his pupils are dilated and act sluggishly. He also complains of thirst, languor and pain of the back, wants to make water more frequently than usual, and especially during the night; and his face gradually acquires the peculiar leuco-phlegmatic appearance so characteristic of the disease. On examining the urine, we find it to contain some of the abnormalities described in the previous article.* It is also more scanty than natural, of lighter specific gravity, and of a reddish-brown or a peculiar smoky opalescent tint. After these symptoms have extended over a period of from a day or two to two or three weeks, the patient complains of a deep-seated heavy pain of the back and loins, and sometimes of a more acute pain of the thighs and testes, headache, and frequently of nausea or vomiting. The headache is sometimes intermittent, or occasionally periodic, when it is usually very intense, precisely resembling brow-ague. Painful muscular spasms, resembling those of tetanus, are occasionally amongst the premonitory symptoms. In one case of spasm of the abdominal muscles, no small anxiety was caused by their intensity and duration; the pain was most severe, and lasted for nearly three days with very frequent intermissions of a few minutes' duration. In this case the diagnosis was not satisfactorily made out, until paucity of the urine was ascertained to be amongst the symptoms, when the urine was examined, and the case cleared up.

Another set of symptoms of occasional occurrence, are those arising

* See British and Foreign Medico-Chirurgical Review, pp. 244-46.

from another affection of the nervous centres—viz., a greater inclination to sleep, without any actual stupor, the patient being quite intelligent when roused. These symptoms are usually attended with a diminished secretion of urine, and consequent excretion of urea, and frequently co-exist with other symptoms of uræmia. Sometimes the only premonitory symptoms are languor, depression of spirits, and a gradually increasing pallidity of the face. Anæmia, indeed, forms one of the most important and invariable symptoms of renal dropsy; so much so, that many consider the alterations in the blood as the fount and origin of the disease. In other cases, the only premonitory symptoms are the leucophlegmasia, languor, and loss of spirits. After the premonitory stage has lasted for an uncertain period, the dropsy makes its appearance, usually in the eyelids, and gradually extends over the whole body, with but little, if any, alleviation of the febrile symptoms. If any indications of implication of the nervous centres existed previously, they are now very frequently alleviated, or disappear altogether; but at other times, though less commonly, they become decidedly aggravated. If the latter be the case, and the urine be lessened in quantity, unless prompt measures are used, stupor, coma, convulsions, or both the latter, may supervene, and death quickly ensue; or the case may be complicated with delirium and great restlessness, and speedily terminate in convulsions and death. These most commonly arise from the circulation of some poisonous compound in the blood,* but occasionally from the supervention of inflammatory disease of the brain or its meninges. From the same cause—viz., circulation of urea, or of a compound formed by its transformation, or of some other constituent of the urine, there is a great tendency to inflammatory disease of the serous membranes, and especially of the pericardium.

When inflammation occurs, the symptoms are at first extremely slight, consisting in an increased rapidity of pulse, anxiety, slight pain on pressing or percussing the præcordial region, and the presence of a friction-sound. I have seen several cases of this kind, which, if not detected early, speedily induce a fatal termination. The same character, insidiousness, belongs to all inflammations of the serous membranes, when they occur in combination with renal dropsy. Pain is but little complained of, and it is only by a close examination of the patient's aspect, pulse, and rate of breathing, as well as by frequent stethoscopic examination, even although we do not suspect cardiac or pulmonary disease, that it can be detected at an early period. The symptoms attending pleuritic inflammation are similar to those described as pathognomonic of pericarditis, except that the breathing is quicker, and cough is frequently present. Many other causes of death have been already pointed out,† but, as space forbids a consideration of their symptoms, I will merely mention that, besides the diseases there enumerated, laryngismus stridulus, œdema of the larynx, and œdema of the lungs, have each caused death in my practice.

Should the disease progress favourably, the general health improves, the thirst diminishes, the countenance recovers its healthy aspect, and the urine increases, contains less albumen and other abnormities, more urea and other normal constituents, becomes of higher specific gravity, and

* See Uræmia, Art. I. p. 24.

† Art. I. pp. 241, 242.

gradually assumes its normal pale-sherry tint. On the other hand, when the disease takes an unfavourable course, symptoms of one or other of the previously enumerated complications supervene, and death ensues; or the acute stage may degenerate into the chronic, when the urine becomes of low specific gravity, of a pale greenish tint, and contains far less saline and organic compounds than in health.

Before concluding this section of our subject, I will again draw attention to the rapidity with which organic diseases of the parenchymatous organs, or of their serous coverings, set in; to the insidiousness of their course, and to the great danger attending their presence. As an illustration, I may mention a case of endocarditis (which was fatal in less than three days), in which the only early symptoms were cough, a greatly increased rapidity of breathing, and a very slightly accelerated pulse, without any abnormal pulmonary or cardiac sounds being detected, although a careful stethoscopic examination was made. On the evening of the second day, a marked irregular action of the heart came on, and a mitral regurgitant and direct aortic bruit were very audible. In twenty-six hours after their detection the patient died, having walked about a quarter of a mile to my house on the evening of the second day.

Diagnosis.—The only positive evidence we can have of the case being one of *scarlatinal* dropsy, is proof of the patient having been exposed to, or having suffered from, the scarlatinal virus; but in many cases of inflammatory dropsy it is impossible to assign any special cause. The peculiar desquamation of the skin after an attack of scarlet fever, even in those cases in which no rash has made its appearance, will often of itself afford satisfactory evidence of the nature of the attack. But it is sometimes far more difficult to diagnose the disease in its first stage before dropsy makes its appearance, and when the history leads us away from the supposition of the patient having suffered from the fever. In one case of spasmodic contractions of the abdominal muscles, simulating tetanus, the diagnosis was made out only by the peculiar state of the skin exciting suspicion, and leading to inquiry about the renal functions; and the same occurred in another case of uræmic poisoning, in which the patient (a girl of 15 years) was seized, as the parents stated, with convulsions like epilepsy and coma, without having suffered from any previous illness. In both these patients there was suppression of urine. The *diagnosis* of this latter class of cases from convulsions produced by other causes, meningitis, epilepsy, apoplexy, typhus, and narcotic poisoning, must be based on—(a) the previous history; (b) the state of the skin; (c) the presence of ammonia in the expired air (Frerichs); (d) a careful inquiry into the state of the urinary secretion, including chemical and microscopic examinations; (e) and a careful comparison of the symptoms present with those of the suspected diseases.

The detection of albumen alone is not a sufficient foundation for making a certain diagnosis, as there are several medicinal agents, and other ingesta, which, in certain conditions of the system, or when taken for some time, will induce albuminous urine. Albumen has also been detected in the urine during the course of several febrile inflammatory diseases; but there is no other febrile disease which will cause its excretion after the inflammatory stage has completely passed. Nor is the detection

of renal epithelial cells alone a more certain guide, as the long-continued exhibition of diuretics will lead to their presence in the urine. And we should expect this would happen, as we know that any agent which excites an abnormal activity of the renal secretory cells diminishes the duration of their existence and induces desquamation before their full growth is attained. The only certain guides as regards the urine are the presence of albumen and fibrinous casts, or of the combined presence of the following other departures from a normal state, viz., the presence of renal epithelium, a diminution of its solid constituents, especially of urea, and a diminished quantity and altered colour of the urine.

Prognosis.—The prognosis will materially depend on the presence or absence of organic disease. If inflammatory disease of any organ exist, the prognosis will be unfavourable in proportion to the importance of the organ affected, and the extent and severity of the disease. The organic disease most fatal in my practice has been meningitis, and the next, endo-pericarditis (for the fatality of complications, see former article, pp. 241-2). If there be no visceral disease, the state of the urine affords the most definite information. To arrive at a sure prognosis from the urine, we should examine it at certain regular intervals of time, and compare each result obtained with those noted at the previous examinations, when, if we find its quantity, specific gravity, and the amount of saline constituents to increase daily, the albumen, fibrinous casts, and blood corpuscles to diminish, and its smoky or dark colour and opalescent tint to gradually change into its normal pale-sherry colour, our prognosis should be favourable. On the other hand, if its quantity, the total amount of saline constituents excreted, and its specific gravity be diminished; or, at any rate, if the total amount of solid constituents in the twenty-four hours be less; if the fibrinous casts, albumen, and blood corpuscles be increased; if the smoky colour and opalescent tint become more marked, we may be sure that our patient is getting worse. There is another condition of urine which is indicative of a greater amount of renal disorganization even than the last, viz., when the urine gradually becomes clearer, lighter in colour, less albuminous, and contains also less blood-corpuscles and fibrinous casts, but, at the same time, its specific gravity diminishes, and the amount of saline compounds contained in the urine passed during the twenty-four hours slowly but regularly decreases. After a short time this kind of urine assumes a peculiar greenish tint, froths much when shaken, and contains a variable, but usually a small, quantity of albumen. We must be extremely cautious in giving a favourable prognosis in these cases, as they frequently progress to an advanced stage of renal degeneration. The continued leuco-phlegmatic appearance of the patient would lead us in these cases to suspect this termination, even without an examination of the urine.

Treatment.—Few authors perfectly agree in treatment to be adopted in scarlatinal dropsy, some advising a strictly antiphlogistic plan in all cases, others, tonic astringent remedies even from the first; and, as usual, the truth seems to lie in the mean between the two extremes.

Prophylactic Treatment.—As the dropsy usually follows the milder forms of the fever, there is but little doubt that proper treatment, medical and dietetic, subsequent to the eruptive period, would prevent many

attacks, as doubtless almost the majority result from improper management. It is, therefore, a matter of considerable moment to powerfully impress on the parents the necessity of paying great attention to the functions of the skin, and to diet, and also of guarding against atmospheric vicissitudes. The most important matter to be attended to after an attack of scarlet fever, is to encourage the proper action of the skin. For we know that the skin and kidneys are, to a certain extent, vicarious in their action, each excreting saline particles, organic compounds, and water. A warm bath, containing, according to its size, one, two, or three ounces of common washing soda, or carbonate of potash, should be used twice a week, followed by a good rubbing with a coarse towel; or the patient may be well rubbed all over with yellow soap before using the bath. A bland nutritious diet, chiefly composed of farinaceous food, eggs, and milk, with occasionally a little broth, should be taken, so that the kidneys may have little else to excrete than the compounds resulting from the disintegration (oxidation) of the ultimate elements of the body. I know that in making this observation I am invading disputed ground, but the observations of Lehmann and others, show that the nitrogenized elements of the urine are in a near proportion to the quantity of nitrogenized food taken. There is certainly, chemically, some difficulty in the way, but not enough to overturn this view, which is supported by most recent observers, including Frerichs. It is also probable, as supposed by Prout, that if imperfectly formed albuminous compounds are absorbed into the circulation, they are excreted in the form of lithate of ammonia. Stimulating drinks of all kinds are also to be strictly avoided, and especially spirits. An occasional aperient, and a sudorific at night twice a week, will also be found advantageous. But if, in addition to all these precautions, we do not protect our patient from draughts of cold air, and from the vicissitudes of our variable climate, all our care will be lost. As proof of this statement, I refer to tables 4 and 5, where we see that the proportionate and absolute mortality from the disease is least in that month of the year, August, which presents less atmospheric changes than any other, and that it rises in each of the following months, when the changes become greater. It is, therefore, evident that, in changeable and cold weather, confinement to one room kept at an equable temperature is necessary, and also that, whatever the weather may be, warm clothing, and especially the use of flannel next the skin, are indispensable to the success of prophylactic treatment. These precautions should be rigorously enforced during the first three weeks of convalescence, or certainly after the fading of the rash, and may then be less carefully observed during the fourth week, and afterwards abandoned. That the periods here assigned are not too long, may be seen by examining table 15, which shows that 47·6 of all the attacks happened during the first fortnight, 82·3 per cent. during the first three weeks, and 93·8 per cent. during the first month.

The treatment of the non-albuminous variety of the dropsy is very simple, our chief indications being to restore the tone of the system, increase the red corpuscles of the blood, and remove the excess of water. To effect these, a combination of slight diuretics with the tincture of the sesquichloride of iron, and the occasional exhibition of a hydragogue purgative, have proved very effectual. My favourite combination con-

sists of tincture of digitalis or vinegar of squills, spirits of nitre, and tincture of sesquichloride of iron; and a dose of compound jalap powder every other or every third morning.

The treatment of the albuminous variety requires much greater care and consideration, and varies greatly in the febrile and post-febrile stage, and also according to the intensity of the former. Before deciding on any plan of treatment, we must carefully investigate the condition of the urine, as it is our best and most certain guide. In practice, I divide the febrile cases into three classes: (a) into those in which the urine is scanty and bloody; (b) into those in which the urine is scanty, but not bloody; and (c) into those in which the quantity of urine is but little diminished, and there is no blood present. The first variety (a) is usually of the most acute kind, and is indicative of considerable disease in the secreting part of the kidneys. The treatment required here is at first decidedly antiphlogistic, both as regards diet and medical appliances. Our first step will consist in the abstraction of blood from the loins by cupping or leeching, to an amount, varying according to age, of from two to six ounces, remembering that our object is not to cure the disease by bleeding, for we cannot effect this, but to relieve the present extreme congestion, and prevent the occurrence of further mischief. After having abstracted a little blood, our next indications are to restore (1) the functions of the skin, and (2) the normal state of the circulating fluid; (3) to relieve still further the congestion of the kidneys; (4) to afford tone to the distended renal vessels; (5) to remove the dropsical effusion; and lastly (6), to adopt such other measures as the peculiar state of our patient, any idiosyncrasy of constitution, or the presence of complications may require.

The first indication—viz., *to restore the functions of the skin*, must be effected by keeping the patient in bed, or confined to one room; in preventing him from feeling the changes of weather; and in the exhibition of antimonial sudorifics. Saline sudorifics must be carefully avoided. A warm bath, containing common soda or potash, followed by long-continued friction with a coarse towel, should be frequently used. This plan of treatment is of very great service in all stages of the disease, and especially when uræmic symptoms are impending or actually present, as they will sometimes induce free perspiration, and thus relieve the blood of part of its superfluous fluid, and of the urea and other salts which should be excreted by the kidney. Another powerful remedy consists in wrapping the patient in a sheet dipped in warm or cold water, and then covering him with blankets, carefully watching, if cold water be used, that faintness do not happen. I would not recommend the cold sheet in any but extreme cases, for fear of inducing congestion or inflammation of the lungs, and lest the vital powers should not be sufficiently strong to induce reaction.* In addition to these measures, warm clothing should be adopted, and especially flannel next the skin.

* A friend of mine, Mr. E. May, related to me a most successful case of "cold packing." The patient had been under his care, and under that of another medical practitioner previously, for some time, with symptoms of effusion into all the serous cavities, dilated pupils, and coma. All the ordinary remedies were tried, including diuretics, and without effect, as, except with a motion, no urine was passed, even if twenty-four or thirty-six hours elapsed. The child was wrapped in the cold sheet, and then covered with blankets, which induced in ten minutes most profuse perspiration, and was followed by slow but steady recovery.

The next indication, *to restore the purity of the circulating fluid*, is most important; and to effect this, we have to eliminate the urea and other organic compounds which have accumulated from the diminished action of the kidneys and skin, and also to restore the proper proportion of the red corpuscles and albumen. To eliminate the foreign matters, we must restore the functions of the skin and kidney, and set up increased action of the intestinal glands, by which some of the excrementitial compounds of the body are normally excreted. The object of purgatives in this form of dropsy is not merely to drain off the accumulation of water, but also to remove these compounds from the blood; and we must, therefore, not merely use hydragogues, but occasionally rhubarb, jalap, aloes, or other purgatives which will produce the effect desired. A most useful medicine is the compound jalap powder, given daily in full doses, with the substitution once a week of a dose of calomel and rhubarb, the proportion of the former being very small. We must remember that the intestinal glands excrete compounds which are chiefly, if not entirely, un-nitrogenous, so that they cannot take on an action vicarious with that of the kidneys. To restore the functions of the kidneys is often by no means easy of attainment, and the treatment must vary according to the amount of renal disease existing. When the urine is bloody, after the local abstraction of a little blood, we have to attend to the two next indications—viz., to relieve still further the congestion of the kidneys, and to afford tone to the distended renal vessels. The most effectual way to fulfil the first of these is to act powerfully on the skin by the means previously enumerated; to apply dry cupping, stimulating embrocations, and other counter-irritants to the loins. The use of epithems of turpentine every other day, or of embrocations containing turpentine, is often attended with very beneficial results; mustard poultices are sometimes useful. The application of blisters as counter-irritants must be carefully avoided, unless required for some inflammatory disease of the viscera or their coverings. This remark applies with increased force to setons or issues.

The medicines which we exhibit should be selected to fulfil both the indications here pointed out, and to produce a tonic or constringing effect on the distended renal vessels. In the early stage of the disease, the plan which I have found most effectual has been, to exhibit the tincture of the sesquichloride of iron in full doses, in combination with a few minims of tincture of digitalis, or by itself; the digitalis being added in those cases only where there is an increased rapidity of pulse. When blood has ceased to be passed, and the other abnormalities have diminished, should the patient not progress sufficiently rapidly, the exhibition of tannic acid in full doses is eminently useful. I have also, in an advanced stage, found much benefit from the free use of alum, with or without cubebs. Some have recommended gallic acid in preference to tannic; but Frerichs and others prefer the tannic. The object of these astringents is not merely to prevent the exudation of albumen, but to relieve the congestion of the gland, by producing tonic contraction of the capillary vessels. The great reason for preferring the tincture of the sesquichloride of iron to any other astringent, is because it assists most materially in restoring the blood to its normal condition. Thus it has

been shown, that the blood is always more or less anæmic in these cases; the proportion of the red-corpuscles being very much diminished; and while, therefore, we are relieving the blood of its accumulated effete particles, and of its excess of water, we should attempt also to increase the number of blood-discs. A compound of tannic acid and lactate, or ammonio-citrate, or potassio-tartrate of iron, is very valuable, as, when combined with the use of bitartrate of potash, or some other hydragogue, given at proper intervals, it fulfils all our indications. We must in all cases remember the necessity of exhibiting iron in scarlatinal dropsy, as one or other of its preparations are almost invariably required. The diet should be bland and nutritious, care being taken that, whilst we support our patient, we do not overload the stomach, and should therefore recommend eggs, milk, farinaceous food, with a little meat, and less vegetables.

Our next indication—viz., *to remove the dropsical effusion*—is one of some moment, and requires care in its fulfilment, but must not be attempted at the expense of neglecting any other indication. Purgatives, as already pointed out, are the most powerful agents in effecting this, and especially hydragogues, the type of which, and the most useful—*elaterium*—may be administered every other morning, the dose being carefully regulated according to the age of the patient. Another most effectual purgative is the compound jalap powder, given in full doses every morning, in a moderate quantity of fluid; for we must remember, in administering saline purgatives, that their action depends on the specific gravity of the dose given; for if a saline aperient be administered in a quantity of fluid sufficient to reduce the specific gravity of the dose below that of the serum of the blood, it will be absorbed into the blood, and not act as a purgative.

The period at which diuretics should be commenced is a point somewhat in dispute; but it is quite certain, if they are administered too soon, that they will induce bad consequences, by adding to the diseased action. A consideration of the action of diuretics is too large a subject to treat of here, but it is pretty well established that many diuretics, whilst they induce an increased flow of water, diminish the quantity of the saline constituents passed in a given time, and might, therefore, very well be termed hydragogue diuretics. As proof of this, we may refer to Professor Krahmer and Dr. Golding Bird, who state the quantity of water and solid constituents passed in a corresponding period of time, previously to the exhibition of diuretics, and whilst the patients were under their influence. Also to the experiments of Dr. Parkes* with liquor potassæ, which were performed partly on himself and partly on some of his hospital patients, as well as to the researches of others, which show that the albuminous compounds of the blood are acted on by it.†

A careful consideration of the facts adduced by these and other authors lead to the following conclusions, viz., that juniper, squills, and turpentine, if not all the vegetable diuretics, are hydragogues, and, therefore, suited for removing dropsical collections, unless we wish, at the same time, to induce an increased excretion of the saline constituents of the urine. If, however, we have any reason to suppose that the blood is charged with the

* British and Foreign Medico-Chirurgical Review, vols. xi. & xiii.

† See an admirable summary in Carpenter's Physiology, fourth edition.

nitrogenized compounds which result from the disintegration of the fluids or solids of the body, we must then combine them with one or other of the alkalies or their compounds, strictly avoiding the exhibition of liquor potassæ on account of its action on the albumen of the blood. But in our use of diuretics, we must be chiefly guided, not by the amount of the dropsical effusion, but by the state of the kidneys, for if these glands are the seat of much congestion, whether active or passive, diuretics *per se* will prove injurious, by affording increased stimulation to an already over-stimulated organ. The presence of blood in the urine is, as a rule, a direct contra-indication to the use of saline diuretics, and, in my opinion, of any, except digitalis, colchicum, or spirits of nitre, either of which, combined with tincture of the sesquichloride of iron, or tannic acid, is useful. When the urine ceases to contain blood, and the quantity of epithelial casts and scales is less, saline diuretics are very useful, and in some cases, especially the preparations of potash in combination with squills, guaiacum, colchicum, or broom. If the disease should degenerate into the chronic form, the free use of Vichy, Seltzer, or other similar water, is often beneficial, the former especially, for they seem to wash, as it were, the fibrinous casts out of the uriniferous tubules. Those waters which contain alkalies also act on the effete particles contained in the blood.

Having bestowed so much space in considering the general principles of treatment to be followed, it will be unnecessary to point out every variation required in the different forms of the disease. The more acute the disease, the more energetic must be our treatment, care being taken lest the *nimia diligentia medici* be not exercised injuriously. We should not think of using blood-letting in any but those cases in which the urine is very scanty and bloody, or unless imminent symptoms of uræmia were present, with scanty urine; and it would, therefore, be inadmissible in those cases of dropsy included in varieties (b) and (c). The former (b) may be treated by alkaline warm baths, and subsequent friction of the skin, sudorifics, preparations of iron, or of some other renal astringent, with or without vegetable diuretics (excluding squills in the early stage), hydragogue purgatives, terebinthinate applications to the loins, a carefully-regulated diet, with protection from atmospheric vicissitudes. In variety (c), diuretics can be used earlier and more safely than in the other forms; baths are not frequently required; and the exhibition of tincture of the sesquichloride of iron, with hydragogue purgatives twice or three times a week, will usually induce a satisfactory result in a short time.

The treatment of the *complications* is most important, and varies considerably from that which we should adopt if they existed independently of the dropsy. The frequent occurrence of inflammations of the serous membranes in cases of renal dropsy was particularly pointed out by Christison, who also noticed that the renal disease exercised a considerable modifying power on the action of some medicines, and especially on that of mercury. Under the head of *Causes of Death*, we have shown that all the complications may be included under two heads, the accidental and pathofusical;* and we shall adopt the same division in considering the subject of treatment, premising a few observations on the modifying influence of the renal disease on our remedies.

* See Art. I. p. 241, note.

Blood-letting, as a rule, must be small in quantity, and as much as possible avoided, for in these cases the normal proportions of the blood-corpuscles are materially diminished. As pointed out, this has been shown by chemical analysis, and by the peculiar leuco-phlegmasia of our patients. To remove blood unnecessarily would, therefore, exert a most prejudicial influence. General blood-letting should, therefore, be used sparingly, both as to amount and frequency; and local blood-letting, either by cupping or leeches, is decidedly preferable, care being taken not to allow the draining from the leech-bites to go on unnoticed and unwatched, as we might thus abstract more blood than we wished. We must remember that we cannot remove, although we may reduce, the amount of the *materies morbi* by blood-letting, and must, therefore, to effect this, trust chiefly to other evacuants. The use, however, of blood-letting in serous or parenchymatous inflammations, is undoubtedly beneficial, and in those of the pulmonary or circulatory organs often relieves the extreme oppressions of their functions; but, so far as my experience goes, it is not of equal service in cranial affections. Should we meet with inflammation of any organ, and an almost total cessation of the renal functions, I would prefer abstracting the blood from the lumbar regions rather than from the locality of the inflammatory disease. The object sought, as regards the kidneys, by blood-letting, seems to me rather to be that of relieving congestion, than of stopping inflammatory disease.

The exhibition of mercury, even as a purgative, must be very sparing and careful, as this remedy often produces its peculiar effect (salivation) from very small doses, and in a very short time. I have seen profuse salivation result from one-grain doses of calomel given at intervals of four hours, and even from a single dose given as a purgative. In 1842-3 I witnessed several cases, in hospital practice, of most profuse salivation from moderate doses of mercury in pericarditis complicating Bright's disease. And not only is the use of mercury open to this objection, but it does not so certainly or readily produce its ordinary effect of inducing the absorption of the effused fibrin,* or of staying its further deposit. I always treat inflammations of the parenchymatous organs, or their serous coverings, with mercury, but am very careful as to the dose and frequency of exhibition, and would advise the case to be visited at least twice a-day, and the remedy to be discontinued, at any rate for a time, directly the red line shows itself on the edge of the gums. I have also seen pericarditis ensue, in several cases, after the patient had been salivated for other diseases.

We must also be very careful in the use of blisters, especially in children, as frightful sloughing sometimes follows their use. I saw one case of most extensive sloughing of the thoracic parietes, which resulted in death, follow the injudicious application of a blister. I say injudicious, because it was kept on too long; indeed, in children, we should never keep them on in any disease, and especially in this, until they produce vesication.

* In thus expressing myself I must not be supposed to express a belief that fibrin once poured out of the vessels and coagulated can be reabsorbed without undergoing any change; for recent researches prove that in many, if not in all, cases, it is first converted into a kind of fat, with the evolution of ammonia, and that the two then combine to form a kind of soap, which is dissolved in the effused serum, and then absorbed.

The *diet* should also be less strictly antiphlogistic than in ordinary disease, as the red-corpuscles of the blood suffer such extensive diminution, and the powers of the system are so much reduced. Indeed, it is a question, in some cases, how far this alteration in the blood is to be looked on as one of the causes of the inflammatory attack.

Without treating specifically of the action of any other remedies, I will conclude these preliminary considerations by again observing, that in curing the complications we do not remove the *fons et origo mali*, and do not prevent the occurrence of similar complications in other organs.

Treatment of Cranial Disease.—We must be very careful not to treat any case of cerebral disease as inflammatory until, by careful examination of the symptoms, we have assured ourselves that the symptoms are not those of uræmia. Our analysis of the causes of death in 128 cases shows that 6 were from meningitis, and 7 from cerebral effusion. We must, therefore, if possible, distinguish between uræmia, cerebral effusion, and meningitis: I say if possible, because the pathognomonic symptoms are often very obscure in these cases, especially in those of meningitis; still, with care, they can be separated. Having satisfied ourselves that the case is one of meningitis, we must abstract a little blood locally, apply cold to the head, blister the temples or back of the neck, exhibit mercury until it produces the red line, and should adopt the other treatment pointed out for relieving the renal disease and the abnormal state of the circulating fluid. In cases of cerebral effusion, local treatment seems of little avail, still we might try the effect of moderate leeching and of blisters. The treatment of uræmia will be considered after that of the inflammatory complications.

Disease of the Larynx is rare, but very fatal. I have lost several cases from (a) laryngitis, (b) laryngitis with effusion of lymph, and (c) œdema of the glottis. The treatment of the former should be by antimony, in free doses, combined with opium or syrup of poppies, local blood-letting, blistering, and, perhaps, mercury: of the second (b), by more free and *early* depletion, mercury, blisters, and the exhibition of sulphate of copper in emetic doses every hour and a half or two hours; mercury seems to exercise little influence here, as it is not speedy enough in its action. Blistering by caustic ammonia or boiling water may be adopted, and also painting the back of the fauces, the glottis, and larynx, with a strong solution of nitrate of silver (ʒij. ad ʒj.), is in this, as well as in œdema of the glottis, a most useful application. Œdema of the glottis must be treated by the nitrate and by scarification, conjoined with tonics and stimulants.

Disease of the Lungs and Pleuræ.—There is often much discrimination required for the early detection of thoracic inflammations, and yet it is a most important matter, as no less than 40 of the 128 cases were fatal from thoracic disease, and of these 40, 75 per cent., or nearly 22 per cent. of the 128 cases, were fatal from inflammatory disease of the heart or lungs. The principles laid down for the treatment of inflammatory complications apply forcibly here, as the exhibition of tartar emetic, in full doses, is not nearly so useful as in ordinary pneumonia. The most successful plan I have found to consist in the careful exhibition of small doses of mercury with ipecacuanha, followed, if hepatization should ensue, by small doses of

iodide of potassium, combined with opium and ipecacuanha. Opium, or syrup of poppies, is also very valuable. The treatment of pleuritis should be very similar; and in the advanced stages of either—that is to say, after the acute stage has passed—the judicious application of blisters is often attended with very good results.

Heart and Pericardium.—Diseases of the heart and its covering are very fatal when they occur, and are very insidious. We may treat pericarditis by local depletions, calomel with ipecacuanha, or antimony, preferring the former if there be much constitutional depression; followed speedily by a large blister to the præcardial region. I have been successful in two cases, and unsuccessful in one, which was otherwise complicated. Endocarditis may be treated by calomel and blisters. Dropsical effusion into the pericardium is sometimes excessive, and it becomes a question whether, instead of trusting to hydragogue purgatives and other general measures to drain off the fluid, we should tap the pericardium. I have never seen this done, but think with care that it may be readily performed. We must also be careful not to depress the system too much by whatever remedies we use, as the symptoms of pericarditis may continue after all necessity for active treatment has passed by, and when the exhibition of wine, rather than of antiphlogistics, is required.

The Treatment of Diseases of the Abdominal Organs and the Peritoneum varies according to the organ affected; that of peritonitis should be similar to that just indicated for pericarditis. Chronic diarrhœa, as it usually depends on ulceration of the intestines, must be treated accordingly, by the administration of sulphate of copper, in as large doses as the stomach will bear, either by itself, or combined with small doses of ipecacuanha, or of the tincture of the sesquichloride of iron. A large blister should also be applied to the abdomen, and repeated, if necessary. Should these not be successful, the other ordinary treatment for ulceration of the intestines must be adopted. Cases of ascites without inflammatory disease, which are rare, require only the ordinary treatment for dropsy, with counter-irritant and absorbent applications to the abdomen, such as compound tincture or ointment of iodine.

The other complications, gangrene, sloughing, abscesses, erysipelas, &c., will require the treatment ordinarily adopted in such cases.

The Treatment of Uræmic Poisoning must materially depend on the urgency of the symptoms, as in some cases danger is imminent. Thus, when we have to treat a patient in a comatose state, or in convulsions, with total, or nearly total, suppression of urine, if we lose time, we lose our patient. Warm baths, or packing in a sheet previously dipped into cold or warm water, and then enveloping our patient in a blanket, are measures which must be immediately resorted to. Hydragogue purgatives, cupping or leeching the loins, and, in some cases, diuretics, are most useful. We may also employ frictions of the lumbar and abdominal regions, with terebinthinate embrocations, or turpentine stupes. Frerichs recommends the use of dilute hydrochloric acid to neutralize the ammonia which he believes to be circulating in the blood; and states his belief that the acid passes into the blood, combines directly with the alkali, and is then excreted by the urine; and he also recommends the patient to be washed with vinegar. The question of depletion and counter-irritation

in these cases is often attended with doubt, and requires some care in making our decision. If we have reason to believe that the symptoms arise from congestion of the brain, we should abstract blood from the temples or scalp, and apply frigorific lotions and blisters; but as post-mortem examinations do not ordinarily reveal any organic cerebral changes, or marked congestion, we are not, as a rule, justified in adopting such measures, and should rather trust to our other remedies. Slight cases do not require active treatment. A combination of citrate of iron and tincture of digitalis or squills, occasional baths, terebinthinate embrocations used daily, and occasional purgatives, will suffice.

I cannot conclude this brief outline of the treatment to be adopted in uræmia without observing that the old adage, "prevention is better than a cure," applies most forcibly here. For the history of uræmia leads to the conclusion that proper and effectual measures for promoting the functions of the skin, with due attention to ventilation and diet, would in most cases prevent its supervention. And these prophylactic measures should be adopted not merely to prevent the severer effects of uræmic poisoning, but the slighter—viz., mal-aise, pseudo-neuralgic pains, headache, and other anomalous aches and pains which are frequently referred to all but the right cause. Indeed, I have no doubt that an extended and careful investigation of the symptoms and treatment of uræmia, not only as a sequela of scarlet fever, but in its relation with other diseases, will clear up many an obscure point in pathology.

ART. IV.

*A Nosological Inquiry into the Acute Diseases of the Chest known to the Greeks—viz., the Pleuritis and the Peripneumonia; with Remarks upon the Peripneumonia Notha of the Moderns.** By W. T. GAIRDNER, M.D., Lecturer on the Practice of Medicine in Edinburgh.

THE very important position assigned, in all ancient nosological descriptions, to the disease variously called *pleuritis*, ἡ πλευρίτις (νόσος), τὸ νόσημα τὰς πλευρᾶς, ἡ πόνος τοῦ πλευροῦ, by the Greeks, and *morbus lateralis* by the Romans, shows that this affection (*the disease or pain of the side*) is by no means one of those which has been left for modern ingenuity to discover or appreciate. Nor does it seem possible to doubt, that the ancient *pleurisy* had, to say the least, as wide a range, and as great a fatality, as the modern. Moreover, the conformity of most of the early descriptions with each other, and with facts which any one may now observe, as well as the numerous speculations as to the nature and seat of the disease, show that great care had been bestowed upon its diagnosis, and that no little discussion had taken place as to its more recondite anatomical characters. And it is likewise a most remarkable fact, of which we have ample evidence, that throughout an epoch of history when the "right of private judgment" was more freely exercised than it has, perhaps, ever been since, in matters of medical practice, and

* This paper may be considered as an appendix to the memoir on Collapse of the Lung in its Relation to Modern Practice, the first part of which was published in this journal in January, 1854.

when the general theory of medicine was perplexed by multitudes of opinions little less varied and discordant than those current in the present day, the general treatment of pleurisy maintained a steadiness and consistency that leave us, in this nineteenth century, little cause for self-gratulation. Here, then, is a case in which, if ever the experience of the past is to be useful to us, it ought to be worthy of our most careful study and conscientious appreciation.

If, now, we attempt to reconcile the ancient idea of pleuritis with that of almost any author of the present day, we shall be struck by a discrepancy, to which I have elsewhere alluded in the case of pneumonia, and which arises directly out of the ancient as compared with the modern point of view. The rational or subjective characters—the symptoms—all the attributes, in short, of pleuritis, which constitute its essence *to the sufferer*, were combined by the ancient physicians with a few well-marked phenomena of a less obtrusive kind, into a definition which, to their view, *marked the disease*. The definition, or general idea, thus established, was subject to no such exceptions or irregularities as we are now led to attribute to the disease we call pleurisy. There was, for instance, no question of diagnosis or treatment relating to *chronic* or to *latent* pleuritis. The latter had no existence in nosology before the era of Laennec; the former may have been known indeed, but known only under some other name. *Pleuritis* without pain, fever, cough (or even devoid of any one of these) would have been disowned equally by Hippocrates and Celsus, by Galen and Avicenna, by Sydenham and Huxham. Yet such a form of pleurisy is now not only recognised in science, but is specially urged upon the attention of our students as having the characters of an inflammatory disease, the detection and treatment of which, by means of physical signs, forms one of the triumphs of modern medicine. And, inasmuch as it is generally admitted that all the rational symptoms (with trifling exceptions) may be absent in the most acute pleurisy, it is plain that the tendency of modern studies is to thrust into a corner, if not to dismiss from consideration altogether, those attributes of this disease upon which, for at least twenty-one centuries, its position in the nosology, its diagnosis, its prognosis, and its relation to remedial measures, have been conceived absolutely to depend.

The importance of this consideration will not be denied; it may, however, be maintained that I exaggerate the practical difference between the ancient and modern pleuritis, by pressing an argument founded on exceptional cases. In the view of this objection, and in order to bring into view some of the more important points of difference, I propose to pursue this subject a little further.

The pleuritis of the ancients was essentially a disease of short duration. In the Hippocratic writings it is constantly described as one of the fevers, amenable to the supposed laws of crisis and critical days. In the description of the well-known case of Anaxion,* the only *clinical* history of pleuritis which can be certainly ascribed to Hippocrates himself, the disease is called an *acute fever*, and the appearance or non-appearance of critical phenomena is particularly noticed on the seventh, eleventh, seventeenth, and twentieth days. On this last day the patient was,

* Epidem. III. 35, case 8.

without fever, but continued to have "thirst, and no good pulmonary purgation" (expectoration). The final crisis took place on the thirty-fourth day. From the other authentic writings of Hippocrates, it would appear that this case, though terminating within the forty days which constituted the ordinary limit of acute fevers, must have been regarded by him as unusually protracted. In the 'Aphorisms,' v. 8, he remarks, that "in the pleuritics who are not purged (ἀνακαθαίρονται—i.e., who do not expectorate) in fourteen days, the disease is changed to an internal abscess or *empyema* (τοῦτέοισιν ἐς ἐμπύημα μεθίσταται)." And again in 'Aphorisms,' v. 15, he remarks that "those who pass from a pleurisy to a *suppuration* (ἐκ πλευρίτιδος ἔμπυοι γίνονται) get well if they are purged within forty days from the rupture (meaning, probably, the first considerable expectoration); but if they are not purged within this time, they change to *phthisis* (ἐς φθίσιν μεθίστανται)." In the 'Prognostics,' and in various parts of the less authentic Hippocratic writings, as in the treatises 'De Morbis,' the reader will find descriptions in accordance with these passages; but he will hardly apprehend the full signification of them, unless he be aware that *empyema* (an internal suppuration) is universally defined as a chronic disease by the Greeks; and that this word, besides a limited application in accordance with our present use of it, comprehends all the cases in which there is a long-continued discharge of pus, or in which there occurs a collection of matter, attended by hectic fever, in any internal organ, and especially in the lung. This disease was regarded as perfectly distinct, in its essence, from pleurisy, although sometimes grafted upon it; and, indeed, the majority of cases of pulmonary empyema (which was also called *vomica*) were more allied to phthisis, from which, however, it was likewise distinguished by most writers. All the Greek authors, moreover, agree with Hippocrates, in limiting the application of the term pleuritis to those early stages and rapid forms of disease which may be called truly acute; and which, occurring in previously healthy individuals, either ran their course within a few weeks, or underwent such changes of type as, in the opinion of these writers, converted them into different diseases. Hence an empyema was not regarded as the natural result of a pleuritis, but as a metastasis, produced (in the language of the humoral pathology) by an imperfect coction of the febrile matter, and the consequent absence or imperfection of the critical evacuation. I refrain from further quotations in proof of these statements, and will merely refer the reader to a series of passages, to some of which I shall have to allude repeatedly hereafter.*

It can scarcely be necessary to give any references to modern authorities in order to prove that the modern pleurisy does not, as respects duration, correspond in any degree with the idea formed of this disease

* Aretæus Morb. Acut. Caus. &c., Lib. I. chap. 10; Morb. Diaturn. Caus. &c., Lib. I. chap. 8, 9. Cælius Aurelianus, Morb. Acut., Lib. II. chap. 13—17 inclusive; Morb. Chronic., Lib. II. chap. 14, and Lib. V. chap. 10. Paulus Ægineta, Lib. III. § 22, 23, (he does not distinguish empyema from phthisis). Numerous references will be found in the excellent edition of Dr. Adams, to all the other authors of the classical period. I am happy to be able to strengthen the above views in regard to the empyema of the ancients, by the authority of this truly learned and able physician, to whom the present age lies under deep obligations for his great efforts on behalf of ancient medical literature. The note of Dr. Adams on the Prognostics of Hippocrates (Sydenham Society's edition, vol. i. p. 246), as well as the singular confusion of M. Littré's commentaries on this subject, had escaped me till after the above passage was written.

by the ancients. If certain cases of pleurisy follow this course, it will be generally admitted that a very large proportion of those which are fatal by effusion have a much more protracted duration.

Pain, as a symptom of pleurisy, is common to many cases of the ancient and modern disease. Moreover, the ancient and modern symptomatology agree in recognising mere pain, apart from the other symptoms, as not characteristic. The distinction of pleurisy from certain painful affections, supposed to be of a different nature, and to depend on an inflammation of the intercostal muscles, was clearly made by Cælius Aurelianus,* as well as by the later Greeks;† it seems, however, probable that this distinction acquired much greater vogue after the revival of letters, when the contrasted names, *pleuritis vera*, and *pleuritis notha vel spuria*, became established.

The chief diagnostic signs referred to by Galen (*see note*) as distinguishing the real pleurisy from the muscular affection, are the presence of cough, even where there is no expectoration; the hardness and tension of the pulse; the severity of the fever and dyspnœa; also the absence of tenderness on pressure, which he gives as a character of the muscular pains. It is sufficiently evident, however, that Galen must himself have sometimes mistaken the true pleurisy (speaking *anatomically*) for the muscular affection. He says of the latter that “when the phlegmon is concocted, unless the pus is discussed, it comes to the surface, and is opened” (*loc. cit.*); an assertion which sufficiently indicates that in all probability some, at least, of these pains arose from pleuritic effusions, limited, perhaps, to one portion of the pleural cavity. Many of the systematic authors of the sixteenth century recognise very distinctly the *pleuritis notha*, which from that period has, under one name or other, a distinct place in all nosologies.‡

* *Acut. Morb.*, II. 17, *initio*.

† Galen, *De Locis Affectis*, Lib. V. cap. 3. See also Paulus Ægineta, III. 33; and Aetius *Tetrabibl.* II. Serm. IV. chap. 28—“Of the disease which is apt to be mistaken for pleuritis.” The disease thus designated is probably one of the originals of the more modern *pleuritis notha*. How Dr. Badham could construe this into a description of acute bronchitis is to me inexplicable. It was manifestly an acutely painful disease, closely simulating pleuritic inflammation, and produced, according to the author, by crude humours arising from the food. There is no mention of its being connected with catarrh, unless an allusion to the passage of the humours to the pleura and lung be so interpreted; but such an interpretation is unnecessary, and is certainly not in accordance with the general scope of the description, which, however, I admit, it is difficult to identify. I shall have more to say hereafter as to the ancient prototypes of the modern bronchitis. Oribasius, Trallian, Actuarius, Scribonius Largus, and Psellus (this last I know only through a reference by Dr. Adams in *Paul. Ægin.*, vol. i. p. 499), all refer, more or less distinctly, to pains of the side which are not pleurisy. I presume they copy after Galen, as do the Arabians. Van Swieten (*Aph.* 875) finds some slight hints of the same kind in Hippocrates; they are not very definite, and are extremely brief. Celsus also (*IV.* 6) gives merely a hint as to the existence of non-pleuritic pains.

‡ There are also some descriptions of isolated cases,—e.g., Forestus *Obs. et Curat. Med.*, Lib. XVI. *Obs.* 42, 43—“*De pleuritide non exquisitâ, sed mendosâ et spuria dictâ.*” But all the narratives and general descriptions are very much after Galen. Rondoletius, however, distinguishes himself by confounding the treatment while recognising the symptomatic distinctions, (*pleuritis vera et non vera*). Nicolaus Piso (*De Cogn. et Cur. Morbis*, II. 8,) has it *pleuritis notha*; Plater (*Praxis Med.* III. 10), *pleuritis falsa*; Lommius (whose elegant little synopsis contains the very marrow of the Greeks) describes it once more (*Med. Obs.*, Lib. II.) as *pleuritis notha*; in Sydenham it disappears altogether, to reappear in the often-quoted but truly unprofitable work of Verna, (*Princeps Morborum Acutorum Pleuritis*, chap. 1, 17,) first under the old designation, and then under a new one, which may be given in the author's own words:—“*Pleuritis aut propria; aut minus propria, aut impropria. Propria solius pleuræ inflammatio est. Minus propria est vera thoracis partium, aut exterarum inflammatio . . .*;

But if pain be a symptom common in most cases to the ancient and modern pleurisy, it is by no means of the same significance in the two forms of disease. In the pleuritis vera it would not be difficult to show, by the concurrent testimony of practical writers, that pain and fever in conjunction were the really *guiding* symptoms by which the intensity of the disease was in a great degree measured and the practice determined. Thus the practice of bloodletting was reserved, in the early times of the art, for the cases of extreme pain, or of pain not relieved by other means. The persistence of pain after expectoration, and after remedial measures, was looked upon as most unfavourable;* and notwithstanding the consideration which the favourable or unfavourable character of the expectoration received from Hippocrates, we find him submitting all these grounds of prognosis to one very simple test—viz., “All expectoration which fails to remove pain is bad; that which removes it, on the contrary, is best of all.”† And so with other authors the pain is, to a great extent, the essential feature of the disease; and its removal is at once the indispensable condition, and one of the most important evidences of approaching amendment. It can scarcely be necessary to point out in detail how much all this is removed from the practice of the moderns, to whom pain has become a symptom of very secondary consideration. “The greater part of the symptoms which are supposed to be distinctive of pleurisy,” says Dr. Williams, “depend on a much exalted sensibility of the pleura, which is *by no means a necessary accompaniment of its inflammation*. . . . On the other hand, the physical signs in the greater number of cases are very unequivocal; and although they by no means speak of the degree or extent of the inflammation, they seldom fail to announce its presence, and they pretty accurately measure *its most serious concomitant, the effusion*.”‡ Statements of this kind from other authors of equal repute will occur to every one.

I shall not dwell in detail upon the other, and, as the ancients would consider them, the subordinate symptoms of pleuritis. It is well-known to those who have given any consideration to the Greek medicine, how much attention was paid to every kind of uneasy sensation and abnormal evacuation; the cough, the dyspnœa, the febrile heat, the thirst, the occasional restlessness and delirium, the unfavourable characters of the urine and alvine discharges, as occurring in pleuritis, had therefore undergone, by the excellent observers of that epoch, a very full examination and discussion. The eminently considerate and practical spirit of these observers is nowhere more clearly shown than in their appreciation of the value of each of these symptoms, and of the greatly increased significance which they acquired when in combination. The idea of pleuritis included nearly the whole of them as essential parts of its definition: and nothing was further from the spirit of the Hippocratic medicine than the exclusive leaning upon a single pathognomonic sign. I have already noticed that the only personal narrative of pleurisy left

impropria tandem est, in quâ revera nullius partis pectoris, aut cæterorum ventrium inflammatio adsit,” &c. This division, he says, he uses “*brevitatis causâ, et claritate*” (sic); yet all his arguments for the localization of these affections are *in nubibus*! In the Nosology of Sauvages, and in most works since his time, the *pleuritis notha* appears as *pleurodyne*, or *pleurodynia*.

* Hippocrat. Coac. Prenot. 394.

† Idem, 391; and in the Prognostics.

‡ Diseases of the Chest, third edition, p. 106.

us by Hippocrates does not bear any inscription to distinguish it as such; for it is everywhere apparent that this great man, with a truly lofty view of his art, preferred real knowledge to verbal distinctions, and the unwearied study of individual cases of disease to arbitrary refinements in their nomenclature.* Aretæus also takes broad and clear views of the disease under consideration, resting its distinction not on any single symptom, but on a combination of phenomena, which, he remarks, "must harmonize and conspire together, and must arise from a single (exciting) cause; for if they arise at random from a variety of causes, even if they happen to appear at once, the disease is not called *pleuritis*."† The whole ancient practice which has come down to us, and all the better part also of the modern practice in pleurisy, is regulated by similar principles; and it is most important to keep in view that the heroic remedies in thoracic inflammations, whose use and abuse have been the subject of so much controversy, were never applied by the Greeks to anything short of *an acute disease, suddenly arising in a previously healthy subject, and accompanied by great functional oppression, with extreme distress and fever*. If we would always keep this fact steadily in view, we should be saved from many extravagancies which beset the medicine of the present day. We cling to the therapeutical traditions of the past, and rely on an assumed universal experience, while we neglect the studies necessary to make our practice consistent with that of our predecessors, and strike blindly, with their heaviest weapons, at enemies whose very existence was scarcely known to them. In improving diagnosis, we have made a chaos of nosology. What wonder if rash innovators and pedantic obstructives, misled by names, fall into an equally vicious routine? If, on the one hand, the wise are scandalized, and the simple perverted, by practices for which the authority of all antiquity is claimed, but at which common sense revolts? If, on the other, the charlatan and the sceptic simultaneously arrive at the idea, that infinitesimals and extract of grass are better and safer remedies for acute diseases than all that have descended to us from our fathers? The reader of the preceding pages, and of the illustrations of modern practice which I have discussed elsewhere in reference to *pneumonia*, will, I trust, have little difficulty in detecting the misapprehensions which lie at the root of such vagaries.‡ They have a place in the history of opinion, and will pass into the limbo of its vanities, though not without a useful result, if they shall lead us, by their very extravagance, to consolidate more care-

* For his reproach of the Cnidian School on this point, see the admirable introduction to the book *Περὶ διαίτης ὀξέων*, de victus ratione in morbis acutis.

† Aretæus, *Acut. Morb. Causis*, &c., I. 10.

‡ In the course of these investigations I have made notes of a considerable amount of evidence bearing on the ancient and modern use of bloodletting, which tends to show that the general experience of that remedy is very far from justifying either the practice of many modern physicians, in refraining from it altogether, or the mad caprices of Botal in the sixteenth century, and Bouillaud in the nineteenth, which, it must be confessed, have proved but too seductive to certain minds. I refrain from pursuing, for the present, this argument, which, however, I hope to overtake at a future time. Meantime, the reader who is anxious to pursue this train of thought, cannot do better than peruse a most suggestive clinical lecture by Dr. Alison, in the *Monthly Journal*, vol. xv. p. 498; in which, besides many most valuable considerations on various therapeutical fallacies, the peculiarities of the modern nomenclature of chest diseases, and its misleading influence, are indicated with a clearness of which there is no other example, so far as I know, in our literature.

fully the structure of our knowledge. It is not yet too late to retrace the lost footprints, and to re-establish the old boundaries, which enabled us to reconcile our experience with that of many great physicians, whose names we still hold in regard.

I have endeavoured in the preceding remarks to show that the ancient use of the term *pleuritis* confined its application to severe, and acute, and transient forms of painful and febrile chest-affections. Pleuritis represented essentially an acute fever, with superadded local symptoms; and no case of what we now call *chronic*, or even *subacute*, still less *latent*, pleurisy, could by possibility have been included under that term, or been the subject of the treatment addressed, as a rule, to pleuritis. It follows that the ancient idea of pleuritis, though theoretically and anatomically ill-defined, was in relation to practice (i.e., to prognosis and treatment) far more simple, clear, and, as it were, homogeneous than ours. It was not without reason that the ancient physicians maintained this disease to be essentially *the same* (in relation to treatment), from whatever exciting cause it arose; whether from dissipation in food or drink, from venereal abuses, from violent exercise, or from external injury;* for it comprised a series of cases practically of the same species, far more so, indeed, than those of pneumonia, selected by Louis for comparison in relation to the effects of bloodletting; although we have seen† that Louis proceeded with a degree of caution and exclusiveness which distinguish his researches from those of most other modern authors, singling out the really severe and acute cases, and thereby obtaining results which afford an easy triumph to homœopathic and other statistical medicasters.

It would, however, be a great error to suppose that the modern idea of pleurisy could be reconciled with the ancient by merely cutting off from the former the chronic, subacute, and latent cases. This process would, indeed, reduce the field occupied by the pleuritis to proportions very inconsistent with the term *princeps morborum acutorum*, applied to it by Verna. I have already‡ alluded to the fact that pneumonia occupies in most of our modern therapeutical disquisitions, the position assigned by the Greeks to pleuritis. I must now endeavour to show, what I believe to be beyond all question, the fact *that the pleuritis of all the authors preceding Galen, and indeed of the entire world of medicine up to the sixteenth century, included (besides the comparatively few cases of acute pleuritic effusion) nearly the whole of the more acute types of the modern pneumonia.*

The proof of this proposition cannot of course be looked for in morbid anatomy; for, although the Greeks formed some shrewd guesses (possibly backed by a few imperfect observations) as to the seats of internal disease, it is evident that they do not (with the exception, perhaps, of Galen and his followers) pretend to accurate and definitive information as to the condition of the diseased parts. Still less can we look for the diagnostic characters in which we are now accustomed to place our chief reliance in distinguishing the inflammations of the chest. We must, therefore, have recourse to the more obvious symptoms and signs, and to the general description of pleuritis and peripneumonia.

* "Una est atque eadem passio, ex quâlibet veniens causâ, quæ unâ atque eâdem indigent curatione."—Cælius Aurel., Acut. Morb. II. 13.

† British and Foreign Medico-Chirurgical Review, No. 25, p. 213.

‡ Ibid., pp. 208—9.

Now, with regard to the first, it is to be observed that the *pleuritis* was a disease of great frequency and fatality; that it was almost invariably attended by expectoration from an early period, the contrary cases being exceptional;* and that the expectoration which constituted its usual critical evacuation was precisely similar in its character, in most cases, to that which is now regarded as almost pathognomonic of acute inflammation of the lung. It appears to me that these three points are almost conclusive as to the true place in the nosology of the ancient pleuritis, and that they show to how small an extent it corresponded with the *acute pleuritic effusion* of the moderns. For there can be no question that the latter is, comparatively, a rare disease; that its termination directly by death is still more rare;† and that expectoration can by no means be regarded as one of its proper symptoms, or critical phenomena. Add to this, that the operation of paracentesis, which was not unfrequently performed by the ancients for the chronic disease, *empyema*, is scarcely ever mentioned in connexion with pleuritis.‡

It appears to me very certain, from these and other considerations, that the differences between the ancient and modern pleuritis are not those of degree only (as formerly discussed); and that these *two diseases* (for so we may now call them) are not merely symptomatically, but anatomically distinct, in a large proportion of cases. In other words, that a very great majority of the cases of Greek pleuritis were the pathological equivalents of the modern pneumonia; and that the small proportion of cases of acute pleuritic effusion constituted an exceptional variety of the pleuritis, which was commonly ascribed, after the fashion of the humoral pathology, to defective coction of the morbid matter. It remains that I should illustrate the true relation of the pleuritis to the peripneumonia, by placing before the reader a graphic picture of each. I shall extract its elements from Aretæus and Cælius Aurelianus,§ adopting chiefly the expressions of the latter.

* Such cases are said by Galen to be called ἀπέπτους πλευρίτιδας, *unconcocted pleuritis*. He adds that they are either quickly fatal, or are resolved by a slow process (χρόνῳ πλείονι λυομένης). This last expression must be understood as opposed to the *crisis*, which was, in the opinion of the Greeks, the natural termination of an acute febrile disease, such as the pleurisy, in its ordinary form, undoubtedly was. This description of the unconcocted pleuritis agrees very well with the characters of pleuritic effusion, as we now know it.—Galen, *De Locis Affectis*, Lib. V. cap. 3.

† Dr. Walshe says, on this point—"I have neither myself lost a patient from pure primary idiopathic pleurisy, with or without effusion, nor known of an occurrence of the kind in the practice of others. And although, where chronic disease, either of the lungs or of other organs, pre-existed, death is a more common result, it is still an unusual one." My own experience of this disease has been somewhat less happy than that of Dr. Walshe; but I can readily accept this testimony, which concurs with that of Louis. *Diseases of the Lungs and Heart*, p. 366.

‡ Galen mentions, as we have seen in a former passage (p. 245), that the non-pleuritic pains of the side sometimes result in an abscess opening externally. He does not, however, allude to any such occurrence in the true pleurisy, whether of the ordinary kind, or of the unconcocted variety, which, in this very passage, he takes much trouble to distinguish from the non-pleuritic pains. In true pleurisy, he says there is cough; in the false pleurisy, none. It is plain that Galen here falls into his usual error of over-refinement.

§ I choose these authors in preference to either Hippocrates or Galen, because at the epoch of the Hippocratic writings, the definitions of disease had hardly assumed form and consistence; whereas, in Galen, they are so much stereotyped, as it were, and mixed up with anatomical and physiological hypotheses, as to carry but little of the impression of reality to the mind. Cælius Aurelianus, notwithstanding his crabbed and corrupt Latinity, is unquestionably one of the most valuable and instructive of all the authors of antiquity. I know not how so wretched a linguist came to be possessed of such clear ideas; unless, indeed, we sup-

The pleuritic passion, according to these authors, is marked by acute fever, with cough and severe pain of the side, extending to the root of the neck and to the scapula: sometimes also affecting the arm, breast, and ilium. The respiration is difficult; the cough sometimes dry,* but commonly with liquid expectoration, at first frothy, afterwards sanguinolent, also bilious (yellow), and then sanious. The patients are also affected with difficulty of lying on *that* side,† and when they turn on the opposite side they experience pain, from a sensation as if the swelled viscera were dragged out of position by their weight. Various incidental symptoms are then described; watchfulness, dry rough tongue, cold extremities, suffusion of the eyes and countenance, vomiting, delirium, &c. The pulse‡ is large, rapid, hard, strong, and fluctuating. There is sometimes a rattling in the throat, sometimes a noise resounding or whistling internally on that side which suffers.§ In the transition of pleuritis into peripneumonia|| (a frequent and fatal change, according to all the ancient authors), the pain alone is relieved; the other signs become all worse, the pulse is feeble or absent, and the decubitus is supine. When a *vomica* supervenes on pleurisy, the pain becomes fixed in one place, and is diminished; there is dry cough; the respiration is difficult, and the typical rigor or tremor (of hectic fever?) comes on, the pulse being large. When empyema or vomica occurs as an independent affection, there are shiverings,¶ and pungent pain; and as the pus is at first collected in the side, the occurrence of expectoration is secondary; whereas, in pleuritics, the fever is severe from the first, and the sputa are preternatural, and of various kinds.**

pose, with several authorities. that he was a mere translator. It indicates, however, a very marvellous preference for style over matter, on the part of mankind, that the elegantly-written, but common-place compilation of Celsus should have run through hundreds of editions, while these truly instructive and original writings have found only one careful editor, and no translator, so far as I am aware, in modern times.

* “Tussicula aliquibus arida, frequenter tamen cum liquidis excrementis, et primo spumosis, dehinc sanguinolentis, ita fellosis, ac inde saniosis.”—Cælius Aurel., A. M. II. 14. He afterwards describes at length these and other varieties of the pleuritic expectoration, and says, that the order of their ejection is often changed (*mutato sæpe ordine sue egestionis*). They are, besides, *not homogeneous* (*inæqualia*). According to Aretæus, dry cough, sputa difficult to be brought up, composed of phlegm, or of matters like bile (*χολώδες*), or largely mixed with blood (*διαίμων κατακορέως*), or yellowish (*ὑπόξανθον*); and not keeping this order, but promiscuously appearing and disappearing. The worst of all, however, he says, is when the blood-mixed sputum suddenly disappears.—Aret. de Morb. Acut. Causis, &c., I. 10.

† “*Id latius*.” There is evidently some confusion here; for Aretæus, who has the same ideas of the dragging of the viscera, says, that on the inflamed side the decubitus is well borne, because the membrane (of the pleura) settles into its proper place (*ἐν ἑδρῇ γὰρ ἔξει τῇ ἐνυτέρῳ ὁ ὑμῆν*), while, by reclining on the opposite side, pain is excited. See the chapters already cited. It is now well known that the phenomena of the decubitus in the early stages of pleurisy are not to be depended on.

‡ Aretæus does not mention the pulse. Galen describes it as *small*, and indicating a hard and tense artery.—De Locis Affectis, V. 3. Modern physicians (many of them, perhaps, without knowing it) have adopted too many of Galen's intricate and fantastic descriptions of pulses, in which, as usual, his imagination runs a long way in advance of the facts; as any one may see in his books De Pulsibus.

§ “Gutturis stridor, vel sonitus interius resonans, aut sibilans, *in eâ parte quæ patitur*.” (Loc. cit.) Laennec, whose reading was commonly as extensive and careful as his personal studies of disease, seems to have missed this passage; otherwise he would surely have put it beside the one which he quotes from Hippocrates—de Morbis—as showing the origin of auscultation among the ancients. There is a somewhat similar passage in the Prognostics, in which Hippocrates describes the boiling of matters in the air passages. See further on, p. 253, note.

|| Cælius Aurel., loc. cit., c. 15.

¶ Loc. cit., c. 17.

** According to Aretæus, loc. cit., if pleuritis is not resolved in three weeks, it changes to

So much for the pleuritis, its consequences, and its diagnosis. Let the reader compare with this the description of the peripneumonia, as extracted from the same authorities.

Peripneumonia is accompanied by acute fever, a sense of weight in the thorax, and of labour in effecting its movements;* the decubitus is supine, or a little raised; sometimes the sitting posture is preferred. The face is florid, especially the cheeks; the veins of the temples and neck are dilated; the eyes are bright and full (pinguist†). Moreover, the breathing is accelerated, and there is cough, with sanguinolent and bilious (yellow) or fuliginous (?) sputa, *more tawny and frothy than those of pleuritis.*‡ The pulse is vehement and rapid (various other general symptoms are here described). When the disease becomes worse, the thorax is rather protuberant,§ and the breathing is distressingly difficult, accompanied by *a certain vehement and harsh whistling*; at last the pulse is latent or creeping, such as the Greeks call *μυρμιζοντα*, and a sounding noise is heard in the breast, which they call *rhogmon*. The circumstances which chiefly distinguish the peripneumonia are these,—acute fever, rapid and difficult breathing, cough, expectoration of various kinds, *oppression* (gravatio) *without any pain, or with slight pain only*, and a sense of suffocation.

That the reader may be the better enabled to contrast the ancient peripneumonia with the pleuritis, and that he may be likewise led to bring the descriptions of both diseases into comparison with the modern affections bearing the same or similar names, I shall place in a tabular form the symptoms of these affections, together with some reference to the opinions maintained in respect to their nature and seat.

PLEURITIS.

Acute fever.

Cough.

Pain of one side, severe (vehemens), pungent, extending to neck and scapula, &c.

Respiration impeded and accelerated (δυσπνοια). (Galen and others distinctly describe the thoracic movements as checked by the pain.)

Expectoration sometimes absent (but this was exceptional, and more characteristic of *empyema* or internal suppuration); commonly charged with blood, or yellow; sometimes mucous, and varying during the progress of the disease.

PERIPNEUMONIA.

Acute fever.

Cough.

Pain absent, or slight, not localized; sense of weight in the thorax (gravedo, gravatio).

Respiration rapid, accomplished by means of laboured movements of thorax.

Expectoration never absent, not differing much in appearance from that of pleuritis, but, on the whole, more yellow and frothy.

empyema. (I have already noticed the ideas of Hippocrates on this point, see *ante*, p. 244.) The great danger is, that the lung, being a warm and rare organ, may attract all the matters to itself; whence follows either rapid death by suffocation, or an internal suppuration.

* The text of Cælius is here very corrupt, but there can be no doubt of the general meaning. "Febres acutæ, gravedo thoracis, et sensus laborantium quâdam difficultate laterum, atque medium papularum."—Acut. Morb., Lib. II. c. 26. All other authors agree as to these facts.

† Compare Aret., Acut. Morb. Caus. II. 1. The references to the "fat" appearance of the eyes, and the bluntness of the nose, are a little puzzling. I think it better to leave questionable and trivial details out of view, and only to give the general sense. The curious reader may refer to the originals.

‡ "Sanguinolenta, atque fellea, vel fumosa jactans sputa, et in comparatione pleuriticorum fulviora, vel spumosiore."—Cæl. Aur. loc. cit. We know something of fuliginous or carbonaceous sputa now-a-days, but I question whether these are here meant.

§ Possibly this idea is connected with the supposed swelling of the lung in peripneumonia, a fancy which continued to prevail down to the time of Laennec, and which that observer did not fail to notice and to correct.

Decubitus lateral.

Pulse large, hard, and strong.

Countenance injected and suffused.

Abnormal sounds occasionally heard on the side affected.

Disease believed to be unilateral, and to have its seat in or about the costal pleura, and the neighbouring parts. (Some, however, maintained that it was in the lung itself.)*

Decubitus dorsal or suberect.

Pulse at first strong and rapid, afterwards small and indistinct.

Countenance congested, with swelling of temporal or jugular veins.

Abnormal sounds, not localized, and of a loud and harsh character.

Disease believed to be bilateral and general, and to have its seat in or about the lung,† which, from its anatomical structure, was believed to be incapable of feeling pain. Hence the idea, "plus periculi quam doloris," as applied to peripneumonia.

The preceding descriptions, and their analysis, will, I believe, be found sufficient to sustain the conclusion which I have already placed before the reader by anticipation—viz., that the *pleuritis* of the ancients included, besides the more acute forms of pleuritic inflammation, the greater part of the cases of modern *acute pneumonia*. It may, however, be desirable still farther to illustrate this proposition, and to lead the way to another, by a few considerations tending to show in detail, not only that the description of the ancient pleuritis corresponds, for the most part, accurately with that of the modern pneumonia; but that the peripneumonia, as described by the Greeks, does not do so; and that, on the other hand, the latter does correspond, in general, with another form of disease.

Let the reader, then, consider, for a moment, the severe pungent *pain* which was the most striking attribute of the Greek pleuritis, and contrast this affection with the peripneumonia, in which pain was either absent, or slight and non-localized. Let any careful observer, then, compare these two diseases with the pneumonia of the moderns, in its really acute forms (excluding, of course, the cases of typhoid and catarrhal pneumonia, of which I gave an account in a former paper), and let him say which of the two, peripneumonia or pleuritis, most nearly represents

* Called *ὑπεζωκὸς* (*ὑμῆν*), the encircling membrane; so called, says Galen, because it encircles inwardly the whole side. Its relation to the ribs and intercostal muscles was pretty well understood; but it is important to recollect that the *pulmonary* pleura was totally unknown till a comparatively recent period. The humours which produced the pleurisy were therefore supposed to be collected between the costal pleura and the ribs; and the pain was due to the compression which the parts sustained, as well as to the seat of the disease being a membrane part, which, like all the other membranous parts, was considered to be derived from the membranes surrounding the brain, and to be amply furnished with nerves or fibres. The lung, on the contrary, being scantily provided with *nerves*, was wholly insensible; and this argument was conceived to fix the lung as the seat of the peripneumonia, as well as to form an answer to those of the ancients who alleged it to be the part involved in pleurisy. The controversies on this point, beginning as they did in the earliest times, were not much nearer a settlement at the time of Morgagni. We shall see hereafter how much confusion they have introduced into the nomenclature, and even the descriptions of authors. Meantime, compare Galen, *De Anatomicis Administrationibus*, c. 2; *Idem*, *De Locis Affectis*, II. 3, and V. 3; Aretæus, *De Acut. Morb. Causis*, &c., II. 1; Cælius Aurelianus, *Lib. II.* c. 16 and 28.

† The chapters of Cælius Aurelianus referred to in the preceding note, give a curious and most interesting view of the anatomical speculations of the ancients, as to the special seat both of pneumonia and pleurisy. With regard to the peripneumonia, it is evident that the veins, arteries, and bronchi of the lung had all fallen under suspicion, and it does not appear that anything more definite was known up to the beginning of the sixteenth century. The term *peripneumonia* has puzzled authors and etymologists not a little; some of them supposing that *περί* here means *around*. Iacnec repudiates this, and argues, with some reason, that *περί* is merely an intensive preposition; as though the meaning were, the *general affection*, or *very grave* inflammation of the lung. On the other hand, Galen remarks that, according to some persons, the lung itself is not susceptible of inflammation; but that the adjoining parts being in this condition give rise to the inflammation about the lung, *περί πνεύμονα*.—Galen, *Isagoge*, seu *Medicus*.

the acute inflammation of the lung as known to him. Is the modern pneumonia a painless disease in a large proportion of cases when the above deductions are made? I scarcely anticipate much difference of opinion on this point; but lest my own statements be regarded as biassed, I shall place below the answer of Grisolle,* undoubtedly the most exact of modern observers of this disease, which show that in primitive acute pneumonia the absence of localized latent pain is a rare occurrence, and that the disease now called pneumonia never could have led the Greeks into the belief that its essential seat was in the "insensible lung" (ὁ πνευμὼν ἀναισθητός).

What, then, was this *painless*, or slightly painful, affection of the lung, more dangerous than painful, accompanied by acute fever, cough, various expectoration, difficult and laborious respiration, orthopnoea, and all the signs of impending asphyxia? What was that disease which, when it supervened on pleuritis (pleuro-pneumonia), was almost invariably fatal, because it superadded to the *local* a severe *general* affection of the lung? What was the disease in which a "vehement and harsh whistling" was habitually heard, and in which, on the suppression of expectoration, the lung was wont to "fill with matters which boil in the air passages?"† Such was the ancient *peripneumonia*. It is to me evident that such, too, is the modern *acute bronchitis*.

In acute bronchitis the respiration is not short and hurried, but, even when it is much more frequent than natural, it appears long-drawn and laboured. This was precisely the distinction between *peripneumonia* and *pleuritis*.

In acute bronchitis, the expectoration differs from that of pneumonia and pleurisy chiefly in being more yellow and frothy. This was also a distinction of the *peripneumonia*.

In acute bronchitis, the decubitus is dorsal or suberect, not lateral. This was also the case in the *peripneumonia*.

In acute bronchitis, a strong and rapid pulse is quickly reduced, by the advance of the disease, to a fluttering or "latent" one. This character was also conceived to mark the pulse of the *peripneumonia*.

In acute bronchitis, the veins of the neck and face become not unfrequently swollen, especially when the disease occurs in connexion with emphysema and disease of heart. This symptom was also insisted on in the *peripneumonia*, which was, moreover, the most frequent and best established fatal termination of almost all other thoracic diseases, and especially of catarrh.

Finally, in acute bronchitis, a sense of weight or oppression in the thorax takes the place of positive pain, and is perhaps the most marked abnormal sensation. The description of the *peripneumonia* here, again, is exactly in accordance with that of the modern disease.

On these grounds, as well as on others which will probably occur to the readers of ancient descriptions, but which if detailed here might

* "Une douleur plus ou moins vive siégeant dans un des côtés de la poitrine est un symptôme qui accompagne presque constamment la pneumonie. Il résulte, en effet, de mes observations que sur 301 malades affectés d'inflammation du poumon, la douleur de côté n'a manqué que chez 29, ou chez un dixième environ." A further statement shows that the pain was most accurately localized.—Grisolle, De la Pneumonie, p. 198.

† See Hippocrates, Prognostics, ἀλλὰ πλήρης ἔων (ὁ πνευμὼν) ζέει ἐν τῇ φάρυγγι.

seem over-refined, I am disposed to identify the *peripneumonia* of the Greeks with *acute bronchitis* far more than with *pneumonia*. Yet I would by no means be understood to deny that a certain number of cases of inflammation of the lung, or of broncho-pneumonia, fell under the designation of peripneumonia; in the same way as a certain proportion of cases of pleuritic effusion without pneumonia fell under the designation of pleuritis. I believe that both these results were inevitable, and that they actually took place. In fact, the general scope of the Hippocratic writings would seem to imply (what, indeed, is undeniably the fact), that the localization and distinction of the different acute diseases of the chest was by no means so easy in practice as it was in nomenclature and in theory. How, indeed, could it be otherwise at that period? Are these distinctions absolutely clear, even at the present day? I venture to believe that no one familiar with disease from personal observation, and particularly with the phenomena which I have elsewhere described as interfering with the stethoscopic diagnosis of pneumonia, will hesitate to admit the negative.

Yet it is of extreme importance, in studying the past history and progress of the medical art, to possess a nosological index, however imperfect, to the works of those who have gone before us in observation and in theory; and who have left us a legacy of opinion, despised by some, neglected and forgotten by others, but clinging with not less tenacity to those who treat it with professed indifference and contempt, than to those who have endeavoured to appreciate and understand it. On this ground I venture to propose the following interpretations of the ancient nomenclature of diseases of the respiration, submitting them, not without much diffidence, but yet with a strong feeling of conviction, to the verdict of those more accustomed than I am to such inquiries.

1. That for the *περιπνευμονία*, or *περιπλευμονία* of the Greeks, we may usually read *acute bronchitis*, or *broncho-pneumonia*; and in every case an extremely acute febrile disease, with little pain, tending to rapid suffocation and prostration of the vital powers.

2. That for the *πλευρίτις* we may read *pleuro-pneumonia*, or more rarely *pleurisy*, excluding the chronic, trivial, and protracted cases of both; a highly acute *localized* febrile disease, attended by hard firm pulse throughout, but sometimes passing into the *περιπνευμονία*, and then assuming its characters.

3. That in the descriptions of *ἐμπύημα*, and in the case of the corresponding expression *ἔμπυοι*, we should have in view not merely the disease called empyema, but a much wider range of sub-acute and chronic affections—viz., all cases of acute disease of the lung and pleura indiscriminately when protracted beyond three, or, at most, six weeks; and in addition to these, all diseases commencing as chronic affections, and ending in purulent discharge, whether by expectoration, external opening, or otherwise; whether in the chest or in the abdomen.

One further consideration appears requisite in order to illustrate completely the above propositions, and the nosological revolutions connected with them. The history of modern inquiry ought to afford some clear explanation of the modifications which have taken place in the ideas of

the classical period as to peripneumonia and pleuritis. The links between the nomenclature of the past and that of the present day deserve investigation. Fortunately, the materials for this inquiry still exist; its results, too, are both curious and interesting.

The close of the fifteenth century brought with it in medicine, as in other departments of human knowledge, a revival of the classical, and especially the Greek models, in opposition to the mediæval commentators and encyclopedists. Almost simultaneously, the spirit of observation began to awake, clinical facts were recorded with something like independence, and anatomy, which had been to some extent cultivated and reformed by Mundinus and his successors, was applied to the discovery of the seats of disease, in regard to which the Greeks had made few, if any, direct observations. It is impossible to deny that in the glimmering twilight of this period pathological observations were recorded which have not been discredited by the progress of science; and if the sixteenth century failed to produce its Morgagni as well as its Vesalius, it was not so much from the want of a scientific spirit at its commencement, as from the erratic fanaticism of the Paracelsists, whose clamorous folly succeeded to a considerable extent in withdrawing attention from the incipient science of morbid anatomy, and, indeed, well-nigh supplanted clinical observation by the angry disputes of the chemists and Galenists. It was not till the middle of the seventeenth century that the storm blew over; many of the earlier observers had then been almost forgotten; and had it not been for the laborious collections of Schenck and Bonet, and the well-directed efforts of a few distinguished men, whose truthfulness and simplicity of character withdrew them from the theatre of this disastrous and turbid controversy, medicine would have fallen back again into worse than mediæval darkness. Perhaps, in our somewhat exaggerated, but still well-founded, admiration for Sydenham as "the English Hippocrates," we have too hastily set aside some of his predecessors. Plater, at least, deserved more attention than he has received; the same may be said of not a few still earlier leaders in the path of original observation. The present subject affords an illustration of this remark.

All the systematic nosologists, from Sauvages to Cullen—and, indeed, all the writers of the eighteenth century, not excepting even the learned and generally accurate Morgagni—ascribe to Sydenham the original description of the *peripneumonia notha*. All of them discuss its nature as if it were a new disease, or, at least, one greatly neglected by the older physicians.* To judge, indeed, from Sydenham's own descriptions, it can scarcely be supposed that he intended to introduce the *peripneumonia notha* to the world as a member of the *nova februm cohors*. He expressly says that it was of annual occurrence: "Hyeme ingruente, at sæpius sub ejusdem exitum, vereque adhuc nascente, quotannis emergit febris symptomatis peripneumoniacis haud paucis conspicua."† Besides, his classification of it, with pleuritis and rheumatism, among the *intercurrent* fevers, seems to show that he regarded it as one of the permanent denizens of the English soil, at least in his own age. Still, the habitually terse and

* See Van Swieten, Commentar. in Boerhaavii Aph. 867; Lieutaud, Précis de la Médecine Pratique, Livre I.; Cullen, First Lines, Part I., Book II., chap. 7.

† Sydenham, Observationes, VI. 4.

dogmatic style of this author, and the circumstance of his using, without explanation, a term not familiar to the readers of the classical literature, contributed to diffuse the impression, that Sydenham either invented this term, or gave to it a new significance by attaching it to a hitherto neglected disease.

The difficulties which have been felt and expressed by authors in relation to the *peripneumonia notha* of Sydenham show very clearly that the term, unless applied to a new and rare disease (which it was not), is an unnecessary refinement on the ancient nomenclature. In the first place, it must be remarked, that Sydenham himself, in order to find room for this "fever distinguished by most of the symptoms of *peripneumonia*," absolutely removes from the nosology the *peripneumonia* itself; of which he merely says, that it is "of the same nature as pleurisy, and only differs from it in affecting the lungs more generally."* Boerhaave, on the contrary, describes both the true and false *peripneumonia*; but the peculiarities that he ascribes to the latter are almost entirely theoretical, relating, as they do, more to its cause than to its symptoms. It is impossible to read the portion of Morgagni's twenty-first letter† which relates to this subject without feeling that the identification of this disease was by no means an easy or satisfactory task to him; and the same remark will apply to Lieutaud. Finally, Cullen adopts Sydenham's distinction; but Cullen also refuses to distinguish the *peripneumonia vera* from the pleurisy. In both these respects he agrees with John Peter Frank.‡

The reader of the preceding pages must have already divined the cause of these embarrassments. The distinction of Sydenham was a distinction without a difference; for his *peripneumonia vera*, which he believed to be identical in nature with pleurisy, was, in fact, the pleuritis of the ancients; and his *peripneumonia notha*, making allowances for theoretical and insignificant differences, was the *peripneumonia* of the ancients. In other words, it was only by losing sight of the ancient distinctions between pleurisy and *peripneumonia* that the moderns contrived to introduce a new febrile acute disease of the chest. By doing so, they effectually broke the link between the ages, and established a barrier of words between themselves and the Greeks. It was not, however, Sydenham who was directly responsible for this result. A very slight acquaintance with the writings of that great physician and of his predecessors will show that, in regard to nomenclature at least, he was no rash innovator; and that, in this instance, he merely adopted a term which had been current for the greater part of a century.

To show the real origin of this change, we must go back to the clinical observations of the sixteenth century. One of the earliest collective writings in which the actual name "*peripneumonia notha*" occurs is the work of Forestus, who gives it, however, not as his own innovation, but as a term used among his contemporaries:

"Non desunt," he says, "qui in *veram* et *legitimam* et *notham* (*peripneumoniam*) distinguunt; unde et Fernelius *raram admodum veram peripneumoniam esse asseruit*, cum et tenuis et acris multaque destillatio è cerebro confertim in pulmones illapsa, interdum ardore præter naturam incenditur, æstum ac febrem inferens; ac plerique

* Loc. cit.

† De Sedibus et Causis Morb., Epist. XXI. 13, 14.

‡ Frank, De Curandis Hominum Morbis, vol. i. § 183 et seq.

eam affectionem peripneumonia nomine designant, in quâ et tussi et spiritus difficultate et lentâ febre sensim conficitur æger sine ulcere, et sine expuitione cruentâ. Hæc si peripneumonia dicitur, ab exquisitâ profecto plurimum tum causâ tum symptomatum magnitudine dissidebit.”*

The latter part of this passage is merely a quotation from Fernelius. Now Fernelius, although he protests against the undue extension of the idea of peripneumonia, and asserts that *true idiopathic peripneumonia* (quæ non alium morbum subsequitur, sed ex sese primum duxit originem)† is a rare disease, does not himself employ the term *peripneumonia notha*. On the other hand, this term is employed by Forestus (as we have seen), by Nicolaus Piso,‡ and possibly by other compilers, who either quote or refer to Fernelius as the source of their ideas. Lommius, another contemporary compiler, follows Fernelius very exactly in his ‘Observationes Medicinales,’ and, like him, leaves this spurious and evidently catarrhal peripneumonia without a name.§ From all which circumstances it is quite clear that the name first acquired currency among the numerous followers of Fernelius, being applied by them to the *more dangerous kinds of catarrh, i. e., to bronchitis and broncho-pneumonia, unattended by extreme fever and bloody expectoration*. This has also been considered, by the more modern writers, to be Sydenham’s application of the term. Frank, indeed, describes it as a “catarrhus bronchiorum.” Badham identifies it with the modern *sub-acute* bronchitis.|| Sydenham’s description, however, refers to a highly acute disease, evidently only one stage removed from the most acute forms of thoracic inflammation, and distinguished by the intensity of its febrile accompaniments, from asthma, and from the *febris hyemalis*, which he describes in a postscript to the treatise, ‘De Podagrâ et Hydrope.’

Now, I have endeavoured to show above, that the Greek peripneumonia was distinguished from pleuritis by precisely those characters which separated Sydenham’s *peripneumonia notha* from pleuritis or from the true peripneumony, regarded by him as the same disease. It will be observed that both Sydenham and Fernelius come to the conclusion (though on different grounds), that the name *peripneumonia vera* is of very limited application. This was certainly not the Greek idea. There is no hint in any classical writer that the peripneumonia was otherwise than a common disease; and we have seen above that the descriptions of it were by no means wanting either in clearness or in truth to nature, if we may judge by the forms of disease which nature presents to our observation at the present day.

The more closely the details of this subject are examined, the more evident does it become that the distinction between the *true* and the *false* peripneumony sprang from an entire misapprehension of the idea of peripneumonia, as understood by the ancients. The assertion of Fernelius that it was in his time “very rare,” would be utterly incomprehensible (except, indeed, upon the theory that the whole face of nature

* Forestus, Observ. et Curat. Med., Lib. XVI., Obs. 44.

† Fernelius Patholog., Lib. V., chap. 10.

‡ N. Piso, De Cognosc. et Curand. Morbis, Lib. II. 8.

§ Lommius, Med. Obs. Liber Secundus. “Non omittendum autem id vitium pulmonis est, quod ex tenui, acri, multâque destillatione e capite confectum in hunc ipsum illapsa oritur,” etc.

|| Badham on Bronchitis, 2nd Edition, p. 5.

had changed) were it not for the distinct proofs which I shall presently adduce, that even before the middle of the sixteenth century (and Fernellius's great work was not published till 1554), the very existence of peripneumonia as a separate form of disease had been brought into controversy, in consequence of the progress of anatomical knowledge, and its supposed bearing upon nosological questions. That the name peripneumonia notha, on the other hand, was not a real gain to science, seems to be sufficiently demonstrated by the confusion which followed its introduction; for although the writers after Sydenham for the most part retain the names of both the true and the false peripneumony, they show perpetually that they do not know what to do with them, and that they would willingly suppress either the one or the other. Thus, Hoffman and Cullen confessed, like Sydenham, the inability to distinguish the peripneumonia from the pleuritis; De Haen and Stoll, following Vincent Baron, merged these two names into a compound term, and thus maintained the identity of the diseases; Morgagni and Lieutaud, even while making the same admission, hesitated in adopting the new nomenclature (which they referred to Sydenham and Boerhaave), on the ground that authors were not agreed about the characters and nature of the *peripneumonia notha*. It is unnecessary again to refer to the evidence that more modern writers have misapplied both of the Greek terms, and by identifying bronchitis and broncho-pneumonia with Sydenham's description rather than with the peripneumonia of the Greeks, have crowned the confusion, and lost the key of the whole ancient pathology, as regards thoracic acute disease. In Dr. Badham's work, this misunderstanding is clearly apparent. The author sweeps through ancient and modern literature in search of bronchial inflammation, which he discovers everywhere in odd corners and exceptional passages; leaving the impression that this disease, though "touched upon in various writings on practical medicine," was nearly unknown to the whole ancient world, and scarcely sufficiently known—at least, in its most acute form—up to the date of his own work.

To find the source of the nosological movement by which the peripneumonia of the Greeks was broken up into new elements, we must have recourse to other authors than Fernellius, who, though he embodied in his erudite system of medicine most of the ideas of his time, can scarcely be said to have added largely to them by his own observations. It was in the course of the anatomical investigations of the sixteenth century, to which I have already alluded, that the question was raised as to the independent existence of the peripneumonia. The points thus brought into controversy were of great importance.

In the first place, anatomical investigations taught the physicians of the sixteenth century that the pleuritis of the Greeks was not, as had been supposed, an inflammation especially of the costal pleura, or *membrana succingens*; but that it almost always involved the lung and its covering, and generally, indeed, appeared to have its origin there. The opinion of Galen and the Arabians was not indeed given up without a struggle; and long after this period it continued to be held by Sennert, Diemerbroeck, Riolanus, and others, that the pleura of the ribs only was affected in some cases of pleuritis, and that the lung and its pleura were

secondarily involved.* But the results of unbiassed observation afforded multiplied proofs that in pleurisies the lung and the pleura were commonly simultaneously affected; and Plater, accordingly, with his usual good sense, assumes that the peculiar expectoration of pleuritis, which the Greeks had supposed to be a critical evacuation from the pleura, was in reality an expectoration from the lung itself. He also concludes, as Sydenham afterwards did, that there is no important distinction between the symptoms of inflammation of the pleura and that of the lung.† It is easy to see how the confusion of peripneumonia with pleuritis sprang out of this opinion.

In the second place, the physicians of the sixteenth century learned from anatomy that the peripneumonia of the Greeks was not allied to any single group of morbid appearances; and, in particular, that it could not be regarded as the symptomatic correlative of inflammation of the lung, which, as we have seen, they found to be the most frequent cause of the pleuritis. Thus, while it occasionally happened that those affected with peripneumonia had the lungs inflamed and hepatized, or suppurated (as described by Lælius a Fonte, Dodonæus, and others), cases constantly occurred which were rightly considered to be exceptions to this law. It was undoubtedly the existence of such cases which led to the idea of a false or spurious peripneumony.‡ Nor was it an incorrect observation that the peripneumonia of *Galen* (i. e., an inflammation of the lung having the symptoms of the peripneumonia) was a rare disease; for the true inflammation of the lung is almost always unilateral and attended by pain, whereas the peripneumonia was general and painless.§

To sum up in a few words the principal elements of this protracted inquiry: it will be remembered that the Greeks did not attempt to distinguish more than two severe acute diseases of the respiration—i. e., the painless *Peripneumonia*, resulting, when fatal, in suffocation and rapid prostration; and the acutely painful *Pleuritis*, attended by hard pulso and high fever throughout, but often terminating in peripneumonia. From these two affections, the modern correlatives of which are *Bronchitis* or *Broncho-pneumonia* on the one hand, and *Pleuro-pneumonia* or *Pleurisy* on the other, all the chronic or even subacute, and all the trivial cases, were separated under other names (as Catarrh, Phthisis, Empyema, Pleuritis notha, &c.). The anatomical seat of these diseases raised much controversy, but was never held to be decided, until Galen, with his habitual dogmatism (but with no better information than his predecessors), laid

* The whole of this controversy may be found in Bonetus, Sepulch. Anat., Lib. II. § 4, Obs. 14, 20; and in Morgagni, Epist. XX., XXI.

† Felix Plater, Praxis Medica, Lib. III. 10; and Observationes, Lib. II.—in pectoris dolores.

‡ The following words by Cardan are remarkable, as presenting perhaps the earliest instance on record of a distinctly described bronchial affection. In recording certain cases of epidemic catarrh, he remarks of one of them:—"Dissecuimus illum, invenimusque non esse abscessum; sed omnia vasa pulmonis ex triplici genere, *magis tamen asperæ arteriæ ramî* erant pleni sanie simili lacti. . . . Fluxio illa non erat, ut dux priores; sed *pulmonia munda*. . . . Differt hic morbus a pulmonia, quod abscessus non est."—De Providentiâ ex anni Constitutione, quoted in Schenck, Lib. VI., Obs. 4.

§ The localized character of inflammation of the lung is remarked by Joubertus, De Affect. Thoracis, cap. 7, (see also Schenck, Lib. II., De Peripneum., Obs. 3.)—"In pulmoniam non est necesse totum pulmonem inflammarî, (as Galen and the Arabians had taught); quin sæpius unus aut alter lobus id patitur; ut infinitorum cadaverum apertio nos docuit."

down what proved to be the law on the subject for the whole middle age.

The earliest cultivators of morbid anatomy proved that Galen was wrong in his ideas; that the pleuritis was not an inflammation of the costal pleura, but of the lung; and that the peripneumonia was not in all, or even in most, cases coincident with an inflammation of the lung. Hence arose the name peripneumonia *notha*; the peripneumonia *vera*, or true inflammatory affection of the lung, being regarded by the best writers of the sixteenth century as but little different from pleuritis, and being likewise treated as indistinguishable from pleurisy by Sydenham, Huxham, Morgagni, De Haen, Stoll, Hoffmann, and Cullen, in more modern times.

Finally, the key of the ancient nosology has been doubly lost by the moderns:—1st, by the description of trivial and chronic diseases under the names which formerly were applied only to serious and very acute ones; 2nd, by the introduction of *anatomical* distinctions and names, without sufficient regard to the *symptomatic* distinctions and names which they supplanted or thrust aside. But as a good and practical nomenclature of diseases ought to be founded on symptomatology, and not on morbid anatomy, it still remains for consideration how far clinical observation corresponds with our present nomenclature. From what we have already seen, it would appear that the practice of physicians has been in some cases endangered, and their diagnosis perplexed, by the present neglected condition of nosological science, which renders it impossible to connect accurately the experience of the past with that of the present day. It ought, therefore, to be carefully considered, whether the same evils cannot be avoided in future.

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF PHYSIOLOGY.

BY HERMANN WEBER, M.D., Physician to the German Hospital.

I. FOOD AND DIGESTION.

1. *Succi Gastrici Humani Indoles Physica et Chemica.* By OTTO DE GRUNEWALD. (Dissert. Dorpat, 1853.)
2. *Succi Gastrici Humani vis Digestiva.* By E. DE SCHROEDER. (Dissert. Dorpat, 1853.)
3. *De Succo Pancreatico.* By SIGISMUND KROEGER. (Dissert. Dorpat, 1854.)
4. *Some Remarks on the Gases of Digestion of Horses.* By Professor VALENTIN. (Vierordt's Archiv, xiii. 3, pp. 356, ss. 1854.)

1, 2. GRUNEWALD and SCHROEDER made their observations on a peasant woman affected with fistula of the stomach. The woman was otherwise healthy,

* We have been compelled to defer, till future numbers, Reports on Chemistry, and on Materia Medica, and to shorten the Reports on Medicine and on Midwifery. We do not undertake to notice all new papers in the Chronicle, but trust not to overlook any important article. As less accessible to most of our readers than the British periodicals, the Foreign journals are chiefly reported. During the months of September, October, and November, we received the following Foreign journals.

GERMAN.

1. Archiv für Anatomie, &c., von J. Müller. 1853, Heft 6.
2. Archiv für Phys. Heilkunde, von Vierordt. 1854, Heft 3.
3. Archiv für Pathol. Anat., von Virchow. Band vii. Heft 1.
4. Archiv für Chemie und Mikroskopie, von Heller. Band ii. Heft 10—12.
5. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. July, August, September.
6. Zeitschrift (Henke's) für die Staatsarzneikunde, von Behrend. 1854, Heft 3.
7. Zeitschrift für Psychiatrie, von Damerow, &c. Band xi. Heft 2.
8. Vierteljahrschrift für die Prak. Heilk. Prag, 1854. Band 3.
9. Verhandlungen der Phys.-Med. Gesell. zu Würzburg. Band v. Heft 1.
10. Schmidt's Jahrbucher. 1854, Nos. 9, 10, 11.

FRENCH.

11. Archives Générales de Médecine. Sept., Oct., Nov.
12. Revue Méd. Chir. de Paris. Sept., Oct., Nov.
13. Bull. Gén. de Thérapeutique. Sept., Oct., Nov.
14. L'Union Médicale. Sept., Oct., Nov.

ITALIAN.

15. Bulletino delle Scienze Mediche. Bologna. Feb. April, May.

SPANISH.

16. Seminario Médico Español. Sept., Oct.

AMERICAN.

17. American Journal of the Medical Sciences. Oct.
18. Philadelphia Medical Examiner. August, Sept.
19. The American Journal of Insanity.
20. New Orleans Medical News and Hospital Gazette. Vol. I., No. 14.

35 years of age, weighed 53 kilogrammes, and was, at the time of observation, nursing a child; the fistula had existed for two or three years, and was caused probably by a perforating ulcer of the stomach. The *quantity* of the gastric juice, after abstraction of the saliva (65 grammes per hour) was computed at 584 grammes (=19 oz. nearly) for every hour, or 14,016 kilogrammes (=22 $\frac{3}{4}$ imperial pints nearly) for the day, which is about the fourth part of the weight of the whole body, a proportion much larger than that given by Bidder and Schmidt (6.4 kilogrammes for an individual weighing 64 kilogrammes);* 1 kilogramme of weight of a man would secrete, according to this calculation, 264 grammes (=8 $\frac{1}{2}$ ounces nearly) of gastric juice in twenty-four hours. The smallest proportion was obtained in the morning (state of fasting), being, however, even then not less than between 40 and 400 grammes per hour. The fluid collected at that time was, in general, thin, serous, colourless, and clear; at other periods it was more viscid, containing sometimes bile, without any accompanying signs of functional derangement; not rarely sarcina was detected in it by aid of the microscope. As regards the *chemical constitution*, according to the examination performed by C. Schmidt, the fluid obtained at an early hour of the morning (state of fasting) was either neutral or slightly alkaline; after the ingestion of food it was always acid. *Hydrochloric acid* was not found in any of the analyses of the various portions of the gastric juice secreted at different times of the day, but *butyric acid* and the presence of *lactic acid* became highly probable. The following table shows the numerical proportion of the single constituents; for the sake of comparison we add the figures given by Bidder and Schmidt for the gastric juice of the *dog* and *sheep*.

	MAN. Gastric juice mixed with saliva.	DOG. Gastric juice without saliva.	DOG. Gastric juice mixed with saliva.	SHEEP. Gastric juice mixed with saliva.
Water	956.595	973.062	971.171	986.147
Total solids	43.405	26.938	28.829	13.853
Organic substances	36.603	17.127	17.336	4.055
Inorganic substances	6.802	9.811	11.493	9.798
Single substances:				
Hydrochloric acid	—	3.050	2.387	1.284
Chloride of potassium	—	1.125	1.073	1.518
Chloride of sodium	4.633	2.507	3.147	4.369
Chloride of calcium	—	0.624	1.661	0.114
Chloride of ammonia	—	0.468	0.537	0.473
Phosphate of lime	0.961	1.729	2.294	1.182
Phosphate of magnesia	0.260	0.226	0.323	0.577
Phosphate of iron	0.006	0.082	0.121	0.331
Potash, belonging to the organic } substances	0.363	—	—	—

The organic substances consisted of coagulable albuminous substance (pepsin), sugar, butyric acid, uncoagulable proteinaceous substances, and lactic acid. The organic acids are not considered as originally present in the secretion of the stomach, but as products from the ingested aliments; they are changeable in quantity according to the quality of food. The hydrochloric acid, on the contrary, was considered as an essential component, although it is not exhibited by the analyses, probably on account of its being so easily bound by the alkalies of the saliva. In an analysis performed at a period, the results of which are communicated in the appendix, Schmidt actually found free hydrochloric acid, but only in the proportion of 0.2 in 1000 parts,—a proportion ten times smaller than in the dog. Concerning the question, whether the gastric juice prevents the saliva from transforming starch into sugar (as asserted by Bidder and Schmidt) the authors found that the action of the saliva is not annihilated; they confirm, however, the observation of Bidder and Schmidt, that no sugar is detected in the stomach of the dog even after the ingestion of boiled starch.

The digestion of *proteinaceous food* was examined by introducing weighed pieces

* Die Verdauungssaefte u. der Stoffwechael. Mitau and Leipzig, 1852, p. 36.

of coagulated albumen, meat, &c., in thin linen bags through the fistula into the stomach, and observing both the loss of substance within a certain space of time, and the changes in the microscopic structure of the elements. Thus it was found that, for proteinaceous substances, the dissolving power of the gastric juice of man is far inferior to that of the dog; their solution is completed in the stomach of the dog within two to four hours, in that of man it requires nineteen to twenty hours. Raw meat is better digested by the stomach of man than boiled meat, and veal better than beef.

Concerning the *microscopic alterations*, they found after one hour and a half the primitive fasciculi easily separable from each other, without change in themselves, the sarcolemma destroyed; after two hours and three quarters, the primitive fasciculi commenced to exhibit transverse fissures; after three hours, only transversely striated lamellæ were visible; after three hours and a half, besides the quadratic lamellæ, many primitive fasciculi, fissured in longitudinal and transverse direction, frequently dentated at the extremities; after three hours and three quarters, the primitive fasciculi twice or three times divided in the longitudinal direction; after four hours and a half, scarcely any solid remains left in the stomach, except some few primitive fasciculi much fissured in the longitudinal and transverse direction, still, however, exhibiting the transverse striæ. After three and a half to four hours the stomach is, in general, empty; the proteinaceous substances become therefore subject to the influence of the enteric secretion, &c. As regards the digestion of *fat*, the membrane of the cells was dissolved in the stomach; but the fat itself did not form a coagulum, enclosing a large quantity of milk globules and free fat; after two and a half hours, the casein was seen partly as amorphous substance, partly in transparent, membrane-like pieces, with some unchanged milk globuli; after three hours and three quarters scarcely any remains were left in the stomach.

These alterations, which required in the stomach of this woman three hours and three quarters to four hours and a half, were completed in that of a dog within two hours.

Finally, the authors allude to the great importance of the secretion of the gastric juice for the intermediate circulation of fluids and the intermediate change of matter, the principal object of which is to economise in the animal household, by prolonging the series of changes to which animal matter is submitted within the organism, and thus to render it for a longer time efficient in the discharge of vital processes. We find ourselves more compelled to adopt this view, as the quantity of the secretion amounted in this instance to one-fourth of the weight of the whole body.

3. KROEGER gives in his dissertation the results of some experiments performed with Prof. Bidder. Through a fistula in the pancreatic duct they collected the pancreatic juice during certain spaces of time at various periods of the day and under various influences. Concerning the quantitative relations, 1 kilogramme of dog secretes, on the average, within twenty-four hours, about 89·3 grammes of pancreatic juice. According to this calculation, an adult man (weighing 64 kilogrammes) would secrete, in twenty-four hours, 5·715 kilogrammes, i. e., nearly the eleventh part of the weight of the whole body. The ingestion of food exercises great influence over the secretion, the latter becoming much increased in quantity almost immediately after meals, reaching its maximum within about half to three quarters of an hour after the meal, when it is about six or ten times larger than it had been just before the ingestion of food. Water has not the same effect: on the contrary, when taken simultaneously with solid food, it prevents the latter from causing so evident an increase. The concentration of the pancreatic juice appears frequently diminished in the same measure as the quantity is increased, but this phenomenon is not a constant one; and, at all events, the absolute quantity of solid substances is greater after meals than before.

As regards the physical and chemical character of the secretion the author does in general, agree with Bidder and Schmidt; * some differences, however, must be

* l. c. p. 244.

noticed. The specific gravity is, according to Kroeger, 1.01065, while Bidder and Schmidt had assumed 1.0306, and Frerichs 1.0082.* Further differences become apparent by comparing the following figures relating to 1000 parts of fresh juice.

	Kroeger. (Analysis by C. Schmidt.)	Frerichs.	Bidder & Schmidt.
Water	981.52	988.40	900.76
Solid matter	18.48	13.60	99.24
Organic substance	12.68	3.50	90.38
Inorganic substance	5.80	10.12	8.86

Concerning the *physiological action* of the juice, its power of transforming starch into sugar is not doubted. According to the author's experiments, 1 gramme of the fresh juice transforms within half an hour, under the influence of a temperature of 35° C. 4.672 grammes of dry starch into sugar; as 1 gramme of fresh juice contains 0.014 grammes of pancreatic ferment, one gramme of this ferment would transform 333.7 grammes of dry starch. If we assume, with Frerichs, that an adult man requires daily about 490 grammes (=15 ounces nearly) of starch to compensate the daily loss of carbon, the quantity of pancreatic juice necessary for the transformation of this starch into sugar would be less than 105 grammes, while the quantity actually secreted amounts to more than 5000 grammes. Kroeger is therefore of the same opinion with Bidder and Schmidt—viz., that the pancreatic juice cannot have as its *principal* function the transformation of starch into sugar. As one of the functions, he considers the promotion of the constant interchange of fluids within the body, in the same manner as Bidder and Schmidt have made it so probable concerning the saliva, and with Grunewald and Schroeder concerning the gastric juice. It further appears to him that an intimate connexion exists between the secretion of the stomach and that of the pancreas—namely, that the hydrochloric acid secreted by the former is, after having performed its part, neutralized by the soda of the latter, thus again forming the chloride of sodium previously disunited by the process of secretion. In favour of this theory Kroeger observes that the hydrochloric acid secreted by 1 kilogramme of dog through the gastric juice in twenty-four hours amounts to 0.305 grammes, while that of soda contained in the pancreatic juice of twenty-four hours is calculated at 0.237 grammes, i. e., very nearly the equivalent (0.259) corresponding to 0.305 grammes of hydrochloric acid.

4. VALENTIN examined the gases of various parts of the intestinal canal of horses. The animals were killed by bleeding. Separate portions of the intestinal canal were isolated by double ligatures, and the gases collected immediately after death. We abstain from describing the chemical methods employed, but will construct a table showing the percentic composition of the gases of the two horses examined.

- A. gives the figures belonging to a gelding, 20 years old, healthy, fed with oats and hay. The stomach contained a large quantity of food; the gases of the small intestines were collected from comparatively empty portions; the cæcum was in a great part filled with the remains of food; the gases of the rectum were from between the faecal balls.
- B. was an old mare, healthy, fed with oats. Stomach and cæcum were almost filled with solid masses. The rectum contained so small a portion of gas that no sufficient quantity for an analysis could be collected. The interval between the last meal and the death of the animal is not named.

* Wagner's Handwoerterbuch der Physiologie, III. p. 844.

Names of the gases.	Stomach		Upper por- tion of small intestines, A.	Middle por- tion of small intestines, B.	Lower por- tion of small intestines, A.	Cæcum.		Middle portion of rectum, A.
	A	B				A	B	
Carbonic acid . . .	44.85	55.64	18.83	41.78	19.41	77.70	71.59	47.94
Carbur. hydrogen . . .	0.90	—	0.45	4.98	0.77	4.09	6.96	11.82
Sulphur. hydrogen . . .	2.70	4.92	1.61	4.52	1.46	2.02	3.71	0.54
Hydrogen	0.66	13.29	—	0.02	0.08	4.67	0.20	13.82
Oxygen	7.16	0.77	5.76	—	4.97	—	—	—
Ammonia	—	—	—	—	—	1.29	1.22	1.49
Nitrogen	44.23	25.38	73.35	48.70	73.31	10.23	16.82	24.39
	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

The coincidence of the large proportion of carbonic acid in the stomach and cæcum of both horses is remarkable, a circumstance probably connected with the functions which both organs have to fulfil in the digestive process.

The presence of sulphuretted hydrogen in the gases of all parts of the intestinal canal, makes Valentin conclude that the formation of this gas commences in the stomach. He considers the albuminous substances of the food to be the source of this formation, and draws the inference, that in the stomach not only a mere solution of some of the substances containing nitrogen and sulphur takes place, but that there is also a partial decomposition. It must, however, as yet remain uncertain how much of the sulphuretted hydrogen found in the stomach is due to a development of this gas in the stomach itself, and how much may have been introduced from lower parts by the diffusion of gases. Ammonia, it will be seen, on the other side, is met with only in the large intestines. The proportion of hydrogen found by Valentin is smaller (except in the rectum) than that stated by former observers. He agrees, however, with them concerning the total absence of oxygen in the gases of the large intestines. The larger quantities of carburetted hydrogen and hydrogen in the rectum show that changes in the remains of food continue taking place up to the last portions of the digestive tube.

II. BLOOD, RESPIRATION, CIRCULATION.

1. *Contributions to the Physiology of the Blood.* By Prof. VIERORDT. (Vierordt's Archiv, xiii. 3, pp. 409. 1854.)
2. *On the Admixture of Ammonia in the Expired Air* (De l'Ammoniac dans la Respiration). By Profs. VIALE and LATINI. (l'Union Medicale, viii., No. 98, Août. 1854.)
3. *On the Admixture of Ammonia in the Expired Air.* By Dr. REULING. Dissert. (Giessen. 1854.)
4. *Lithic Acid in the Tissue of the Lungs.* By Dr. CLOETTA. (Virchow's Archiv, vii. 1; p. 168. 1854.)
5. *On the Action of the Heart in Various Gases.* By T. CASTELL. (A Successful Prize Essay at the University of Königsberg. Müller's Archiv, 1854, iii. pp. 226, ss.)
6. *Observations on the Body of a Decapitated Criminal.* By VIRCHOW, KÖLLIKER, and others. (Verhandlungen der Würzburger Gesellschaft, vi. pp. 116. 1854.)
7. *Remarks on the Action of the Heart.* By Prof. VIERORDT. (Appendix to the article above named—"Contributions," &c.)

1. VIERORDT counted the blood globules in the blood of a marmot at various periods during the state of hybernation. Commencement of hybernation on November 22, 1853, when the weight of the animal was 845 grammes.

I. A few drops of blood were taken from the subcutaneous cellular tissue (the bloodvessels of which were remarkably full) on November 28th. The blood is of dark colour, coagulates quickly; number of blood globules in one cubic millimetre = 5.748.000. (Weight of body not determined.)

II. On January 5th, 1854, weight of body=750 grammes. Blood does not so quickly coagulate; is of much lighter colour than it was on November 28th; number of globules in one cubic millimetre=5·100·000.

III. On February 4th, weight 613 grammes; the subcutaneous cellular tissue contains so little blood that the few drops for the examination must be taken from the jugular vein; the colour of the blood is very pale, the coagulation slow; number of globules=2·355·000. In consequence of these experiments, the little animal lost about six to seven grammes of blood, became extremely feeble, died on 18th February. Weight, eighteen hours after death, 537 grammes; number of globules 3·665·000. The gradual decrease of the weight of body, of the quantity and concentration of the blood, are very evident in these experiments. It is to be expected that further observations promised by Vierordt on the same subject will considerably add to our knowledge of the physiology of the blood and respiration.

Vierordt made also some observations concerning the proportion of the *coloured* to the *colourless globules* in the blood of the splenic vein of man. As the colourless globules possess a great tendency to join in heaps, this examination cannot be made so easily on man as on animals. Vierordt had the advantage of taking the blood of a decapitated criminal one hour and a half after death. The mean numbers of various countings give the figures 1 : 4·9 as the proportion of the colourless to the coloured corpuscles.

2. VIALE and LATINI, of Rome, are led, by their experiments, to the following inferences: 1. An emission of ammonia takes place in the act of expiration (0·763·026 grammes within twenty-four hours; 278·504·490 grammes within a year—which would make, in a town of 160,000 inhabitants, 44·560 kilogrammes). 2. The ammonia is then in the state of the sesquicarbonate. 3. The nitrogen mentioned by the chemists as a product of respiration, is only contained in the ammonia. 4. The ammonia developed in this way serves in a considerable degree to renew that which enters into the composition of the atmospheric air, and which, dissolved by rain-water, renders the earth fertile. 5. The contagious principle is, perhaps, nothing else but a salt of ammonia. 6. During the respiration an excretion of a certain quantity of albumen takes place which one might consider as ferment.

3. Dr. REULING draws from his observations a conclusion very different to those just given, viz., that in the lungs of healthy persons ammonia is rather absorbed than developed. According to him the quantity of ammonia contained in the air of a healthy person, *expired* within twenty-four hours, amounts only to 18·72 milligrammes, which would be not more than 1·82 to 1,000,000 parts (by weight), while the same quantity of *atmospheric* air contains not less than 15·63 parts of ammonia. (See the Report on Medicine.)

4. CLOETTA considers the presence of lithic acid in the lungs of herbivora as a normal phenomenon. He examined six ox-lungs, the aqueous extract of every one of which contained lithic acid. The following is the process adopted:—The minced lungs were exposed to the influence of distilled water during twenty-four hours. After this was as much as possible squeezed out, the albumen and colouring matter were precipitated by boiling, the fluid filtered, and baryta added until precipitation did not take place any longer. The fluid was filtered again, and placed on the water-bath, until it amounted to not more than 50 c.c. when the addition of acetic acid precipitated lithic acid crystals. The watery extract of the whole lung of an ox yielded 60 milligrammes of lithic acid.

5. CASTELL performed his experiments on the hearts of frogs, which he placed, immediately after their separation from the body, in glass cylinders filled with various gases. Temperature, 68°—77° F. The following table shows the average time during which the heart continued to move after having been exposed to the

influence of the respective gases. The O attached to a line means that the heart, when taken out of the gas and exposed to the atmospheric air, did not commence again to move; the — signifies that the atmospheric air excited traces of muscular action; the + that it effected regular contractions.

The heart of the frog continues to act in the atmospheric air . . .		during 180 min.
"	"	under the air-pump " 30 min.
"	"	under water " 20 min. +.
"	"	in oxygen " 720 min.
"	"	in hydrogen (moist*) " 72 min. +.
"	"	in nitrogen " 68 min. +.
"	"	mixture of 2 nitro. and 1 carb. acid " 67 min. +.
"	"	hydrogen (dry*) " 47 min. O.
"	"	carbonic oxide " 44 min. O.
"	"	phosphoretted hydrogen " 27 min. O.
"	"	sulphuretted hydrogen " 12 min. +.
"	"	carbonic acid " 6 min. +.
"	"	protoxide of nitrogen " 5 min. —.
"	"	cyanogen " 4 min. O.
"	"	sulphuric acid " 3 min. —.
"	"	olefiant gas " 2 min. O.
"	"	chlorine " 2 min. O.
"	"	mixture of 4 nitrogen & 1 chlorine " 2 min. O.
"	"	iodine " 0 min. O.

The preceding table shows that the heart, in all the gases tried, with the exception of oxygen, ceases beating sooner than in the atmospheric air. The author concludes from this, that it is the presence of the oxygen in the atmospheric air by which the continuation of the action of the heart is rendered possible for so long a period after its separation from the body; while it is the want of oxygen to which the ceasing of the action is to be attributed. Not one of the gases which contain no free oxygen is able to keep up the action of the heart. The influence exercised by the single gases is apparently very different, some of them acting only by the want of free oxygen (the heart commences beating again, when soon after exposed to the atmospheric air); others destroying the irritability of the fibres of the heart, either by over-irritation, or by too strong an affinity to the elements of the heart (neither the atmospheric air nor any stimulants effect the re-appearance of action).

6. KÖLLIKER and VIRCHOW opened the pericardium of a decapitated criminal thirty-five minutes after death; the pericardium contained a small quantity of pale yellow transparent fluid, which formed soon after exposure to the air, a rather firm coagulum. Irritation (by electricity) of the sympathicus on the neck did not produce any action of the heart, but irritation of the substance of the heart itself excited local contractions. The aorta abdominalis contracted from sixteen to one millimetre; the vena meseraica, near the ilium, contracted about one-fourth of its lumen. A piece cut out of the *vena cava infer.* 39 millimetres long (sixty minutes after death), shortened by continued application of the stimulus to the length of 25 millimetres, forming many transverse wrinkles of the internal membrane. A piece from the vena portæ (after ninety-three minutes) contracted from 20 to 18 millimetres. The small pulmonary veins show decided contraction after fifty-five minutes.

7. VIERORDT examined the action of the heart of a decapitated criminal by his instrument for the examination of the physical phenomena connected with pulsation (*pulsmaschine*). The heart was taken out immediately after death, and placed under the instrument with the right ventricle. The experiment commenced nine minutes after death.

* Moist hydrogen means hydrogen mixed with some vapour of water, through the process of development in distinction from dry hydrogen, which had been led through a tube containing chloride of calcium.

The average duration of the systole of the ventricles to that of the diastole was shown to be in the proportion of 10 to 65; the duration of the systole is therefore shorter than it appears to be to the eye.

Thirty-nine movements of the heart (each movement comprising one systole and one diastole) were performed in sixty-four seconds. The contractions following these were too indistinct to permit of being marked on the kymographion.

The duration of each movement is expressed in millimetres, by the following numbers :

I.	7.5	7.4	8.6	8.2	8.8	9.2	7.0	8.5	7.9	9.9
XI.	9.1	9.3	8.7	8.6	8.2	8.1	8.3	8.4	8.9	8.1
XXI.	17.0	9.0	5.2	12.1	9.7	8.8	18.2	9.9	9.9	10.0
XXXI.	10.2	10.0	24.6	10.1	9.2	18.6	11.8	10.7	10.0	

III. LYMPHATIC SYSTEM AND DUCTLESS GLANDS.

1. *Observations on the Body of a Decapitated Criminal.* By VIRCHOW, KÖLLIKER, &c. (Würzburg Gesellschaft, vi. 1854.)

(See also, under II., "Contributions to the Physiology of the Blood." By VIERORDT. On the proportion of the *coloured* to the *colourless globules* in the blood of the splenic vein.)

VIRCHOW produced, by application of electricity to the ductus thoracicus, fifty-five minutes after death, intense contraction, continuous, not peristaltic. The contraction had, fifty minutes later, not altogether ceased. Several hours after death the irritability of the ductus thoracicus had not yet disappeared; therefore it remains longer in this than in any other organ. Also the small chyliferous vessels showed energetic contraction when stimulated by electricity, but here again the contraction was continuous, not peristaltic.

The *spleen* did not appear to contract, either in the longitudinal or transverse diameter, by the application of electricity; but a powerful stream made the impressions (corresponding to the insertion of the trabeculæ to the tunica), appear deeper than before. Another interesting phenomenon was a gradual change in shape. Soon after the abdominal cavity had been opened, the spleen was 95 millimetres long, and 68 or 69 millimetres broad; about half an hour later (seventy-two minutes after death), length, 102 millimetres; breadth, 65 millimetres; one hundred and eight minutes after death, the organ having been previously placed in tepid water, the length was 109 millimetres; breadth, 65.

IV. ORGANS OF SECRETION AND EXCRETION. SKIN.

1. *Contributions to the Knowledge of the Secretion of Urine in Healthy, Pregnant, and Diseased Persons; especially on the Quantitative Relations of the Phosphates.* By Dr. MOSLER. (Dissert. Giessen, 1853.)
2. *Contributions to Urology, II.* By Dr. BENEKE. (Archiv des Vereins für gemein: Arbeiten, No. 4, pp. 608. 1854.)
3. *Hippuric Acid in the Urine of Man.* By Dr. DUCHEK. (Prag. Viertelj. xi. 3. 1854.)
4. (GRUNEWALD and SCHROEDER, "On the Gastric Juice." KROEGER, "On the Pancreatic Juice." See I.)
5. *On the Immediate Principles of the Excretions of Man and Animals in the Healthy Condition.* By WILLIAM MARCET, M.D. (Proceedings of the Royal Society and "Medical Times and Gazette." Sept.)

1. MOSLER made a series of observations partly on himself, partly on several other healthy persons. For the information concerning the individual circumstances of the subjects of examination, as also concerning the methods of analysis,

we must refer to the work itself. The average quantity of *phosphoric acid* secreted within twenty-four hours by a healthy man is calculated at 3·209 grammes; the acid contained in the alkaline phosphates to that in the earthy phosphates bears the proportion of 3 : 1. The quantity of colouring matter within twenty-four hours is about eight grammes. As regards the various periods of the day the secretion of phosphoric acid was greatest in the evening, when the author was, in general, mentally engaged; after this follow in a descending series, noon, night, and morning. By intense mental occupation the quantity of phosphates excreted became regularly increased one-half, the increase relating more to the earthy than the alkaline phosphates; the quantity of colouring matter became likewise greater. By an abundance of proteinaceous food, the excretion of phosphates became in a similar proportion larger. The effect of both influences combined on the excretion of phosphoric acid, urea, and chlorine, is striking in the following table under A, while the figures found in normal circumstances are placed sub B.

	Urea.	Chlorine.	Phosphoric acid.
A	43·050 grammes.	20·720 grammes.	5·104 grammes.
B	29·120 ,,	7·010 ,,	4·479 ,,

By fasting, the excretion of phosphoric acid was more diminished than that of urea, the quantity of chlorine in the urine remaining unaltered. The examination of a non-pregnant female, extending over five days, gave much lower figures for the phosphates, than of men under similar circumstances. Concerning the urine of pregnant females, the author found, as the mean figures of several examinations for women of 128 pounds' weight, after the sixth month of pregnancy: quantity of twenty-four hours = 1·488 C.C. (62 C.C. per hour); specific gravity = 1·011; reaction more frequently neutral or alkaline than acid; colouring matter 14·592 grammes (0·608 per hour); urea = 26·193 grammes (1·090 per hour); chlorine = 7·930 grammes (0·330 per hour); phosphoric acid = 2·422 grammes (0·100 per hour); sulphuric acid = 1·250 grammes (0·052 per hour).

2. **BENEKE** dwells principally on circumstances connected with the quantity of urine, its specific gravity, and the amount of phosphoric and sulphuric acid contained in it. We must refer, as regards the manner of observation, &c., to the paper itself, restricting ourselves to the principal points discussed in it.

I. Concerning the *quantity* passed in twenty-four hours, he says that it is in some instances larger than the quantity of fluids taken as *drink*, in others smaller; in the former case the excess of the fluid in the urine is, according to all the observations made on man and animals (Bischoff,* C. Schmidt,† &c.), derived either from the fluids contained in the solid food, or from the organs of the body itself; this latter circumstance being almost always connected either with diminished activity of the skin, or with an irritated state of the nervous system. The *quality* is of great importance on the quantity of urine excreted within a certain space of time; abundance of meat produces, as well in man as in animals, considerable increase, which Dr. Beneke is inclined to attribute to its stimulating action on the nervous system. He alludes to another circumstance of importance connected with the quality, the *endosmotic equivalent*. We know, for instance, that each equivalent of sugar entering into the blood assumes for itself seven equivalents of water; that albuminous substances do not require quite as much; that chloride of sodium has a much higher equivalent: by the introduction and excretion of bodies with a high equivalent the body may be deprived of a large quantity of fluids.

II. Concerning the *specific gravity*, and the excretions of solid matters with the urine, the author reminds us that the quantity of water is not necessarily proportionate to the quantity of solids contained in the urine. As regards the various periods of

* Bidder and Schmidt, *Verdaunung u. Stoffwechsel*, 1852.

† Bischoff, *der Harnstoff als Maass d. Stoffwechsel*, 1853.

the day he agrees with Vogel, that the quantity of solid substances excreted per hour is greatest during the afternoon and the evening (after food), smaller during the night and the morning. Beneke draws our attention to the importance of observations on the influence of temperature on the excretion of solid substances, respecting metamorphosis of matter. Summing up our knowledge of the action of the various points in question, he says, "Increase of the ingestion of water increases the egestion of water; increased ingestion of solids produces an increased egestion of solids (afternoon-urine). As yet it remains undecided whether an increased ingestion of water alone causes an increased egestion of solids, but it appears to be certain that an increased ingestion of solids does not necessarily effect an increased egestion of water, unless the *quality* of the food (meat, &c.) be acting as an excitant on the nervous system (Bischoff); influences exercising a depressing action on the nervous system effect a decrease of the excretion as well of solids as of water; influences of an exciting character have the opposite effect; as regards the *solids*, the change does *always* appear in the urine; as regards the *water*, it does not *necessarily* so, as there are various ways for the elimination of water.

III. *Phosphoric acid*. The average quantity of twenty-four hours, in one series of observations, was 2.946 grammes, in another 2.183 grammes, which is rather less than Winter* and Mosler† had found. The proportion of the acid combined with alkalies to that combined with earths, was in the average = 2.118 : 0.663 (determined only in the first series). Concerning the distribution of the excretion over the single periods of the day, the highest figures belong to the afternoon and evening, the lowest to the morning (dinner taken early in the afternoon).

IV. The quantity of *sulphuric acid* of twenty-four hours ranged between 1.8404 and 2.819 grammes, which is quite in accordance with Gruner's‡ observations. Here again we find the highest figures for each hour in the later part of the afternoon and evening (0.0866 to 0.1269 grammes), the lowest in the morning (0.0321 grammes).

3. DUCHEK found hippuric acid in his urine whenever he had taken green prunes (the ripe fruit of *prunus domestica*—var. *chlorocarpa*). The analysis of these prunes exhibited a considerable quantity of benzoic acid. The hippuric acid began to appear about seven to eight hours after he had eaten the prunes, and ceased to appear from three to five hours later. Supposing that hippuric acid is a conjugate acid, consisting of benzoic acid and glycocoll, the author made some experiments concerning the quantity of glycocoll which the healthy human organism can yield under the influence of a merely animal diet. He examined, therefore, the urine after having taken weighed quantities of benzoic acid. The ingestion of 1 gramme of benzoic acid was followed by the excretion of 0.714 grammes of hippuric acid; 2 grammes of benzoic acid by 1.857 grammes of hippuric acid and 0.421 grammes of benzoic acid; 4 grammes of benzoic acid by 1.714 grammes of hippuric acid, and 2.500 of benzoic acid. Duchek concludes from this, that within a fixed space of time the organism can dispose only of a certain amount of glycocoll, which must be either detracted from the fabrication of bile or exist in superabundance. If the former supposition were correct, the fabrication of bile would be impaired by taking benzoic acid; while, according to the other supposition, it would act as an indifferent body; the author is inclined to adopt the latter view, as the ingestion of larger quantities of benzoic acid appears to be without any influence on the state of health.

4. A very interesting series of observations has been made by Dr. MARCET on excrement. He has discovered a new immediate principle in the fæces of man which he terms excretine. This principle crystallizes out of alcohol or ether in

* Bestimmung d. Phosphate u. d. freien Säure im Urin. Giessen, 1852.

† Analyzed just before.

‡ Die Ausscheidung d. Schwefelsäure durch d. Harn. Giessen, 1852.

beautiful four-sided prisms; its exact composition has not been made out, but it contains both nitrogen and sulphur though in small proportions. Besides excretine, Dr. Marcet finds margaric acid, colouring matter similar to that of blood and urine, and two other organic substances, one of which is probably a mixture of bodies and the other is a fatty acid which it is proposed to call excretoleic acid in human fæces. He has found no butyric or lactic acid.

In animals fed on meat (dog, leopard, tiger), a substance analogous to, but not identical with, excretine was found. Butyric acid was also discovered.

The fæces of herbivorous animals (horse, sheep, dog, when fed on bread) contained no excretine, no butyric acid, and no cholesterine.

The excrements of the crocodile contained cholesterine but no uric acid; those of the boa, uric acid, but no cholesterine.

The report before us does not mention what amount of excretine is contained in human fæces, and whether it constitutes the bulk of them. We shall, however, return to the paper when the 'Transactions of the Society' are published.

V. NERVOUS SYSTEM.

1. *De Actione Belladonnæ in Iridem.* By G. C. P. DE RUITER. (Dissert. Trajecti ad Rhenam. 1853.)
2. *On the Action of the Nervous Splanchnicus Major.* By Dr. HAFETER. (Dissert. Zurich. 1853.)
3. *Observations on a Decapitated Criminal.* By Drs. MÜLLER, LEYDIG, &c. (Verhandlungen der Würzburg Gesellschaft, vi. pp. 116, ss. 1854.)

1. RUITER considers the action of the iris as twofold: 1, *reflex*, by the rays of light acting on the retina, and only through this on the iris; not adopting the view of Harliss* and Budge,† that the light acts directly on the iris; 2, *voluntary*, for the accurate examination of near objects. Ruiter calls this voluntary, because we are conscious of the effect of the contraction, although we are not conscious of the contraction itself. Ruiter confirms the experiments of Budge, Waller, and Wagner, that the pupil becomes immediately smaller by section of the nervus sympathicus (in frogs and rabbits), and of the nervus vagus (in dogs) of the same side. In rabbits and frogs the proportion of the dimension of the pupil of the side operated upon to that of the other eye, was as 2:3; in dogs as 1:3. Several days after the section the difference was still visible, but not so great. Ruiter considers, therefore, the sympathicus nerve as the nervous *dilatator* of the pupil. He agrees with Budge, in opposition to Volkmann and E. Weber, concerning the supposition that the nervus oculomotorius regulates the action of the sphincter pupillæ; on the ground as well of the physiological experiment as also of the pathological fact, that paralysis of the nervus oculomotorius is always accompanied by dilatation of the pupil. To the fifth pair he attributes the sensibility of the iris; but, besides the sensitive functions, he is inclined to ascribe to it some motor influence on the iris, as section of this nerve on the base of the brain, and also irritation of the nervus ophthalmicus Willisii (Budge, Waller,) are followed by some contraction of the pupil, which, however, is less considerable and of shorter duration than that produced by section of the sympathicus. As regards the *modus operandi* of the atropia, we restrain ourselves to the two following inferences of the author: 1. that it diminishes the energy of the musculus sphincter pupillæ (scil., filaments of the nervus oculomotor); 2. that it does not diminish, but probably increases the action of the musculus dilatator pupillæ (scil., filaments of the nervus sympathicus).

2. H. MÜLLER applied a powerful stream of electricity to the nervus sympathicus of the left side, near the ganglion cervicale supremum (about forty minutes

* Die Muskelirritabilität. München, 1850.

† Comptes Rendus, tom. xxxv. p. 561.

after the decapitation, the head being separated at the sixth cervical vertebra); he observed immediately a considerable contraction, less, however, than can be produced by belladonna; the dilatation lasted as long as the irritation was continued; as soon as the latter ceased, the pupil returned to its normal size, but less rapidly than the dilatation had taken place. There was no difference in the effect whether the stream was applied to the upper or lower extremity of the ganglion. The pupil of the other side remained unchanged.

Irritation of the nervus vagus did not influence the pupil. Irritation of the nervus oculomotorius, ninety minutes after death, was likewise without effect on the pupil.

LEYDIG could not detect ciliar action in the lateral ventricles of the brain (as Valentin and Purkyni had done), but he found it very distinctly in the fourth ventricle; the cells were of a round rather flat shape, with long cilia. By the application of liquor potassæ, the action of the cilia was once more reproduced after it had already ceased.

3. HAFSTER, in connexion with Professor Ludwig, divided, in five cats, the nervus splanchnicus externally to the peritoneum. Concerning the *sensitive* function, it appears to contain a large amount of sensitive fibres, as the signs of pain produced by the section of the nerve are as great as those by the section of an equally thick branch of the trigeminus. The perception of *hunger* was not destroyed after the operation; as the section of the *vagus* does likewise not annihilate it (Bidder and Schmidt), Haffter concludes that it depends on the combined action of the nervus vagus and nervus splanchnicus. As regards the *motor* function, the author is led to conclude from his experiments that the nervus splanchnicus neither *excites* the motion of the intestines, nor possesses the power of *arresting* it as the vagus that of the heart. The *secreting* function of the intestines became only slightly altered; the secretion of the stomach, however, and of the upper part of the small intestines, seemed to be rather increased. The nutrition, as well as the secretion of the kidneys unimpaired. The colour of the liver unusually dark.

VI. ORGANS OF SENSES.

1. *Contributions to the Physiology of the Sense of Sight.* By Dr. L. FICK. (Müller's Archiv, 1854, iii.)
2. *On the Accommodation of the Eye.* By Dr. CZERMAK. (Prag. Vierteljahrs. xi, 3. pp. 109.)
3. *Observations on a Decapitated Criminal.* (Verhandl. der Würzburg Gesellschaft, vi. pp. 116. 1854. Compare also No. 1 and 3, sub. v.)

1. L. FICK considers the question—"Why do we see all objects as they are, and all parts of objects in their correct relation to each other, i. e., right, where is right? left, where is left? above, which is above, &c.? while it is an undeniable fact that an inverted image is thrown on the retina?" None of the explanations as yet given appear satisfactory to the author. Before giving his own, he reminds us that the soul forms the perception of objects by means of the impressions conveyed to the brain from the periphery through the nerves, the central ends of which must be arranged in such a manner as to produce impressions accurately corresponding to the external objects. His explanation is then that the central ends of the optic nerve are implanted into the brain in a manner perfectly opposite to the arrangement of the peripheric ends in the retina.

2. CZERMAK agrees with Cramer and Helmholtz that the interior changes in the eye during the process of accommodation for near objects, consist in the increase of convexity of the anterior surface of the lens, while the convexity becomes diminished during the accommodation for distant objects. That the lens does not at all change its place, is proved by the fact that during the accommoda-

tion the first and third of the three little images in the eye, i. e., that caused by the corner and that caused by the posterior surface of the lens, do not alter either in position or in size, while the image, during the accommodation for near objects, advances and becomes smaller. Concerning the means by which this change in the shape of the lens is effected, Czermak again agrees with Cramer in finding it in the iris and tensor choroidæ. Irritation of the iris by electro-magnetism produces the alteration in question; the same irritation effects no change as soon as the iris is cut from the pupillar margin to the periphery. Czermak is, however, of opinion that the iris is assisted in action on the shape of the lens by the *uvea*. In a former volume of this journal* we have mentioned Fick's view, that the *uvea* is the apparatus which effects, besides the regulation of the quantity of light, the accommodation for near and remote objects, by means of a change of the position and form of the lens, placing varying quantities of blood now before then behind the lens. Czermak considers Fick's view by itself as insufficient to explain the process; a varying quantity of blood, he reasons, would be necessary. Cramer experimented, however, on eyes just cut out, removed therefore from the circulation and the possibility of a change in the quantity of blood. Czermak adopts only that part of Fick's view by which is assumed that, through the filling and emptying of the processes ciliares the shape of the lens becomes changed in the way described.

3. Leydig, H. Müller, and Gegenbaur found ciliar action on all parts of the nasal cavity, also on the regio olfactoria of the ethmoidal bone. A distinct direction in the movement to either side could not be detected. Kölliker discovered the presence of *cavernous* tissue in the inferior concha with organic muscular fibres and manifest contraction under the influence of electricity.

Kölliker did not detect any ciliary epithelium on the membrane tympana, but he found it over the promontorium and most other parts of the tympanum.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.†

By E. A. PARKES, M.D.,

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1. *On the Occurrence of Leucin and Tyrosin in the Human Liver.* By Professor F. T. FRERICHS and G. STAEDLER. (Müller's Archiv, Heft 4. 1854.)
The same. By Professor FRERICHS. (Correspondenz Blatt des Vereins für weiss. Heilk. und Wien Méd. Wochenschrift, 1854, No. 30.)

In 1851, Professor Frerichs observed in a liver affected with acute yellow atrophy, a number of needle-formed crystals mixed with destroyed liver cells; they were also present in the blood in the hepatic vein. They were not sufficiently numerous to permit an analysis to be made, and in other livers they were looked for in vain. In 1853, in the liver of a woman who died comatose with closure of the ductus communis choledochus, similar crystals were found in great numbers. The liver was now treated in a manner described fully by the writer, but which is too long to insert here, and both leucin and tyrosin were unequivocally proved to exist.

The authors then discuss, at some length, the alliances of tyrosin to the salicylic group, and give reasons for supposing tyrosin to contain the elements of glycin and saligenin with two equivalents of water, but from want of material they leave the subject only partly explored; but they remark, that in the urine of

* No. 27, p. 257.

† We have been obliged to defer several analyses of long foreign papers till our next number, and this has made the Report shorter than usual. As the number of papers analyzed is so few, we have not attempted any arrangement of them.

beasts and man, compounds derived from this group of bodies can be found. Thus, they allude to the occurrence of carbolic acid in the urine of herbivoræ, and to the observation of Sicherer (Hassall?), that indigo can be obtained from the urine of men. They think that these bodies may be derived from saligenen formed in the decomposition of tyrosin in the liver, the glycin then set free being used in the formation of glycocolic acid.

With reference to leucin they remark, that its oxidation furnishes valerianic and butyric acid; but, as they observe, these substances are also derived from the oxidation of fat and other substances.

The authors then state, that they believe leucin and tyrosin to be formed very early in the body, and to be destroyed in the liver. In healthy livers they have never found them; whereas, when the action of the liver is destroyed, they are found in abundance. With respect to the supposition that the leucin and tyrosin might be found before or shortly after death, by decomposition of protein substances, they observe that the livers they examined had no trace of decomposition, and that if so formed, the quantity of tyrosin would have been small, instead of being so great as in the last case referred to.

Both bodies have been found in the liver of variola and typhus patients (typhus abdominalis). In these patients, also, leucin and valerianic acid were found in the urine.

As in variola and typhus there are similar symptoms of nervous commotion as in the cholæmia of the acute yellow atrophy, the authors conjectured that the cause might in all cases be the same—viz., the presence of leucin and tyrosin in the blood; but on injecting both bodies, and also valerianate of ammonia, into the blood, no poisonous effect was produced. The leucin was easily found again in the blood, but not so the tyrosin—it was perhaps destroyed in the liver.

In a note it is observed, that Dr. Valentiner some time ago discovered leucin in the urine of an epileptic patient.

The views of Professor Frerichs, as given in the second work quoted above, although in part a recapitulation of what has now been said, are of sufficient importance to lead us to translate them:

“1. In the acute atrophy and softening of the liver, large quantities of leucin and tyrosin form in this organ, and pass from thence in part into the blood.

“2. The cholæmic intoxication is not the consequence of the accumulation of bile in the blood, but occurs in connexion with the formation of this product of albuminous decomposition in the liver.

“We have observed this intoxication only when many of the crystals of leucin have been separated in the parenchyma of the liver, and in the hepatic veins. The injection of great quantities of filtered bile never produced disturbance of sensorial activity. The bile entirely disappeared; once only when a concentrated solution of pure bile-acids and soda was injected in very great quantity, could remains of unchanged bile be found in the urine. I have vainly sought to know the various intermediate products which are formed before the bile is perfectly destroyed, nor have I discovered where the Taurin, a body which changes with so much difficulty, remains. At present this inquiry is being pursued in concert with Dr. Baumert.

“3. In typhus and exanthematous diseases, small-pox, &c., leucin and tyrosin form in the liver.

“4. The liver in this way contributes essentially to the origin of the blood-alteration, through which these pathological processes are characterized.

“5. Leucin and tyrosin are found in the blood of typhus and variola patients; they pass partly unchanged, partly decomposed, in the secretions and excretions, and are in this way separated.

“6. The formerly hypothetically received abnormal composition of the blood in these diseases has received in this way actual proof. The nitrogenous organic matters suffer here, in fact, in part, another metamorphose than the customary one, which ends in the production of urea.”

2. *On the Employment of Hydrochloric Acid as a Means of Discovering the Contamination of the Blood by Carbonate of Ammonia.* By Dr. METTENHEIMER. (Archiv des Vereins für wiss. Heilkunde, Band i., Heft 4.)

On the Ammonia Constituent of the Expired Air, with reference to Uræmia. By W. REULING. (Inaug. Dissert. Giessen, 1854.)

METTENHEIMER observes that the breath of many quite healthy persons produces as much cloudiness about a rod dipped in hydrochloric acid as the breath of an uræmic patient, and that from this test nothing can be decided. This is the same conclusion as that to which Guterback and Schottin* had already come.

This question has been investigated most perfectly by Reuling, who, by means of delicate reagents and a particular apparatus, has absolutely proved that the breath of all men, under the most various conditions, contains ammonia. The quantity, however, thus appearing in the breath is considerably less than that normally contained in the atmosphere, so that there is no proof that the ammonia is given off by the pulmonary mucous membrane, and that it is not merely a portion of that which had been the moment before inhaled. It might, indeed, be rather supposed that it is absorbed from the atmosphere; but Reuling, on careful investigation, does not adopt this idea, as he found no ammonia in the healthy blood when it was first removed from the body, although ammonia soon appears in it.

With respect to a test for ammonia in clinical investigations, Reuling objects, on many grounds, to the rude criterion of a rod dipped in hydrochloric acid. The reagent he employs is prepared as follows: A tincture is made of four to eight drachms of logwood, four ounces of alcohol, and twelve ounces of water, and is mixed with sixteen to twenty grains of alum (free from iron), and with three to four drachms of fused chloride of calcium, dissolved in water, and acidulated with diluted hydrochloric acid to such an extent, as to give a feeble redness to litmus-paper. A few drops of diluted hydrochloric acid are added to the solution, and then paper which has been freed from iron and lime by washing with hydrochloric acid and distilled water, is wetted with it. The paper should take a cherry-red colour; if it is purple-red, enough hydrochloric acid has not been added to the solution; if, on the other hand, too much acid is added, the paper takes a citron-yellow colour. When the proper colour has been given, the paper is quickly dried near a warm oven, and is then placed in bottles, which are to be hermetically sealed, and kept from the light. Ammonia in the air gives the paper a blue tint, and so delicate is the test, that a fluid which holds only $\frac{1}{100000}$ of free ammonia produces the reaction, if the paper be suspended over it. By its means, Reuling has found ammonia in distilled water, vinegar, &c.

The breath has been examined in a great number of diseases, but we shall give merely the principal results. The amount of ammonia in it was found to be increased when the teeth were carious, when there was angina and abscess in the tonsils: in a case of typhus (abdominalis) with albuminous urine (while in 14 other cases there was no increase); in 2 cases of pyæmia in dogs (the pyæmia being artificially produced); in two dogs in whom the kidneys were excoised; in 3 cases of uræmia in men (while in a fourth well-marked case there was no increase); in a case of uræmia, produced by blennorrhæa of the bladder, with healthy kidneys. The author thinks, also, that, most probably, it is increased in cholera and in scarlatina.

The expired ammonia was *not* increased in scorbutus, icterus, syphilis, variola, measles, intermittents, carcinoma, dropsy, acute and chronic bronchitis, œdema pulmonum, pneumonia and pleurisy, empyema, tuberculosis, acute gastro-intestinal catarrh, chronic ulcer of stomach, dysentery, peritonitis, epilepsy, neoplasma cerebri, and cerebral and spinal commotion.

With respect especially to uræmia, the author's own words are: "The increase of the ammonia-constituents of the expired air occurs most frequently in uræmia, but is no pathognomonic sign of this disease. The appearance of ammonia in the

* See No. 23, p. 268.

blood is, indeed, the most frequent, but is not the only cause of uræmia. It would also appear that, in sensitive individuals, uræmia can arise when, from impeded renal excretion, extractive matters accumulate in the blood. . . . Uræmia must be cut out from the list of independent diseases, because it exhibits no essential peculiarities in the nervous phenomena which are observed so frequently in inflammatory fevers, acute exanthemata, typhus, and many poisonings."— (p. 49, 50.)

3. *On Paracentesis of the Pericardium.* By Drs. TROUSSEAU and LASEGNE. (Archives Générales, Nov.)

M. TROUSSEAU records the following interesting case: A man, æt 16, was admitted on the fifth day of an acute disease, with severe frontal headache, lassitude, and præcordial pain. There was intense dyspnœa, a little cough, a very quick pulse, prominence in the cardiac region, increased percussion dulness at the same point, extending to the second rib, and to the right of the sternum; the heart's sounds were feeble and distant. No mention is made of rheumatism, and it is to be presumed that it was absent. For a month the effusion continued the same; only once, for two days, did it seem to diminish, and then there was a little friction at the base. Afterwards, the dyspnœa increased and the fluid augmented as the dulness now reached to the clavicle. There was also pleuritic effusion. It was determined to puncture the pericardium: this was done by an incision in the fifth intercostal space, three centimetres ($= 1\frac{1}{4}$ inch) from the sternum; thirteen ounces of fluid slowly escaped, the cardiac dulness decreased in amount, respiration could be heard in the lung as low as the fourth rib. A day or two afterwards, the pleuritic effusion was found to have increased; the heart was displaced to the right. Thoracentesis was practised in the sixth space in the axillary line, and although at first the canula was blocked-up by false membrane, eventually sixteen ounces of fluid were removed. Neither pleural nor pericardiac effusion reappeared, but, soon after, signs of tuberculosis of the left lung came on.

The authors refer to the other (7) cases of paracentesis pericardii on record.

4. *On Calculous Phthisis.* By Dr. FORGET. (Revue Méd. Chir. de Paris, Oct.)

BAYLE gave the name of phthisis calculosa or cretacea to those cases of consumption in which there were cretaceous, ossiform concretions in the lungs. Later writers have looked upon these as the remains of tubercle, and M. Forget does not dissent from this view. He asserts, however, that there is in addition a form of phthisis in which such concretions are primitive, i. e., do not follow tuberculous exudation. He relates two cases with the physical and general symptoms of phthisis, in which an osseoid mass being coughed up, one patient got perfectly well, and the state of the other was materially benefited. A third case is related, in which a girl having died of variola, a cretaceous mass was found in the apex of one lung, without trace of tubercle round it. On these three (and, we must say, imperfectly recorded) cases, the author bases the following conclusions:

1. The pulmonary calculi may be primitive, *sui generis*, that is to say, independent of tubercles, or of inhalation of dust, &c.
2. The calculi may be solitary.
3. They may remain latent in the lungs for a long time.
4. They can cause the same symptoms as tuberculous phthisis.
5. Phthisis calculosa can be entirely cured by the expulsion of the calculi.
6. Phthisis calculosa is a special malady differing from tuberculous phthisis by its anatomical characters, and by its terminations.

5. *On the Occurrence of Allantoin in Urine in cases of Impeded Respiration.* By Profs. FRERICHS and STAEDLER. (Müller Archiv, 1854. Heft 4.)

The writers desired to test the assertion of Alvaro Reynoso, that sugar appears in the urine when the respiration is much interfered with, but found, as others

have found,* that, although sometimes there was a doubtful reaction with the copper test, it was not sufficient for complete proof. On investigating the cause of the doubtful reaction, they discovered that it was owing (sometimes, at any rate,) to the presence of allantoin, as this substance was found in the urine of two dogs, the action of whose lungs was artificially impeded by injecting oil into the bronchi, or by the inhalation of chlorine. In the case of a man there was a doubtful trace. Observations were made in various diseases with impeded respiration, such as empyema, pneumonia, and aneurism of the aorta, but no allantoin was discovered.

With reference to the fallacies of the copper test, it is remarked that, unlike allantoin, kreatin has no power of causing the precipitation of the oxide of copper.

6. *The Determination of Iron in the Urine.* By Dr. F. W. BÖCKER. (Prag. Vierteljahrsch. 1854—3.)

The method proposed by Böcker is by the use of a solution of permanganate of potash of a certain strength. A measured portion of urine (say 100 c. c.) is evaporated to dryness, and thoroughly incinerated. The residue is dissolved in pure hydrochloric acid, and, at the same time, half a drachm of zinc is added, and as much hydrochloric acid as will dissolve the zinc. A little carbon always remains undissolved, and is well washed with distilled water, the washings being added to the acid solution, until the solution amounts to 4 or 5 ounces. Then it is filtered, and the permanganate is added drop by drop, till a fine rose red colour is produced. The quantity of permanganate used is then read off, and from this the quantity of iron is calculated. The strength of the solution used by Böcker is so prepared that 45 c. c. correspond to 0.100 grammes of iron.

The only possible error is, that the purest zinc always contains iron. To rectify this, it is necessary to make a second experiment with zinc and acid alone, and to see how much of the solution of the permanganate is used before the fine rose colour appears.

7. *The Elimination of Urea by the Skin.* By C. H. FIEDLER. (Inaug. Dissert. in Schmidt's Jahrb. 1854. 10.)

SCHOTTIN has already shown, that in some cases of cholera, and morbus Brightii, urea is found in the sweat. Fiedler relates two cases of Bright's disease in which this occurred. In one case there was suppression of urine, in the other there was sufficient secretion; but the analysis of this urine is not given.

QUARTERLY REPORT ON SURGERY.

By HOLMES COOTE, Esq., F.R.C.S.,
Assistant-Surgeon to St. Bartholomew's Hospital.

Upon the Cicatrization of recent Wounds produced by Caustic. By Dr. GIROUARD. —Dr. Girouard, of Chartres, read before the Association Médicale d'Eure et Loire, Feb. 27, 1854, an interesting essay, purporting to show in what manner recent wounds produced by caustic, healed, and how unseemly or inconvenient cicatrices might be prevented. Old wounds do not pursue quite the same course as recent ones.

After the application of caustic (Vienna paste, &c.) to the integument of a thin subject, the edges of the wound, when burnt perpendicularly from the surface, become thinner from the very day of the separation of the eschar; the day follow-

* See this journal for January, 1853.

ing they form an inclined plane; and a ring of granulations, one line wide and of deeper red colour than elsewhere, forms a zone on the free margin of the wound. On the fourth day the zone becomes narrower and whitens, and forms a cicatrisial membrane; then a new red zone is developed within the preceding, and thus the wound becomes smaller from day to day. In fat subjects, in whom the areolar tissue is charged with adipose matter, the edges of a similar wound undergo but little or no "thinning;" they round themselves; the adipose membrane swells, becomes covered by granulations; the zone then forms, and, rising to the surface of the wound, pursues the same course as above described. When the edges of the wound are so burnt or cut as to be oblique, the cicatrisial zone pervades, it is true, all the phases of the work of healing, but the skin, loosely connected to the subjacent parts, is dragged towards the centre of the wound. The cicatrix becomes in time condensed and contracted.

The daily diminution of the size of the wound always corresponds with the width of the zone—namely, two to three millimetres, or one line to a line and a half. Centres of cicatrization occur only in old wounds.

By constantly destroying with caustic the cicatrisial zone, the process of healing is permanently arrested; and unseemly contractions may be avoided—first, by limiting the cicatrizing process to situations where the contracting force of the granulation can exercise no inconvenience; secondly, in so arranging the shape of a wound that the extremities of the cicatrisial radii terminate in very loose and extensile tissues; thirdly, in so shaping the edges of the wound that the contracting forces neutralize one another. The Mayor of Ponthevrard had on the lip a cancerous growth, which was removed by caustic. The eschar separated on the eighth day. By means of escharotics, the cicatrisial zone was continually destroyed along the line of wound corresponding with the external integument: the mucous membrane was, then, the part furnishing the cicatrisial granulations; and as contraction was confined to that side of the wound, the mucous membrane was drawn outwards, so as to form a very perfect lip.

Upon Epispadias. By M. NELATON.—Nelaton, in the 'Gazette Hebdomadaire,' remarks that epispadias is a fissure of the corpora cavernosa, just as hypospadias is a fissure of the corpus spongiosum, or nymphæ masculinæ. But as the corpora cavernosa and the pubic symphysis form parts of the same system, the deficiency in one implies a corresponding deficiency in the other; and so it can be readily understood how, with a little further arrest of development, epispadias may pass into extrophy of the bladder. A Swede, æt. 20, who came from Stockholm with the express purpose of putting himself under M. Nelaton's care, presented the following appearances: "The separation of the two corpora cavernosa left upon the upper surface of the penis a shallow urethral gutter, covered by a highly sensitive mucous membrane, and terminating posteriorly in a rounded funnel, on a level with the lower border of the sub-pubic ligament. The pubic bones were one inch apart. Deeply seated at the bottom of the funnel was the urethro-vesical orifice. The patient wore an apparatus to retain the urine during the day; at night time, in spite of the contrivance of a funnel passing through the mattress, he lay constantly in a sort of urinal: his sufferings may be readily imagined." M. Nelaton operated in the following manner. He formed, at the expense of the skin of the abdomen immediately above the urethral infundibular, a quadrilateral flap, as wide as the penis, and rather longer than the organ. He dissected it off, so as to leave it attached at its inferior border by a broad pedicle, corresponding with the inter-pubic ligament. Thus he formed a large cutaneous flap, with which he proposed to close the urethral gutter. The next step consisted in attaching the edges of the flap to the penis. M. Nelaton made, on either side of the penis, on its upper surface, at the junction of the skin with the urethral mucous membrane, an incision extending to the base of the glans. Two short transverse incisions, at the extremities of the preceding, enabled him to dissect back flaps a third of an inch broad, destined to overlap and hold in place the large

piece of integument to be brought down from the abdomen; the raw surfaces of the two flaps formed on the penis would then correspond with the raw surface of the abdominal flap. This having been effected, and the parts united by harelip pins and sutures, two longitudinal incisions were made through the integument of the under side of the penis, that the skin of the organ might be loose, and not drag on the raw edges of the wound above. At the end of three days the sutures were removed; the edges gaped a little, but were soon filled up by granulations. At the end of a month the urethra was quite closed, but was wide enough to admit the introduction of the finger. To rectify this condition, and to promote contraction of the urethra, M. Nelaton employed repeated cauterizations on the inner surface of the new skin, care being taken to avoid burning the mucous membrane. At the end of the eighth or tenth day, when the eschars were about to separate, M. Nelaton made longitudinal incisions to favour contraction, and by these means a state of parts was produced, which may be described as follows: The patient still suffers from polyuria, but he can hold his water when he is in bed; secondly, when he is seated; thirdly, when he is upright and making no effort. When walking, he wears the ordinary caoutchouc apparatus. A similar operation, with beneficial results, was performed by the same surgeon, in 1852, upon another young man, in a yet more deplorable condition.

Operations undertaken for the relief of congenital deformities of the urinary organs in the male do not always, however, terminate in so favourable a manner. Professor Denonvilliers operated, September 1, 1853, in the Hôpital St. Louis, upon a young man suffering from both epispadias and an imperfectly-closed bladder. An attempt was made first to fill up the vesical opening with a flap of integument from the abdomen, and then to form an urethra by a portion of the scrotum; but, in a few days, symptoms of peritonitis supervened, and the patient died within a week of the performance of the operation.

Operation to restore to the Penis a Covering, the Skin having been destroyed by Disease. By M. MARCHETTINI.—A syphilitic ulcer had destroyed all the skin of the penis, and a considerable portion of that of the pubes; the scrotum was entire. M. Marchettini pinched up a fold of the scrotum, passed a knife through the centre, leaving the extremities attached, and then slipped the penis through it. Eight days afterwards, the skin was firmly adherent to the penis, which was, however, bound down by its new covering. Two incisions were made through the scrotum, from the root to the end of the organ, by which it became free, and cicatrization was complete in a few days.—*Gaz. Med. Ital.*

Upon Wounds of Arteries, and their Treatment. By Mr. BUTCHER.—The direction given in works on surgery to cut down to a wounded artery, and to put a ligature above and below the seat of injury, is good in itself, but not always practicable. When the internal carotid artery is wounded by a piece of tobacco pipe thrust through the mouth, the common carotid must be the vessel tied. A similar necessity may exist even in wounds of the extremities, as has been shown by Mr. Butcher, F.R.C.S.I., Surgeon to Mercer's Hospital. We find, in the 'Dublin Quarterly Journal of Medical Science,' an account of a wound of the profunda artery; ligature of the femoral, below the margin of Poupart's ligament, by a transverse incision; arrest of the bleeding; death eleven hours afterwards. It was impossible to tie the artery above and below the wound, owing to its great depth from the surface; the main trunk was, therefore, secured by a ligature, the incision through the skin being made transversely, as previously practised by Mr. Porter, in consequence of the necessity of putting on the ligature so close to the crural arch.

The most interesting part of Mr. Butcher's communication is the narration of those cases in which he commanded hæmorrhage from a large vessel by means of well-adapted pressure. A policeman was stabbed in the leg: severe arterial hæmorrhage, followed by faintness, ensued. Mr. Butcher concluded, from the

nature of the accident, that the posterior tibial artery was wounded. Graduated pressure, by means of a roller and compresses (7 or 8 pledgets over the situation of the wound) was carefully exerted; the foot was raised; a compress and roller were applied over the popliteal artery, and a dose of morphia was administered: the limb was kept steady by a splint. On the following day, the pressure was removed from the popliteal artery and applied to the femoral, by means of an aneurism compress on the groin. The case did well; and Mr. Butcher, in commenting upon it, remarks that, as a rule, the surgeon should not seek for a wounded artery unless it be bleeding. A case of wound of the ulnar artery above the wrist was successfully treated by compression of the wound and pressure over the brachial artery; and profuse hæmorrhage from the hand after excision of the index finger, with removal of the head of the metacarpal bone, was completely controlled by powerful flexure of the injured limb, together with gentle pressure over both the radial and the ulnar arteries.

Hydatid Cysts of the Liver treated by Puncture with a Capillary Trochar, and injected with Tincture of Iodine. By Dr. F. A. ARAN, Physician to St. Antoine Hospital.—Cysts in the liver, formed usually by a collection of acephocyst hydatids not uncommonly burst into organs, where the discharged fluids excite disturbance dangerous to life—e. g., in the lungs; hence various attempts have been made to open the cyst externally through the abdominal walls, by surgical operation. Recamier endeavoured to establish connexion between the cyst and the abdominal wall, by successive applications of caustic paste, that the fluid from the cyst might not, when discharged, find its way into the peritoneal cavity. Jobert de Lamballe introduced a large trochar (for the purpose of injecting iodine), which he allowed to remain in the wound. M. Aran uses capillary tubes, and he relates the particulars of a case,* in which, after ten successive punctures, the following fluid was injected—tinct. iodine, ℥ij.; aquæ destill., ℥ij.; potas. iodid., ℥iss. No pain ensued, and the cyst, which had previously contained thirty-two ounces of reddish fluid, slowly contracted. A second case also is recorded.

Large Sanguineous Cyst of the Thyroid Body successfully treated by Puncture and Injection of Tincture of Iodine. By HOLMES COOTE, F.R.C.S.—The cyst, which extended under the sterno-mastoid muscle to the anterior border of the trapezius, was developed in a healthy young married woman, shortly after her first confinement. It was thrice punctured. There first flowed out sixteen ounces of colourless blood, which separated into a firmish yellow clot and serum. Subsequently, the fluid was of pale-red hue, and much less in quantity. An ounce and a half of the following was injected through a trocar:—Tinct. iodine co., ℥j.; aquæ, ℥v. After the second injection the cyst disappeared entirely, without inflammatory symptoms. The patient is now well.† M. Nelaton has used a similar injection with success, for the cure of a biliary fistula, consequent upon the opening of an hydatid cyst by caustic potash. He also injected a spina bifida without any bad results. Bouchut has cured a chronic abscess (*abcès froid*) by the same means, in a young scrofulous patient, in the Hôpital Sainte Eugénie. Dr. Giamb. Borelli‡ has used the injection equally successfully in three cases of ranula, a disease which, according to modern investigation, seems by no means invariably connected with dilatation of Wharton's duct. Its seat is probably, in many cases, a bursa first described by Fleischmann, and situated upon the outer surface of the genio-hyoglossus muscle under the tongue.

But it must not be inferred that iodine injections are wholly without risk. M. Nelaton§ relates the case of a young man suffering from Pott's disease—i. e., psoas abscess. The injection of tincture of iodine, one part; water, two parts, with as much iodide of potassium as was necessary to keep the iodine in solution,

* Revue Méd. Chir. de Paris, huitième année, tome xvi.

† Medical Times and Gazette, Nov. 4th, 1854.

‡ Gazz. Sarda, ii., 1854.

§ Journ. de Chem. Med., i. 1854.

was followed, five hours afterwards, by dizziness, disturbance of vision, sickness, coldness of the extremities, small pulse. On the following day, there was continued sickness, violet suffusion over the eyelids, and deep pain in the neck. Active measures were resorted to, in order to get the poison out of the system—namely, emetics and purgatives of croton oil. Sinapisms were applied to the feet. From these symptoms of poisoning the patient recovered, but the disease remained in the same state as before.

But inflammation, excited by any cause in a cyst, is commonly followed by serious consequences. It may ensue from a simple puncture. A young woman was admitted into St. Bartholomew's Hospital, April 21, 1853, under Mr. Lawrence, with a large cyst of the thyroid body, bound down by the sterno-mastoid muscle. A small trocar, introduced into the mesial line of the swelling, gave escape to six ounces of sparkling, brownish-red fluid, composed of discoloured blood and cholesterine. After a week, the fluid having re-accumulated, Mr. Lawrence re-opened the original wound, and introducing a director, cut open the front of the cyst with a bistoury. The same quantity of fluid escaped, darker coloured than the preceding, accompanied by some bubbles of fetid gas. Considerable constitutional disturbance followed this operation; there was redness of the neck, violent pulsation of the carotids, &c. A cure was effected by repeated injections of small quantities of the compound tincture of iodine.

On Cystic Tumours. By VERNEUIL, VELPEAU, MALGAIGNE, ROBIN, &c.—Three modes of origin are recognised for the development of cystic tumours—(1) by the enlargement of spaces in the areolar or other tissues of the body; (2) by the dilatation of natural ducts, sacculi, or bloodvessels; (3) by the growth of newly-formed elementary structures, having the characters of cells or nuclei. Of the first variety, specimens are familiar to all in the bursæ mucosæ, which form wherever two hard moveable substances come into contact in the body. Of the second, we have several additional illustrations in the recent records of surgery. Of the third, to which Rokitansky attaches no small importance, no considerable additional confirmatory evidence has been received. The hydatid testicle of Sir A. Cooper is shown by Mr. Curling to consist in dilatation of the seminal tubes.* Sir B. Brodie's statement of the origin of cystic disease of the mammary gland in dilatation of the lactiferous tubes has been confirmed by frequent dissections. Mr. Quekett's injections have satisfactorily proved that cystic degeneration of the kidneys commences in the Malpighian capsules, or the tubuli uriniferi. Mr. Coote has traced the origin of certain cystic tumours to dilated bloodvessels, and especially veins. Dr. Verneuil† has contributed the account of cyst formation in consequence of dilatation of the ducts of the sweat-glands. Cysts about the joints have been shown, not only by most English writers on surgery, but also by the French—namely, Velpeau, Malgaigne, Foucher, Bauchet, &c., to be derived either from some synovial bursa or sheath, connected with the tendons, or from some process of the synovial membrane. In skin, in mucous membrane, in gland-structures, in bloodvessels, and about joints, cystic tumours may be referred to the dilatation and enlargement of pre-existing tubes. In the '*Gazette Medicale de Paris*' (Nos. 22 and 23, 1854), M. Robin classifies erectile tumours, according to the experience of many English surgeons, into—(1) tumours formed by the dilatation of capillaries, *nævi materni*; (2) tumours formed by the dilatation of veins; (3) tumours formed by the dilatation of arteries; (4) tumours formed by the extravasation of blood, around which a wall forms; or aneurisms by erosion. Rokitansky objects to the term "erectile tumours," as universal in such cases, assigning a different mode of origin, and substituting the name "cavernous tumours," as more accurately expressing their character. He states,‡ that the stroma of erectile tumours is formed of a network of fibres, analogous to those of cellular tissue. The thickness of the septa is very variable; from the thicker ones there radiate

* *Med. Chir. Trans.*, xxxvi.

† *Gazette de Paris*, 53, 1853.

‡ *Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien*, von Hebra, 1854.

others, more delicate, which incompletely circumscribe irregular spaces, communicating one with another. In these varieties we find blood, either liquid or coagulated, or concretions resembling phlebolithes. The seat of these tumours is variable; but they are most commonly met with in the liver. Next in order of frequency, Rokitansky puts the subcutaneous cellular tissue, the skin; the face, the trunk, the limbs; the cranial bones, the dura mater, and the pia mater. The primitive element of development in these tumours consists, according to the Viennese professor, in trabeculæ (minute septa), sometimes transparent and smooth, sometimes slightly striated, in oblong nuclei, and in fusiform caudate cells. From these trabecules extend processes which bound irregular spaces, in which blood-vessels become developed. He affirms that the independence of these tumours from surrounding vessels at the commencement of their formation is illustrated by their development in the liver, where it will be found that the blood does not enter them, except by the perforation of their areoles. We cannot say that in our opinion these views, however elaborately worked out and cleverly expressed, are yet substantiated. On the contrary, the immediate dependence of such tumours upon bloodvessels seems to receive, from dissection and examinations, yet further corroboration.

The dependence of cystic degeneration of the kidney upon dilatation of the Malpighian capsules and uriniferous tubules has received confirmation from an observation of MM. Guilleton and L. Ollier, entitled, 'Upon the Abnormal Development of the two Kidneys, in a Fœtus, offering an Obstacle to Accouchement.*' The patient, æt. 33, the mother of four children, had reached the full term of her fifth pregnancy without the occurrence of any circumstance worthy of note, except that the abdomen was much more prominent than usual. July 1. She felt something slip into the vagina, while making a violent effort at stool; it proved to be a foot-presentation. Manual efforts to extract the child were unavailing, and the patient was conveyed to the Charité Hospital, where delivery took place, by the contractions of the uterus, and the patient recovered, although the case was complicated with an attack of metro-peritonitis.

The abdomen of the fœtus, of great size, seemed, when opened, to be entirely occupied by two enormous tumours, extending from the sides to the mesial line. They were the kidneys enormously enlarged, and containing a multitude of cysts. Microscopical examination showed that the tumours contained, as principal elements, uriniferous tubules and closed vesicles; and the authors consider, after noting the relations of the vesicles with the vascular element of the organ, and the disposition in the middle of the cortical substance, that the hydatiform element belonged to the glomerules of Malpighi. Thus the morbid change consisted in hypertrophy, of hydatiform character, of the glandular substance, without the formation of any new product.

A cavernous network of veins has been described by Kohlranscht† on the alæ nasi. The fact has been known some time, but it is mentioned (and that but briefly) only in Hyrtl's 'Anatomy.' This disposition of the vessels may be verified with facility by the injection of air. If such a preparation, so injected, be first hardened by immersion in spirit, and then cut with a sharp knife, this vascular structure can be readily distinguished. It lies between the periosteum and the mucous membrane, the vascular meshes perpendicular to the bones. The mucous glands, which, in other parts of the Schneiderian membrane, are superficial, with short infundibuliform ducts, are here placed deeply, and with longer ducts. This arrangement explains the sudden swelling to which the mucous membrane of the nose is subject; the considerable excretion of liquid so frequent in this condition; also the frequent hæmorrhages from the organ. In this situation mother-spots or nævi sometimes form, and extend with great rapidity. Their destruction by strong acids is unfortunately attended with deformity to the face, unless they are completely removed at an early period when small.

* Gazette Médicale de Lyon, Août 31, 1853.

† Müller's Archiv, No. 2, 1853, p. 149.

The Radical Cure of Hernia. By VELPEAU, MAISONNEUVE, JOBERT, WUTZER, &c.—Attempts are still being made to effect by operation the permanent obliteration of the canal down which a hernial protrusion has occurred. In 1837, M. Velpeau demonstrated the possibility of obtaining a radical cure by means of iodine injections; but the difficulty of introducing the instrument into the hernial sac was sufficient to deter surgeons from following this suggestion. Seventeen years afterwards, M. Jobert made some new trials, and with satisfactory results, but the means of execution remained precisely the same. M. Maisonneuve, surgeon of La Pitié, now proposes a new and simple method of making the fluid penetrate the hernial sac, without danger of its passing into other parts. Supposing the case to be one of scrotal hernia, the viscera are first returned into the abdomen; M. Maisonneuve then seizes the middle part of the scrotum between the thumb and index finger of the left hand, opposite to where lies the empty hernial sac; he pierces it through with a long and thin trocar up to the hilt, and then withdraws the perforator, leaving the canula in the wound. The skin of the scrotum is then drawn over the canula in such a way that two openings, that of entrance and that of exit, should be separated as widely as possible. Lastly, the canula is slowly withdrawn, and when its point is fairly within the sac, the fact is known by its great mobility. This proceeding, somewhat complicated in description, is simple as possible to the operator.—*L'Union Médicale*, Oct. 21, 1854.

The operation of Professor Wutzer, of Bonn, has been successfully practised by Mr. Spencer Wells, the particulars of whose cases are mentioned in the 'Medico-Chirurgical Transactions,' vol. xxxvii. The funnel of integument is derived from the scrotum, as in the preceding operation, but it is kept in situ by a wooden instrument or plug, through which runs a long curved needle, by which the funnel is, as it were, "pegged" to the abdominal walls. We consider this operation preferable to that suggested by Maisonneuve, who introduces his finger as far as the plug of integument will allow, and then cutting down upon the tip, dividing all parts composing the abdominal walls, even the peritoneum, attaches the plug to the sides of the wound by sutures.

On the Value of the Treatment of Blennorrhagic Orchitis by Collodion. By M. RICORD.—Thirty-eight patients have been treated, under M. Ricord, by elastic collodion, according to the rules established by M. Bonnafont. The pain produced by the application of this substance lasted from six minutes to a quarter of an hour. The greater number of patients felt easy from that time, but others experienced a recurrence during the course of the day. One of them complained of most acute suffering. The inflammatory pain diminished in some, and ceased in others, without any sensible influence from the collodion; in a certain number of cases the pain was sensibly augmented. In none of the patients treated in this way had M. Ricord to relate the miraculously rapid cure announced by M. Bonnafont. After twenty-four hours the tumour had not sensibly diminished in volume; it presented, in the majority of cases, the diminution of one-third of an inch after forty-eight hours; then the diminution continued to operate gradually and progressively. The mean duration of the malady in the cases treated by collodion was seventeen to eighteen days.

At the same time, eleven patients, also affected by blennorrhagic epididymitis, were treated by repose, strict regimen, and topical cold. The inflammatory pain disappeared promptly, and the diameter of the tumour was sensibly diminished at the end of thirty-two hours. The mean duration of the malady was fifteen to eighteen days. A third series of patients was treated by compression by strapping. Here the symptoms disappeared more rapidly than in the preceding plans. The mean duration of treatment was fifteen days. One patient was discharged cured in six days. M. Ricord believes that elastic collodion does not cause such suffering as the ordinary collodion, but that it is a more painful mode of treatment than others. M. Velpeau believes that collodion does not shorten the duration of orchitis; it sometimes diminishes the pain; on other occasions it

increases it; it irritates the skin; often excites suppurations, which are tedious and inconvenient. M. Bonnafont has explained his success upon the fact of his having treated his cases at the very outset, a circumstance quite possible in a military hospital.

There is in this general account a confusion which M. Malgaigne has pointed out, represented too often in statistics destined to prove the value of certain medicines. There should be taken into consideration the care which one takes of the disease at its outset; also whether the malady is of more or less standing. M. Velpeau and M. Ricord arrived at a mean duration of eighteen days; but there are many instances of orchitis cured in a much shorter space of time; others which last much longer. Are there not in some of the cases instances of the rapid natural cessation of the disease? M. Bonnafont's report should be compared with another, in which a series of precisely similar, i.e., recent, cases were treated by other surgeons; for when patients arrive at an hospital with swelled testicle, the symptoms aggravated by six or eight days' neglect, the symptoms are necessarily more severe. M. Velpeau, analysing seven observations, published by M. Armand, of orchitis treated by collodion, found in one a duration of ten days; in another, thirty-three; and taking the whole number, a mean of twenty-two days and a half, dating from the commencement of the attack. M. Bonnafont, dating his cases from the commencement of the treatment, reduces the mean duration to twelve days. Had the orchitis been taken by both surgeons at the outset of the disease, they would, in all probability, have arrived at the same result. Is it rational to attribute to the treatment a duration which does not belong to it, supposing, for example, that it happened not to be applied until the twentieth day? On the other hand, if the collodion is applied when inflammatory swelling is subsiding, and only two or three days are required for the spontaneous cure, can one say that the patient owes his recovery to collodion? From this disorder, both in ideas and logic, the numerous facts brought together are so badly arranged, that it is impossible to arrive at any definite conclusion.—*Revue Medico-Chirurgicale*, Oct., 1854.

QUARTERLY REPORT ON MIDWIFERY.

By ROBERT BARNES, M.D. (Lond.)

Physician to the Metropolitan Free Hospital.

I. DISEASES OF WOMEN IN THE UNIMPREGNATED STATE.

1. *On the Treatment of Prolapsus of the Uterus, and especially by ZWANCKE'S Pessary.* By Professor CHIARI. (*Zeitsch. der K. K. Gesell. der Aertze zu Wien.* June, 1854.)
2. *On the Snout-shaped and Polypoid Elongations of the Os Uteri.* By RUDOLPH VIRCHOW. (*Archiv für Pathol. Anat.* Band vii. Heft 1. 1854.)
3. *A New Method of Dilating the Os Uteri.* By Dr. CARL BRAUN. (*Klinik. der Geburtsh.*)
4. *A Case of Puncture of the Ovaries per Vaginam.* By Dr. J. SCHNETTER, of New York. (*Verhandl. der Phys. Med. Gesell. in Würzburg.* Band v. Heft 1. 1854.)

1. Professor CHIARI speaks favourably of a form of uterine pessary contrived by Zwancke, of Hamburg. This instrument consists of two spoon-shaped blades, which are prolonged below into simple stems, and joined together by a Charnier's hinge, so that by means of a screw the two blades may be made to diverge from each other. It is introduced with the blades in opposition: their separation when inserted expands the upper part of the vaginal canal, and supports the uterus. Chiari says it is very efficacious, easily worn, gives no pain, and occasions no

hindrance to walking. He has used it with most successful results in many cases.

2. VIRCHOW draws attention to a particular form of hypertrophy of the vaginal portion of the uterus, which consists in enlargement and elongation so as to resemble a hog's snout, and which Ricord has called the *col tapiroide*. Kennedy, Malgaigne (and we add Montgomery,) have observed that this enlargement has been mistaken for polypus, and removed. Some of the cases observed by Virchow were of a character to give rise to a similar mistake. He assigns the origin of this form of elongation to an abnormal development and projection, or eversion, of the mucous membrane of the cervical canal.

3. Dr. BRAUN designates the instrument which he has contrived for the dilatation of the os uteri the *colpeurynter*. It consists of a vulcanized india-rubber bladder, from two to four inches in diameter, and four inches in length, with an india-rubber tube enclosed in horn, fitted with a brass stop-cock, and a ring through which to pass a silk belt. When used, the india-rubber bladder is introduced empty into the vagina, then gradually distended by injecting cold or warm water. It is retained in situ by the belt fastened round one or both thighs or hip. The horn cylinder is curved in the direction of the pelvic axis: it allows only the upper end of the vagina to be stretched by the bladder; and obviates any unnecessary pressure upon the urethra or external parts. The operation is called *colpeurysis*. The advantages of this proceeding over other methods of dilating the os uteri, whatever the indication for that operation, are highly extolled.

4. The case of Dr. SCHNETTER is one of double ovarian dropsy, two cysts having been successively punctured per vaginam. The patient was twenty-five years old; she had been delivered by the forceps, after a difficult labour, a year before she came under Dr. Schnetter's care; she had suffered from painful menstruation before pregnancy; and during pregnancy, from unusual distension of the abdomen and difficult respiration. Fever and marked symptoms of peritonitis followed delivery. Examination revealed a tumour reaching a few inches above the umbilicus, occupying the right hypogastrium. The os uteri was in its normal position. To the right, somewhat behind it, and a little higher, a round and but slightly elastic tumour was felt. The uterus was longer than natural, and bent forwards towards the pubis. The continuity of the tumour in the abdomen with that in the pelvis was ascertained by counter pressure. A curved trocar was pushed into the tumour, when it projected behind the cervix uteri. At first, blood, then a few ounces of discoloured pus followed. The trocar was driven further in, and by means of a curved knife passed through the canula, a further opening into the tumour made. A little bloody serum followed. A tube was adapted to the wound. Considerable fever and pain ensued. Some days after the operation, pressure being made on the tumour through the abdomen, about four or five pints of offensive purulent gelatinous matter, mixed with fibrinous shreds, escaped through the tube. The tumour disappeared. Discharge continued for some time. The patient's health did not improve. Four months after the first puncture, examined again: another tumour was found projecting into the vagina on the *left* side. This cyst was punctured in like manner. Fever and alarming inflammatory symptoms followed. Six days afterwards an abundant discharge took place, gelatinous and fibrinous in character. The wound healed six months after the puncture. The patient eventually recovered her health; and eighteen months after operation there was no sign of relapse.

II. LABOUR.

1. *On the Rising and Falling of the Pulse during Labour-pains.* By Professor MARTIN, of Jena. (Arch. f. Phys. Heilkunde, 13th year, 3rd part, 1854.)
2. *On the Regularly-rising and Falling Frequency of the Pulse during Labour-pains.* By FRIEDRICH MAUER. (Same work as preceding article.)
3. *Cases of Cesarean Section.* By PAGENSTECHEN and ED. BEHM. (Schmidt's Jahrb. der Ges. Med., 1854.)
4. *Cesarean Operation on account of Pelvic Tumour.* By M. RETZIUS. (Schmidt's Jahrb. d. Ges. Med., 1854.)
5. *A Case of Cesarean Section, preceded by a Summary of Three other Cases observed in the Maternity at Turin.* By M. S. GIORDANE. (Giornale delle Sc. della R. Accademia Méd. Chir., di Torino. July, 1854.)

1. The observations of Professor MARTIN and of Herr MAUER on the influence of labour-pains upon the pulse are of great physiological and pathological interest. Professor Martin refers to the observations of Hohl in 1834 ('Geburtshulfliche Exploration'), that, with every uterine contraction, the uterine-rush became more frequent, louder, and accompanied by a peculiar singing, shrieking tone, until it ceased at the acme of the pain, returning, however, in similar frequency, and then gradually subsiding to what it was before the pain. Professor Martin discriminates between the cause of the regular rise and fall of the uterine-rush, and the cause of the peculiar singing, shrieking tone which accompanies the uterine pulsation during the pains.

The rising and falling frequency of the pulse is to be sought for in increased action of the heart, which may be perceived by the touch applied to the arteries. He therefore studied this by feeling the radial artery of women in labour. He did not, however, observe the cessation of the pulse at the acme of the pains. There might be some special conditions in the uterus to cause it there. He noticed the frequency of the pulse of a parturient woman, which, in the intervals between the pains, beat from 5 to 6 times every five seconds. It rose at the beginning of every pain to 7, and gradually even to 8 and 9, and fell gradually back, somewhat in the following manner, from 5 seconds to 5 seconds: 5 5 6 6 7 7 7 8 8 7 8 8 | 7 7 6 5. When the labour was more advanced and stronger, the pulse in the intervals continued increased in frequency until the expulsion of the child, so that it fell back after the pains to 7, 8, or 9 beats. The following scheme was then observed: 7 7 8 9 9 9 10 11 11 12 12 13 | 12 13 13 13 14 13 13 12 12 12 11 11 | 11 10 10 9 9 8 8 8 7.

Similar results were obtained from observations repeated, under varying conditions, upon many different women.

The professor points out the following applications: 1. A new aspect of the condition of contraction; 2. A new phenomenon whereby to judge of the onset, duration, and strength of the pains; 3. A new and very desirable symptom in the discovery of failing labour-pains. He argues, that the nervous centre of the uterine contraction is the sympathetic; and that the heart's action is influenced by the uterine contractions through the sympathetic nervous system.

2. The observations of Herr MAUER were, for the most part, made in the clinique of Professor Martin. They were carried out with greater minuteness than the professor's. The results are confirmatory. Following Naegele, Busch, and others, he divides labour into five periods; and observes the influence of the pains upon the pulse during each. In the first period, the frequency of the pulse rises, at most, but 1 or 2 beats in 5 seconds above the normal rate of the intervals. In the second period, the frequency rises higher, possibly from 7 to 10 and 11 beats. This rising in the frequency of the pulse becomes more and more marked in the later periods; and in the fourth period it is the most conspicuous. In the fifth (the placental) stage, the rising is small, not more than 2 or 3 beats in the 5 seconds.

These constant relations are not observed in defective uterine contractions, or in false pains. The screams and groans of parturient women do not appear to affect the rising and falling of the pulse. Chloroform does not lower the pulse of women in labour, as it does that of persons preparing for surgical operations.

As to the question, What is the cause of the rising and falling of the pulse during a pain? Is it—1. Muscular action; 2. Accelerated respiration; 3. Direct stimulation through the blood; 4. Irritation in the nerve-centres, which influences the heart's action? After eliminating the first three conditions, he, like Professor Martin, declares for the influence of the sympathetic nervous system.

(The reporter ventures to submit that, whilst a certain influence may be accorded to nervous action, another explanation may be given. In the intervals between pains, a large proportion of the mother's blood is freely circulating in the uterus and placenta. On contraction supervening, the muscular compression cuts off, more or less completely, this proportion from its uterine diverticular course: it is thrown back into the ordinary circulatory apparatus; the increased volume of blood operates as an increased stimulus to the heart—hence the increased frequency of the pulse; a frequency, be it observed, which the observations of Professor Martin and Herr Mauer show to keep pace exactly with the increase of the uterine contraction, the pulse being quickest exactly at the moment of the acme of the uterine contraction. It is also interesting to note that the mother's pulse and the foetal pulse bear an inverse relation, in point of frequency, to each other. The reporter has related, in another part of this journal, observations which show that the foetal pulse is at the highest in the intervals of the pains—i.e., during the period of free intercourse between the maternal and foetal bloods, and that it falls remarkably during the contractions of the uterus, when that intercourse is suspended.)

3. The first case of Cæsarean section of PAGENSTECHER was in a woman, 33 years old, pregnant a fourth time. There was osteomalacia of pelvis in a previously rachitic subject. The promontory of sacrum projected forwards, so as to have a conjugate diameter of from 2 to $2\frac{1}{2}$ inches. The infant was baptized in the presence of the mother, eight weeks after the operation.

The second case is related by BEHM. The operation was called for by an enormous bony tumour in pelvis. The patient was a delicate woman, aged 29, who had shown signs of rickets in youth. She had been delivered in first pregnancy by craniotomy; a tumour the size of a hen's egg, seated at the left sacro-iliac synchondrosis, then obstructing the birth. The rapid growth of the tumour was afterwards observed, till it filled nearly three-fourths of the true pelvis. Pregnant again in 1851, delivery by pelvis was impossible. Cæsarean operation was performed under influence of chloroform. The uterus contained two children, each having its own placenta. Blood escaped into peritoneum; and the patient died on the third day.

4. RETZIUS' case is that of a woman who, in a first pregnancy, gave birth to a living child with difficulty, on account of a tumour in pelvis. In her second pregnancy, the tumour was felt compressible, not painful, and so large as to prevent both the natural passage of foetus and the use of instruments. The operation was performed under chloroform. The child was in incipient decomposition. The patient died forty-six hours after operation. The tumour arose from the margin of the foramen ovale. It was of an irregular round form, and of purely fibrinous texture.

5. The Cæsarean section has been performed during life *four* times within sixteen years, at the Maternity of Turin. During that time, about 1200 women have been admitted. In *eleven* cases, the section has been performed after death: in none of these did the child exhibit signs of life. The four operations during life was necessitated by faulty conformation of the pelvis. In the first 3 cases, the death of the mother followed within three or four days, and the autopsy exhibited effu-

sions, inflammation of the peritoneum, traces of pelvi-uterine phlebitis; and in 1, a hernia of the intestine through the wound in the uterus. In all the cases, the child was extracted living and viable. The fourth case is related by the author in detail. The subject was a primipara, aged 19, who had shown symptoms of rickets. All her six brothers, and all her five sisters, had died in infancy of scrofulous affections. She herself, while a child, had had both femora fractured by a carriage passing over her. To this she attributed her deformity. The vertebral column presented two lateral curvatures; but the lumbar region was not depressed. The left iliac crista was one centimetre higher than the right. The sacrum was not exactly central, deviating to the left. The sacro-pubic diameter, measured successively by the finger, and the pelvimeters of Baudelocque and Van Heuvel, gave different results: its extent was estimated at $2\frac{1}{2}$ inches; the cavity and outlet appeared about normal. The operation was performed under anæsthesia. From the second to the sixteenth day, intense symptoms of peritonitis were combated by bleeding and leeches. At the end of some days, a large quantity of blood, mixed with pus, flowed from the inferior angle of the wound. On the nineteenth day, the patient was improving; there was no fever, and occasional diarrhœa was easily stopped. The upper half of the wound was healed; the wound of the uterus, also, seen through the inferior part of the external wound, was also in process of healing. On the twentieth day, pains in the loins; the left leg slightly swelled at the ankles; the abdomen still painless. On the twenty-sixth day, the patient sank from phlegmasia dolens. The autopsy revealed a large quantity of flocculent pus in the abdomen and pelvis, and recent adhesions of the peritoneum. The venæ cavæ et iliacæ were healthy; but the left femoral had its walls thickened, infiltrated, and was filled with coagulated blood and pus.

The prepared pelvis was an excellent type of the pelvis obliquè ovata of Nægele, which explains the discordant results of the mensuration of the sacro-pubic diameter employed.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. KESTEVEN, F.R.C.S.,

Member of the Council of the Epidemiological Society.

I. INJURIES, &c.

Rupture of the Bladder.—In the month of September last, a young woman died at Portsmouth, whose death was supposed to have been caused by violence. A feeling of indignation was excited by some reports which the daily papers circulated against the officers of a certain ship. On a strict and very protracted examination it was shown, from the medical as well as other evidence, that the girl had died from peritonitis, caused by rupture of the bladder; and that this injury had proceeded from falls, concurring with an over-distended bladder, the deceased having been at the time intoxicated.

Case where it was doubtful whether Death was caused by Drowning, Apoplexy, or Violence.—On the 13th of June, the body of a male was taken out of the Rhine, and an investigation instituted as to the cause of death. The body was completely clad, and was that of a strongly-built youth, about sixteen or seventeen years of age, and presented no trace of decomposition; the integuments of the hands and feet were slightly wrinkled; it was probable that the body had not lain in the water more than three days. The face was swollen and blue. The pupils were dilated; a greenish fluid flowed from the nostrils; the mouth was firmly closed; the tongue was found behind the teeth; the abdominal parietes retracted; there was no *cutis anserina* apparent. On both of the temples were traces of former leech-bites, about the middle of the forehead, extending up into the hairy

scalp, was an oblong space of about six square inches in extent, where the integuments were of a reddish-brown colour, and exhibited distinct appearance of extravasation of blood, which on dissection was found beneath the inguments, and under the aponeurosis. The bones of the skull were uninjured. The *pia mater* was much loaded with blood, as were also the other vessels of the anterior lobes of the brain. Soft plastic lymph was found beneath the arachnoid. The larynx was open; no water found in this or in the trachea. The lungs were moderately distended and crepitant. The left lung was firmly adherent to the pleura. The heart was rather empty of blood. The abdominal viscera were healthy, except that the liver was enlarged.

The physician by whom the dissection was performed concluded that death had been caused in this case by suffocation, as he attached considerable weight to the fact of there having been no water found in the trachea, although the glottis was patent. It was doubtful whether the extravasation of blood within the cranium was the result of apoplexy, suffocation, or violence; the exudation of plastic lymph showed some degree of reaction during life. It was evident, therefore, that the contusion observed on the forehead had been inflicted during life; the interval that had occurred between the infliction of this injury and the submersion of the body was uncertain. The effusion of plastic lymph sometimes takes place with great rapidity. Neumann relates that he had noticed firm adhesion of the intestines to have taken place when life had lasted only twenty minutes after the receipt of injury. Positive evidence was wanting to determine whether death had occurred from the cerebral lesion, after or before submersion.

Dr. Simeon, of Maintz, to whom the preceding facts and opinions were submitted, is of opinion that such an amount of extravasation and effusion is not likely to have occurred from violence externally applied, so limited to one region, without any injury to the bony structures; neither does he deem it probable that such an extent of exudation of plastic lymph can have been produced in a very short time, as the result of concussion of the brain, or of apoplexy; pathology teaches us that it must have been the result of inflammation, localised to the anterior lobes. That such a malady had existed was likewise to be inferred from the presence of the cicatrices of leech-bites on the temples.

The conclusion of Dr. Simeon was, that the deceased, who was a sailor, and had been drinking, had fallen overboard from his ship as it lay at anchor, and had struck his head in falling, so that he was completely stunned, and thereby disabled from struggling.—*Casper's Vierteljahrschrift*, July.

Survivorship in Drowning.—In our last report we had occasion to discuss this subject with reference to the case of Underwood v. Wing, in which a decision contrary to every probability was given. We dwelt upon the circumstance, that in the case of a husband and wife being submerged at the same moment, the wife, by reason of her relative physical feebleness, would be drowned before her husband. That view has received confirmation from the painful narrative of one of the survivors of the awful catastrophe that befel the Arctic steamship, which sank in consequence of a collision at sea. Out of four hundred and forty souls, not one hundred escaped death by drowning. One of the few survivors, named McCabe, clung to a raft for two days, and saw at least seventy individuals, who had with him taken refuge thereon, drop one after the other exhausted into their watery graves. "The women," he observed, "*were the first to go; they were unable to stand the exposure more than three or four hours.* They fell off the raft without speaking a word." After eighteen hours, there were not more than three or four individuals left on the raft with him. The rest of this simple but melancholy tale was that he remained the solitary being out of seventy. On the approach of the night of the second day, during which time he had neither eaten nor drunk, he was on the point of falling himself from exhaustion, when he was rescued by a ship's boat.

Condition of the Remains of a Man after Submersion in Water during Twenty-six Years.—In the year 1828 a miner, named John Stephens, aged twenty-four years, was working in the Penandrea mine (near Redruth, Cornwall), when he fell into the water-shaft; every exertion to extricate him was made in vain; and though diligent search was made for two months, the body could not be found. The shaft remained closed until April last, when, on the working of this mine having been resumed, the remains of the body of the deceased were found, where they had lain under thirty fathoms of water during the twenty-six years.

We are indebted to Henry Harris, Esq., of Redruth, surgeon, for a description of the condition of the bones, &c., as examined by himself and his three pupils. The bones were entire, but completely denuded of all trace of muscle, tendon, ligament, or cartilage; they had a dirty brown or blackish colour. On passing the finger into the cranium, it was found to be nearly full of a dirty brownish substance, of the consistency of very soft butter. This matter had not any odour, it appeared to consist chiefly of brain mixed with some mud. It was not, however, examined microscopically or chemically.

II. MEDICO-LEGAL OSTEOLOGY.

Can the Osseous Remains of a Corpse throw light upon the Age of Deceased?—Can the period that has elapsed since Death be determined by the Condition of the Bones?—Dr. KANZLER, of Delitzsch, has endeavoured to elucidate these two questions, by reference to facts recorded in medico-legal and other writings, and by the statement of his own observations. In attempting an answer to the first, Dr. Kanzler consulted thirty-seven authors, and has collated and condensed their descriptions of the development, growth, and characters of the bones, from their first appearance down to old age. The result of this labour is an article covering nearly sixty octavo pages, consisting entirely of detailed descriptions. These do not admit of abridgment within the space assigned to our report. We must, consequently, confine ourselves to giving an abstract of Dr. Kanzler's exposition of the second point: the determination of the period elapsed since death, as inferred from the condition of the osseous system. The difficulty of determining this question, even where the soft parts remain, is shown in the following instances. A body which had lain in a light gravelly soil had become so completely decomposed at the end of eleven months, that the bones were only loosely held together. The body of a child in the same ground was putrified, and the soft parts destroyed, in six weeks. The soft parts of a body suspended in the open air during five days in the summer season had become decomposed. The body of a man, which had lain nearly two years and a half in a damp sandy soil, was exhumed on suspicion of murder, and was found in a tolerably good state of preservation; the odour of the body was mouldy rather than stinking, the muscular substance was pulpy, but its appearance was preserved; the countenance was recognised by those who had known the deceased in life. In another instance, a corpse was disinterred from ground which had not been disturbed for upwards of ten years, and was found not to have undergone decomposition, but had simply become shrunken and dry, the neighbouring corpses having undergone decomposition. Chemical analysis in this case did not discover the presence of arsenic to account for the preservation of the body.

Dr. Kanzler notes, from his experiments and researches, the following circumstances as modifying the rapidity and course of putrefaction: 1. *Age*. The bodies of infants putrefy more rapidly than those of adults. The soft substance of a foetus in the second month will liquefy in the open air, or in moist ground, without the usual process of putrefaction. The same disposition is distinctly observable until the fifth month. 2. *Sex*. Putrefaction generally takes place more rapidly in females than in males. 3. *Obesity* favours decomposition. 4. *The nature of the previous malady*. The progress of putrefaction is more rapid after acute diseases than

chronic diseases, after exanthematous fevers, all diseases attended by sudden annihilation of the sensibility—e.g., apoplexy, lightning-stroke, &c. The bodies of those who have died from hæmorrhage undergo putrefaction slowly. 5. *External pressure*. The deeper the grave, or the higher the superincumbent mound, the slower the rate of decomposition. 6. *Clothing of the corpse*. Unclothed corpses decompose more rapidly than clothed, as also corpses enclosed in coffins of hard wood decompose more slowly than in coffins made of soft wood. 7. *Occupation*. So far as the influence of trade or occupation is known, it is stated that the bodies of chimney-sweepers and tanners putrefy slowly. (?) 8. The *Medium* surrounding the body. Orfila states that putrefaction takes place more rapidly in the open air, or in dung, than in water, more rapidly in the latter than in the ground, more rapidly in soils abounding in vegetable matters than in clayey moist graves; in the latter more quickly than in dry sandy graves. 9. *Temperature*. Decomposition is favoured by moisture, with warmth between 18° and 25° R. ($=72^{\circ}$ and 85° Fah.) In higher temperatures, especially without moisture, as in the deserts of Asia and Africa, the body becomes mummified. At low temperatures a body is preserved by congelation, as in the case of the corpse of Prince Menzikoff, who had been banished to Siberia by Peter the Great, which was found ninety-two years afterwards in a state of complete preservation. It has also been observed, that the corpses of those who have been interred at the Monastery of St. Bernard, in the Alps, 7200 feet above the level of the sea, have been preserved during many years by the extreme cold. 10. *The deposition of Ona* by insects upon the corpse accelerates putrefaction. [An instance of this kind was given in our last report.]

The changes which will be found to have taken place in bones during any period after interment, will necessarily vary very greatly with all the circumstances above-mentioned, as affecting the process of putrefaction in soft parts.

Dr. Kautzler gives the following general approximative statement. In from two to three years all the soft parts are destroyed; in from five to ten years the cartilages are partially destroyed, and scarcely any trace of medulla remains; after from ten to fifteen years the bones are greasy on their inner surfaces, and at the epiphyses; after thirty years the skeleton is rarely found perfect, only the larger bones remaining; after seventy or eighty years, the thigh bones and base of skull only will be found; after eighty or a hundred years these bones are light, brittle, and porous.

Dr. Kautzler quotes the statements of several authors confirming the preceding observations, and at the same time mentions numerous exceptions of bones having been found entire after many hundreds of years' interment.—*Casper's Vierteljahrsschrift*, July.

To the above we take the opportunity of adding Dr. Casper's observations upon the successive decomposition of internal organs, as given in his *Gerichtliche Leichenöffnungen*:

1. The internal organ in which putrefaction is earliest observable is the trachea and larynx, in which the change will be found to have commenced almost as soon as green spots have appeared upon the walls of the abdomen. When the abdominal parietes are entirely green, the mucous membrane of the trachea, although all other internal organs may be still intact, will be found of a dark cherry-red colour, which gradually becomes of a dark reddish-brown. Casper states that he has found only one doubtful exception out of many hundred dissections.

2. The brains of new-born children, and of infants under two years of age, come next in order of time, being quickly converted into a thin rose-tinted pulp.

3. The stomach resists putrefaction longer than the preceding, but yields sooner than other internal organs. The first traces of its decomposition consist in detached, dirty-red, ill-defined, and irregularly-shaped spots of various sizes in its fundus, where, usually, bluish-red streaks of the veins are found, and which show through these reddish spots. It is of importance to bear these in mind in cases of suspected poisoning. As putrefaction advances these spots enlarge, until the whole mucous surface presents the same condition. Dr. Casper has never seen,

as the result of putrefaction, the entire separation of the mucous membrane, sometimes seen in poisoning with corrosive sublimate.

4. The intestines next undergo a change similar to what has been described in the stomach. In extreme putrefaction of the body, these organs become converted into a dark green pulp, without trace of their structure.

5. The spleen will sometimes be found to be in a state of incipient decomposition earlier than the stomach and intestines, although most frequently it follows them in order.

6. Omentum and mesentery; the fatter these organs, the more rapidly they undergo putrefaction

7. The liver is often found fresh, and of its normal colour, many weeks after death. Putrefaction commences on its convex surface. The first change of colour is greenish, which extends over the whole, changing to a grey and finally to black, the parenchyma softening as putrefaction advances.

8. The brain of adults; the first traces of putrefaction are found on its lower surface, a light greenish discoloration, gradually spreading over the cortical, and extending into the medullary substance. After several months, the cerebral matter is changed into a reddish pulp, similar to what is early observed in the bodies of infants.

9. The heart may be found entire when stomach, intestines, &c., have become putrid. In this organ, first the columnæ carneæ, then the walls, and the entire organ, become softened, greenish, and lastly of a grey colour.

10. The lungs begin to exhibit putrefactive changes about the same time, or occasionally a little earlier than the heart. The lungs are often found unaltered when putrefaction is far advanced in other parts. This fact, Dr. Casper observes, furnishes an argument against the inference sometimes drawn from the hydrostatic test to the lungs of newly-born infants, that their floating is referrible to putrefaction. The first traces of decomposition in the lungs is seen in the formation of gaseous vesicles beneath the pleura, at first single, then increasing and multiplying until an entire lobe is thickly studded therewith; these are more especially observable on the lower lobes. In the further progress of putrefaction the substance of the lungs becomes softened and dark-coloured, lastly black and pulpy.

11. Kidneys. 12. Urinary bladder. 13. Œsophagus. 14. Pancreas. 15. Uterus, last of all to putrefy.

Extraordinary Strangulation of a Child by its Mother whilst in a Dream.—Yesterday, Mr. William Payne held an inquest at St. George's workhouse, Mint-street, Southwark, on the body of Henry Rushton, infant son of Mr. James Rushton, of Little Rodney-street, Suffolk-street.

James Rushton, father of the child, said that he went to bed between one and two o'clock on Sunday morning last. The deceased child slept in the same bed with its mother, who, at the time he went to bed, had it in her arms. He heard nothing of the child during the night, but about seven o'clock in the morning, he was awakened by his wife, who in alarm called him by his name. He inquired what was the matter? and she said that the child was quite cold. He immediately got a light, and then saw that the child was black all over the face. Soon afterwards he went for Mr. Llewellyn, the surgeon, who came directly, and he said that the child was dead. His wife told him that she had had a fearful dream.

Mr. William Llewellyn, of No. 12, Great Suffolk-street, said he was called on Sunday morning to the house in question, and found the child dead. It had been so for some time. The tongue protruded, and the face was very livid. It had all the appearance of having been strangled. He questioned the mother, and she told him she had dreamt that a mad bull was attacking her, and had squeezed up the child to protect it, and when she awoke, as she found the child cold, she called her husband. The child had been properly taken care of. The parents had three other children, were very industrious people, and kind to their children.

The child was lying on her arm, and its death might, very probably, have occurred as she described it.

The jury returned a verdict of Accidental Death.—*Morning Herald*, Nov. 23.

Live Birth at Four Months.—Mrs. R. menstruated on the 8th Feb., and quickened 8th June. On the 17th June, a foetus was expelled, which weighed exactly nine and a half ounces, and measured eight inches in length; its placenta weighed six ounces. The eyelids were adherent, the nose and mouth closed, the membrana pupillaris entire. The lungs, in colour and volume, resembled those of an early foetus, and, with the exception of one or two ecchymosed spots, no colour or other evidences of developed air-cells were noticed, all the appearances indicating that no air whatever had reached the tissue of the lungs.

The pulsations of cord, which were vigorous, were allowed to continue for some time, in order that the reflex movements of the limbs, face, and respiratory muscles might be observed. On touching the hands or feet, or blowing upon the face, a convulsive movement of the limbs or respiratory muscles followed. When the pulsations of the cord had fallen to ninety beats, it was divided, and about a drachm of blood suffered to escape; the heart's action immediately became quicker, and one or two thoracic convulsions followed. The reflex movements gradually became more feeble, and ceased in about an hour.

Dr. Keiller, who related this case to the Edinburgh Obstetrical Society, pointed out the medico-legal relations of this case. They are obviously important.—*Edinburgh Monthly Journal*, September.

III. TOXICOLOGY.

Poisoning with Arsenic.—Dr. BLONDLOT, of Nancy, has related the particulars of a series of four murders, in which arsenic, administered by one individual, was detected in bodies that had been buried sixteen and twenty years. In the latter instance, the coffin had become entirely disintegrated, and the bones of the skeleton lay detached from each other, the ligamentous parts having disappeared. The brain, however, was found entire, but shrunk to the size of a fist. The weight of this brain was about eleven ounces. When very carefully analysed, it was found to contain arsenic. The presence of arsenic in the earth of the cemetery was excluded by careful analysis.—*Journal de Chimie Médicale*, October.

Arsenic in Commercial Sulphuric Acid.—In the same journal, M. BLONDLOT states that the sulphuric acid manufactured in the districts of Nancy, in France, has the contamination of a minute quantity of arsenic.

Poisoning with Metallic Arsenic.—Dr. SCHÜTTE, of Wolfenbüttel, relates a case of poisoning by metallic arsenic (*Cobalt mineral*), and states, that after having searched toxicological writings, he finds only one instance of the kind on record, in which it was accidentally taken. In the case before us, the poison was intentionally administered by the husband of the deceased. The symptoms produced were those usually produced by arsenious acid, and proved fatal in five days. The metallic arsenic had been given in the form of a coarse powder. In its mineral condition, this substance does not exert a poisonous action, but after exposure to the air, a portion becomes oxidized and converted into arsenious acid, in which condition it was found in the stomach of the deceased. In the intestines, it was found in its metallic condition, leaving the inference, that oxidation had taken place from the secretions or other contents of the stomach. Dr. Schütte, however, has ascertained that commercial cobalt mineral contains from 4 to 11 per cent. of arsenious acid.—*Casper's Vierteljahrschrift*, October.

Death from Chloroform.—At the University College Hospital, the administration of chloroform was deemed desirable, in order to facilitate the introduction of the catheter, in the case of a man aged 29 years, suffering from retention of

urine. On its full action having been induced, the patient's breathing became stertorous, and the face suffused. Notwithstanding the use of every means by Mr. Erichsen, the patient died in a few seconds. On examination, it was found that the heart was large and flabby, weighing twelve ounces. The muscular fibre had undergone fatty degeneration, the transverse striæ having been converted into small fatty particles.—*Medical Times and Gazette*, Oct. 14.

Poisoning with Red Precipitate.—Dr. FREDERICK JAMES BROWN, of Chatham, relates the following case. M. A. Kain, aged 16 years and 9 months, swallowed thirty grains of red precipitate mixed with jam, in mistake for medicine, at 11 A.M. August 31st. The swallowing of the powder was followed by a burning sensation (in the mouth and throat?); vomiting occurred in fifteen minutes, when the powder was mostly ejected. Vomiting took place five times before a quarter to 2 P.M., when she was first seen by Dr. Brown. No red precipitate was seen in the matters ejected from the stomach after the second vomiting. Pain was first felt in the stomach, two hours and a quarter after the poison had been swallowed. The treatment consisted in the administration of milk and eggs, and half-grain doses of opium. The mouth and throat were inflamed on 1st Sept. Salivation existed on the 4th Sept. From this date she rapidly recovered.—*Association Journal*, Oct. 27.

Toxicological Action of the Vegetable Acids.—Dr. BENCE JONES refers to the action of oxalic acid as being the type of the action of the vegetable acids on the system. The mineral acids, Dr. Jones remarks, altogether differ from this type, their poisonous action differing in kind from the sedative action of oxalic acid, although agreeing in the corrosive action which they possess. The chemical composition of the vegetable acids, as shown in a table given by Dr. Jones, shows so close a chemical relation, that, on these grounds alone, the possibility of the pre-existence of oxalic acid in these acids has been entertained. Thus, for example, tartaric, citric, and valerianic acids have been held to be coupled or conjugate acids, in which oxalic acid is ready formed.

Dr. Jones cites the recorded action of oxalic and other vegetable acids in repeated minute doses, as well as in larger quantities. Oxalic acid has been found to possess narcotic properties, and when long continued in small diluted doses, closely resembles the effects produced by acetic acid. The actions of acetic acid, tartaric acid, and citric acid, in various doses, are stated by Dr. Jones, and the resemblance between these and the operation of oxalic acid pointed out, and illustrated by observations of the striking resemblance between some other compounds of carbon having narcotic properties—e.g., carbonic acid, CO_2 ; oxalic acid, C_2O_3 ; carbonic oxide, CO ; light carburetted hydrogen, CH_2 ; olefiant gas, C_2H_2 ; cyanogen, C_2N ; prussic acid, C_2NH . The less oxygen the more poisonous the substance. Hence the activity of the substance would appear to depend on the carbon rather than on the oxygen. It is not improbable that, if pure carbon were not insoluble, it might be found to be an energetic poison.

The lecture from which the preceding facts are borrowed contains much valuable practical information regarding the medicinal action of lemon juice and citric acid.—*Lectures on Materia Medica*, by Dr. Bence Jones, in *Medical Times and Gazette*, October 21.

M. CHEVALLIER, in the October number of the *Annales d'Hygiène*, has related a case of poisoning by the vapours of carbon, and gives the conclusions of his experiments, confirming the preceding observations of Dr. Jones. M. Chevallier shows that 3 or 4 per cent. of carbonic oxide will suffice to destroy a strong dog, that would not have been killed by less than 30 or 40 per cent. of carbonic acid in the air. Warm-blooded animals may be destroyed by 1 per cent. of carbonic oxide.

Poisoning with Ranunculus Acris.—Some children amusing themselves in a meadow by making coronets of buttercups (*boutons d'or*), one of them was tempted

to eat several of the flowers. A few minutes afterwards, this child was seized with severe colic, and all the symptoms of poisoning, which, however, were removed by medical care.—*Journal de Chimie Médicale*, Octobre.

Slow Poisoning with Copper.—Dr. CORRIGAN states that fatal results may follow slow copper-poisoning, by its effects in undermining the constitution, producing debility, emaciation, and proclivity to the exciting causes of disease. Retraction of the edge of the gums, with purple discoloration, is observed in these cases, and continues for a very long period.

Poisoning with Verdigris.—Dr. REINHARDT, of Ulm, relates the following case, which is interesting from the large quantity taken and the success of treatment. J. K., 26 years of age, was admitted into the Garrison Hospital in consequence of an attempt to poison himself with verdigris, of which he stated that he had taken, on an empty stomach, about one ounce and a half, with some bread and water, at 1 o'clock, P. M. In about half an hour he felt ill, and vomited some of the verdigris. In about three quarters of an hour the vomiting recurred; in the interval, he suffered severe pain over the eyes, and colic-like pain in the stomach, which was somewhat relieved by an oleaginous mixture. On his way to the hospital in the evening, he vomited several times. On admission, he complained of pain in the head, a metallic taste in the mouth, and colicky pains, with feeling of distension of the abdomen; the left hypochondrium was tender to pressure, the pulse was normal, as was also the temperature of the skin. Sugared water and the albumen of four eggs were administered. In about half an hour he vomited some green fluid, with many particles of verdigris in the matters vomited. Sulphuret of calcium was then given. Some abatement of the symptoms took place: these, however, recurred with increased severity about 3 o'clock in the morning, with the addition of ardor urinæ. Leeches, sulphuret of potassium, albumen, &c., were continued. The patient was convalescent by the 8th November. No copper could be discovered in either the saliva, urine, or blood. In the fæces a slight trace was found.—*Henke's Zeitschrift, dritter Vierteljahrschrift*, 1854.

Poisonous Properties of Delphinine.—Van PRAAG, in the *Archiv für Patholog., &c.*, iv. 3, 385, has given the results of his experiments on fishes, frogs, birds, and mammalia. The effects were almost instantaneous paralysis of the heart. On dissection, congestion of the membranes of the brain, of the heart, the large venous trunks, and of the liver, was observed.—*Quoted in Dr. Littlejohn's Report on Toxicology, in the Association Journal*, Nov. 24.

Poisoning by Oil of Bitter Almonds. Recovery.—Dr. PURSELL, of Kennington, relates the case of a boy, aged four years, who accidentally swallowed about four or five drachms of genuine concentrated oil of bitter almonds. He immediately became insensible; the countenance flushed; eyeballs greatly protruded from the sockets, and having a rolling movement, pupils dilated and insensible to light; pulse slow, full, and strong; breathing stertorous; complete opisthotonos; rigidity of muscles of jaw; frequent convulsive action of muscles of face and neck. Vomiting was produced by sulphate of zinc,—warm water, injected with the stomach-pump, removing undigested food strongly flavoured with oil of almonds. Cold affusion, stimulants, external warmth, promoted reaction. Consciousness returned in about four hours.—*Association Journal*, Sept 29, 1854.

Dissection and Chemical Analysis in Elucidation of the Cause of Death, in a case of suspected Poisoning with Cyanide of Potassium.—The body was that of a youth about nineteen years of age, well made. The cornea had undergone scarcely any change. The mouth was firmly closed, and exhibited the dried remains of frothy saliva on the lips, and a brownish coloured fluid flowed from the angles of the mouth. The hair of the scalp was readily extracted. Incipient putrefaction was observed on the abdominal integuments. The posterior aspect of the body presented the usual discoloration from gravitation of blood. On dividing the scalp

a tolerable quantity of thin black blood flowed from the occipital regions. The bones of the skull were thin. The vessels and membranes of the brain were loaded with black blood. The cerebral substance exhibited excess of bloody spots when cut through. The substance of the brain was firm. No odour was perceptible within the cranium. No injury was detected in any of the bones of the skull.

The abdominal cavity contained a quantity of bloody and watery fluid, a portion of which was set aside for analysis. There was increased vascularity in parts of the mucous surface of the small intestines, which presented a dark red colour. The other abdominal viscera were in a healthy condition. The stomach was tied at each end, and removed: a bluish-red discoloration was observed on its upper surface. The remains of food were found in the stomach; the mucous membrane of this organ had an intensely dark reddish-brown colour. The larger veins of the abdominal viscera were somewhat congested with dark blood.

The heart was firm; the large vessels full of blood. The cavities of the heart were empty, with the exception of the right auricle, which was filled with dark blood. The lungs, more particularly the lower lobes, were congested with dark blood, and presented several spots of extravasation.

The upper end of the œsophagus, to the extent of one inch, presented its normal appearances; from this point, however, to its termination, it had a blue colour, not removed by washing in water.

The matters submitted to chemical analysis were,—1. The stomach and its contents; 2. The fluid from the abdominal cavity; 3. Fluid which escaped from the mouth and nostrils; 4. Blood from the cranial cavity; 5. A cup containing matters vomited by deceased; 6. The remains of the food partaken of by deceased. The analysis was performed by Drs. Tschepke and Eichhorn, who detected abundant evidence in 1, 2, and 3, of the presence of prussic acid, supposed to have been derived from ferro-cyanide of potassium, in use in the factory in which deceased had been employed. No prussic acid could be separated from 4, 5, or 6.

It was, for the following reasons, concluded that death in this case had been caused by cyanuret of potassium:—1. It had taken place suddenly; no other poison with which the public is familiar has so sudden an operation as has prussic acid. Strychnine, brucine, atropine, nicotine, are not well known to the public. 2. The unusually slight change that the cornea had undergone. 3. The looseness of the hair of the head. 4. The unusually fluid state of the blood. 5. The congested state of the venous system. 6. The inflamed state of the stomach and intestines. 7. The extravasated spots in the lungs.

The rapidity of decomposition of the body, which has been observed in poisoning by prussic acid, was not noticeable in this case, probably owing to the state of combination of the acid with an alkaline base.—*Cusper's Vierteljahrsch.*, July.

MEDICAL INTELLIGENCE.

The Cause of the late Explosion at Newcastle.

DR. ALFRED TAYLOR was deputed by the Secretary of State to investigate the circumstances of the late catastrophe at Newcastle. The inquiries were more especially directed to ascertain the truth of a rumour, that large quantities of gunpowder, stored in one of the warehouses or vaults, was the cause of the explosion. We have condensed the report considerably, but have not omitted any important point.

After giving the analysis of a powder found on the premises, which was supposed to be gunpowder, but which was merely a mixture of sulphur, sulphuret of sodium, sulphate of soda, and a little oxide of iron, Dr. Taylor proceeded to the report on the cause of the explosion.

“ Arguments for Gunpowder.

“The effects produced—suddenness, violence, and extent of the explosion—apparently no other cause in the first instance to account for it.

“ Against Gunpowder.

“1. No direct evidence of its presence in the warehouse.

“2. No *body of flame* seen at the *instant of explosion*:—only before and afterwards.

“3. No traces of *burning by gunpowder* on the bodies or dresses of the dead.

“4. There were *no traces of gunpowder*, consumed or unconsumed, in any part of the premises.

“5. The explosion, from whatever cause, must have taken place in the *vault*—this being the crater, focus, or centre of explosion.

“The water of the crater (the vault) has been analyzed, and contains no salts of potash (the indication of burnt gunpowder). It is surrounded by large blocks of sulphur, some of which has undergone fusion. This sulphur in the vault must have been in a melted state before the gunpowder was fired, because sulphur melts easily at 230 deg. Fahrenheit, and I have determined by experiment that gunpowder is not exploded under a temperature of 525 deg. Fahrenheit.

“The facts being so, the eight tons of gunpowder, or whatever the quantity, must have been discharged in the midst of the melted sulphur, so that the cloud of *charcoal* and *sulphuret of potassium* would have to traverse the melted sulphur; yet analysis shows that there is not an atom of charcoal in the fused sulphur of the crater, nor any sulphuret of potassium or sulphate of potash.

“Every atom of the 2240 lbs. of charcoal, as well as of the 9000 lbs. of sulphate of potash, has been proved to be absent from about the centre or focus of explosion.

“The mass of *fused sulphur* from the crater was analyzed. Its chemical composition bears out exactly the statement of the witnesses, that there was nothing in the vault but *sulphur* and *nitrate of soda*.

“Within six yards of the crater, in the precise direction which the explosion took across the Tyne—i.e., north—were lying a number of bags of nitrate of soda, which had evidently been blown over by the explosion. As, from their position, they must have been in the very course of the explosion, it is clear that the blast of eight tons of gunpowder could never have passed over them without scorching, singing, and blackening by charcoal the hempen fibre, yet they presented no traces of such action.

“6. The sulphuret of potassium produced in the discharge of gunpowder has a tendency to discolour or turn brown white-lead paint—i.e., if within reach of the cloud of sulphuret formed from the discharge of powder. A marine-store shop in Hillgate has had its front blown in, but there is no general tarnish or discoloration of the painted joists.

“For these reasons I do not see that the theory that this explosion took place as the result of gunpowder has any scientific support whatever.

“ For Aqueous Vapour or High-pressure Steam.

“In favour of this view we may notice the terrific effects—the rending and destruction of all surrounding buildings and objects.

“ Against it.

“1. A large quantity of water, say one or two tons, must have been suddenly converted to steam in order to produce the effects observed.

“2. How could this have entered the vault in such a volume at once? How could it have accumulated to explode all at once in a quantity sufficient to account for the tremendous explosion? There was no boiler or vessel in the vault to hold these tons of water until it had reached that amount of pressure which would suffice to rend the walls asunder.

“3. At the moment of the explosion, clouds of steam would have been visible,

and some of the bodies at least, near the seat of explosion, would have presented marks of scalding.

“The facts above-mentioned show that steam was not the agent in this case.

“*For Gaseous Explosion.*

“1. The adequate production of a large quantity of gas (nitrogen and sulphurous acid) in a strong and comparatively closed vault, by the combustion of the materials contained in this vault—namely, nitrate of soda and sulphur.

“2. The known law in physics, that when any gases are produced by chemical decomposition in large quantity, in a comparatively confined space, and under a high temperature, the vessel or space must, sooner or later, burst or be rent asunder, simply by the mechanical power of the gas.

“3. The open doorway forms a small area compared with the large area of the vault, and the top of the doorway was five feet from the roof, forming an enclosed space above.

“4. The sulphur will burn at the expense of the oxygen of the nitre; and in proportion to the amount of oxygen or nitre commanded by the sulphur, so will be the intensity of the heat. Eighty-six pounds of nitrate of soda would be equivalent to 2400 cubic feet of air in burning with sulphur.

“5. Assuming that about two tons of this material are in a state of active combustion, so much of the two gases might be suddenly extricated that the chamber, and all above and around it, would be blown to pieces before the gases could find a sufficient escape by the doors. If, by any accident during the conflagration, the space of the doorway became obstructed, partially or wholly, this would at once convert the chamber into a vast shell, which would burst and carry all before it at its weakest point.

“6. Confirmatory of this view, I found that the *powder* scraped from a large mass of stone which had been blown across the Tyne consisted of sulphur, sulphuret of sodium, and sulphate of soda.

“An iron kerb-post, weighing three cwt. or more, blown into a stonemason's yard at Gateshead, had on it a powder which I examined, and found to consist of similar constituents—sulphur and sulphate of soda.

“On the gunpowder theory, this appears inexplicable, unless we suppose that all traces of the burnt powder had been carefully washed off. On the theory above given, these deposits on the stone and iron are accounted for.

“7. This kind of explosion is unattended with flame or steam. The gases do not burn (nitrogen and sulphurous acid). They would not (like exploded gunpowder) blacken, scorch, or singe bags of nitrate of soda over which they passed.

“There would be no such volume of flame in the explosion of these gases as in the burning of gunpowder. All would be, at the instant, darkness; and there would be the choking, stifling sensation of sulphurous acid.

“8. How did the sulphur become ignited? It should be known most distinctly and clearly, that sulphur will melt and take fire without the immediate contact of flame, or of any incandescent or ignited body. Hot air alone will cause it to melt in flame and burn. It melts at 230 deg.—i.e., only 18 deg. of Fahrenheit above the boiling point of water; and I have found it to ignite between 347 deg. and 400 deg.—certainly under 450 deg. It will ignite at a temperature at which gunpowder *will not* explode.

“My belief is, that a temperature of 600 deg. soon spread over the whole of the building, including the basement chambers. There was a ton and a half of coal oil or naphtha in the flat above the vault, which, by burning, must have diffused a large amount of heat through the whole building, quite enough to ignite sulphur. From the worsted factory wall, if the bricks became heated, the heat would be conveyed to the air in a narrow passage described to run out and open by a free space near the mouth of the vault. This air might soon become hot enough to ignite sulphur without the necessity of any burning ember or flame reaching it, or without the wooden roof of the vault being burnt through.

"9. Many of the articles in the basement escaped burning, and much of the lead escaped melting. The burning of bodies depends on the presence of oxygen in the air. Sulphur, when once ignited, soon removes the whole of this oxygen, so that no combustion can go on; and it would itself soon cease to burn, unless it were in contact with a substance like nitrate of soda, which can supply to it an abundance of oxygen.

"I have found that sulphurous acid, even when it forms only 33 per cent. of air, extinguishes a lighted candle, a lighted stick, and all bodies that are kindled or in the act of burning. In the concentrated state, nothing can keep ignited or lighted within this vapour for one single instant. Thus the thick wooden roof of the vault might ignite from above, but it could not be ignited from below, because the upper part of the vault would be quite filled with the vapour of sulphurous acid, and there would be no *air* in the chamber to sustain the combustion of a single spark in the timber. This would explain the non-ignition and absence of burning in the bags of nitrate of soda outside the chamber.

"It is my belief, judging from all the facts as they at present appear in this remarkable case, that the burning of the sulphur and nitre in the chamber might attain the vigour necessary to lead to explosion before all the sulphur outside the chamber was melted—before all the bags outside the chamber were burnt, or the nitrate of soda within them melted—or before all the pigs of lead were melted, and before the roof of the chamber was burnt or had given way. If the flat above fell upon the roof of the vault immediately before the explosion, this would add to the superincumbent weight, increase the explosive force of the gas within, and lead to a greater amount of destruction than would otherwise have occurred.

"This is the only theory upon which, as facts at present stand, I can account for this explosion. Gunpowder does not account for it, unless we throw aside all experience concerning the chemical action of this body. Steam cannot account for it. Lastly, upon the only theory reconcilable with the evidence of what is proved to have been in the vault, and to have been carried by projectiles across the Tyne, and to a great distance in Gateshead, had there been substances in this vault not ignitable, or which could not form gases, there would have been reason for looking to some other force, such as gunpowder, steam, or volcanic agency for a cause.

"ALFRED S. TAYLOR, M.D., F.R.S."

The verdict of the jury was in accordance with this evidence.

BOOKS RECEIVED FOR REVIEW.

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Ein Beitrag zur Pathologischen Anatomie der Geschwülste im Verlaufe der Nerven (Inaug. Diss.). Von Florian Kupferberg. Mainz, 1854.

Handbuch der Pathologie und Therapie. Von D. C. A. Wunderlich. Zweite Band, V. Abtheil. (Schluss). Stuttgart, 1854.

Handbuch der Physiologischen Chemie. Von Professor D. C. G. Lehmann. Leipzig, 1854.

Traité des Tumeurs Blanches des Articulations. Par le Dr. J. Crocq. Bruxelles, 1854.

Harrogate and its Resources, with an Analysis of the Waters. 1854.

Eighth Report of the Commissioners in Lunacy. (Printed by order of the House of Commons, June, 1854.)

Practical Observations on the Use and Abuse of Tobacco. By John Lizars, late Professor of Surgery to the Royal College of Surgeons. Edinburgh, 1854.

A Discourse on Medical Botany. By Earl Stanhope. London, 1854.

Cholera, an Analysis of its Epidemic, Endemic, and Contagious Character. By Henry Stephens, M.R.C.S. Third Edition.

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A Course of Practical Chemistry, arranged for the Use of Medical Students. By William Odling, M.B., F.C.S., Prof. of Practical Chemistry at Guy's Hospital. London, 1854.

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A Manual of the Practice of Medicine. By T. H. Tanner, M.D. Second Edition. London, 1854.

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PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Hints towards the Formation of a more comprehensive Theory of Life.* By SAMUEL TAYLOR COLERIDGE. Edited by SETH B. WATSON, M.D.—London, 1848.
2. *The Human Body, and its Connexion with Man, illustrated by the Principal Organs.* By JAMES J. GARTH WILKINSON, M.R.C.S.L.—London, 1851.
3. *Cours de Philosophie Positive.* Par M. AUGUSTE COMTE, Ancien Élève de l'École Polytechnique. Tome troisième, contenant la Philosophie Chimique et la Philosophie Biologique.—Paris, 1838.
Positive Philosophy. By AUGUSTE COMTE. Vol. III.
4. *The Positive Philosophy of Auguste Comte.* Freely Translated and Condensed by HARRIET MARTINEAU. Books IV. and V. ('Chapman's Quarterly Series.')—London, 1853.
5. *Système de Politique Positive.* Par AUGUSTE COMTE. Tome premier, Chapitre troisième; Introduction Directe, Naturellement Synthétique ou Biologie.—Paris, 1851.
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6. *The Positive Philosophy of Auguste Comte.* By G. H. LEWES. ('Bohn's Scientific Library.')—London, 1853.
7. *Psychological Inquiries.* By B. C. B.—London, 1854.
8. *Histoire Naturelle Générale des Règnes Organiques principalement Étudiée chez l'Homme et les Animaux.* Par M. ISIDORE GEOFFROY SAINT-HILAIRE, Membre de l'Institut, &c. &c. Tome premier.—Paris, 1851.
General Natural History of the Organic Kingdoms. By M. ISIDORE GEOFFROY SAINT-HILAIRE. Vol. I.

AN eloquent living author, writing on "the history in words," prefaces his illustrations of such history by the following remarks:

"It might at first sight appear as if language—apart, that is, from literature and books, and where these did not exist—was the frailest, the most untrustworthy of all the vehicles of knowledge, and that most likely to betray its charge: yet it is, in fact, the great, oftentimes the only, connecting link between the present and the remotest past, an ark riding above waterfloods that have swept away every other landmark and memorial of ages and generations. Far beyond all written records in a language, the language itself stretches back and offers itself for our investigation—'the pedigree of nations,' as Johnson calls it—itself a far more ancient monument and document than any writing which employs it."*

The history of a science is often written in the names by which, through successive ages, it has been known, and in the nomenclature which it employs to designate the objects of which it treats. It is not our intention to pursue this thought with regard to physiology farther than to recal the originally wide signification of that term—the discourse, science, history, or "word" of nature. Unrestricted as it is now to the phenomena of organized beings, the science of physiology, among the ancient philosophers, represented the sum of their knowledge with regard to all the varied manifestations of what they conceived to be an all-pervading life, revealing its power and ceaseless energy in earth, air, fire, and water, as well as in the organized creatures which might make those so-called "elements" their home. Gradually, the limits of physiology have been narrowed, and in the present day we include within its circle only the phenomena and laws of life as they are displayed by organic bodies. It is our purpose, in the present article, to inquire into some of the reasons which have led to these progressive separations; briefly reviewing the history of our science, we shall endeavour to discriminate between the generic ideas of life which have prevailed at different and at the same periods of time; to point out what we conceive to be the proper position of physiology as a branch of human knowledge, its relation to the other sciences which surround it, and the method to be employed for its successful prosecution.

In the earliest ages of the world (so far as we can gather from the records which have been preserved), there was extensive acquaintance with the characters, mode of life, habits, and general utility of many animals. The immediate dependence of man upon the lower creation, both for food and for assistance in his daily toil, in his agricultural pursuits, and in the chase, would necessarily be the stimulus for increasing his knowledge, and, at the same time, a most important means to be employed in its pursuit. Some attempts at classification are to be found in these earliest records, but the physiological ideas which prevailed are obscured by their mixture with theologic notions and with religious feelings. The Indian and the Persian, seeing in the bodies of some animals a resting-place for the souls of their departed friends, and in others the foes of their protecting Deity, treated the one with reverence and the other with awe, and thus shut themselves out from the possibility of acquiring a knowledge which should dispel their superstitious dread, and confer upon them the benefit of its own practical utility. In Egypt, similar tendencies prevailed; the priest

* On the Study of Words, by R. C. Trench, B.D., p. 60.

was the depositary and source of all knowledge, and mysterious allegory was the medium through which some fragments were conveyed to others—or, rather, it was the veil which, attractive by its own charms, served to conceal from the general gaze, not only the truth, which each might have made useful to himself, but also the falsehood, which, apart from this veil of mythic beauty, would have appeared in its own naked ugliness, and repulsed the devotee both from his religious ceremony and his priest.

In Greece (as in Egypt), the various sciences were cultivated by the same person, and were all alike taught by him to the common people. The sage or philosopher was the naturalist, the moralist, and the physician. Having more truth to impart, and less error to conceal—his purpose being, primarily, instruction, and secondarily, power—he was more honest in his object, more successful in his work, and more enduring in his fame. The desire of the earliest Grecian philosophers was to reduce all forces and all forms of action to some one great general principle; and in their endeavours to accomplish this end (with the exception of Hippocrates), they pursued the same *à priori* method, often losing themselves amid interminable labyrinths of thought, or wrecking their whole system, with many of its earlier observations, by striking upon some new and contradictory facts. Aristotle, the greatest of Grecian naturalists, combined his knowledge of zoology and physiology with other and almost universal knowledge and speculation. His genius was that of compilation and classification, but he often passed beyond these to the realization of abstract truths, and founded a school in which his disciples were *the* naturalists for many successive generations.

Pliny, Galen, Dioscorides, and others, made large additions to the sum of detail-knowledge; their works evince exhaustless powers of labour rather than the efforts of genius, and though they were of much importance to the science of natural history, they produced no material advance of physiology. During the Middle Ages, physiology shared the common fate of other branches of knowledge, being lost in the general degradation, or even intellectual death. The creation of universities awakened, or demonstrated that something had already awakened, the dormant spirit of inquiry, and once more earnestness and success were apparent in the search for truth. Philosophy ceased to be a branch of theological study, but still the different sciences were unseparated; the cultivator of one was the student of all; and thus the learned men of the fourteenth century resembled the early sages among the Greeks, differing from them, however, as M. Isidore G. St.-Hilaire has said, in exhibiting no efforts of invention and imagination, no aspiration towards the future, their eyes turned only towards the past, having no other thought than to study and comprehend the ancient masters, and gathering from their books to reconstruct the science of antiquity. In such an epoch, the supreme merit was erudition, and compilation was the prominent feature of European work. This continued until the sixteenth century, when men arose who added to their laborious investigation of the past not only analysis and criticism, but original observation and induction. Bernard Palissy, for example, recognised in fossil organic bodies the remains of previously living creatures (already noticed by Avicenna and others), and he did more than this, in rescuing them from the domain of *lusus naturæ*, by inferring the ancient

submersion of the earth. The names of Vesalius, Fallopius, Eustachius, and others of this period, have been handed down to the present day by our daily recognition of their anatomical discoveries; and it is worthy of notice that the naturalists of the sixteenth century, being the practical physicians and surgeons of the time, directed their attention mainly to such lines of investigation as should prove of service in the science and art of therapeutics.

Though belonging to the history of chemistry rather than of physiology, the name of Paracelsus must not be omitted from this sketch of the sixteenth century. His preposterous pretensions and professorial arrogance, inaugurated by burning publicly in his class-room the works of Galen and Avicenna, gained many adherents to his tenets, the scope of which we shall endeavour to point out in the sequel, as they were subsequently developed and modified by Van Helmont and Ernest Stahl. It is sufficient for our present purpose to designate these men as the founders of the iatro-chemic school, who attempted to explain the phenomena of life in health and disease by variations in chemical processes of the body, under the guidance of a distinct spiritual entity, the "Archæus" having its throne on the cardiac orifice of the stomach. The illustrious Sylvius carried some of these notions to their extremest point, and the tendency of the time can be explained only by the vast accessions which were made to chemical science, and the propensity which exists in the human mind to seek for unity in its explanation of the various phenomena with which it has to deal—that "*idola tribus*," in accordance with which "the human understanding, from its peculiar nature, easily supposes a greater degree of order and equality in things than it really finds, and will invent parallels, and conjugates, and relations, where no such thing is."

More correct methods were adopted by others, and with a success which has been the germ of much now taking the rank of scientific truth. Clusius (or Charles de l'Ecluse), by introducing into his classes the animals and plants of other countries, endeavoured to make natural history universal and comparative. Rondelet and Belon were the creators of ichthyology, the latter placing side by side the skeletons of man and of the fish, pointing out their analogous parts, and thus hinting at the "unity of composition" in organic bodies, which it was the honour of Geoffroy St.-Hilaire to demonstrate. Conrad Gesner, at whose prodigious labours of compilation we can only wonder, laid the foundation, by indicating the true principles of its formation, of the "natural system" in botany. Cæsalpinus spoke confidently of the circulation of the blood, and not only of the lesser (pulmonary) circuit already known to Servetus, but of the larger (systemic) current. Fabricius pointed out the existence and direction of the venous valves, as well as the transition stages of the embryo; while his pupil Harvey demonstrated the circulation of the blood, and proclaimed the analogy between those transitory conditions of the fœtus and the permanent characters of the inferior animals.

The influence of Bacon and Descartes was now felt in all branches of scientific investigation; and although the attempts made by the latter and his followers to give mathematical precision and physical explanation to the phenomena of life (which transcend these methods), led to erroneous conceptions, and the most extreme divergence from the paths which

could conduct towards truth, the necessity for minute and careful observation, and for its no less faithful record, was increasingly felt by those who endeavoured to become "the interpreters of nature." The microscope was the great means of progress, under the direction of Leuwenhoeck, Hartsoecker, Malpighi and Swammerdam. Investigation of objects hitherto hidden, and its results, new and increasing knowledge, gave rise to doubts, and more than doubts, with regard to the correctness of ancient writers. The trammels of antiquity were thrown aside; and criticism, based upon original observation, became the *media via* between the extremes of scepticism on the one hand, and reverence for traditional authority on the other. Minute observation was the necessity of the time, and for its successful prosecution the work was divided. Thus the seventeenth century prepared the way for those two illustrious men whose names will last, and whose memory will be revered, so long as the phenomena and laws of life form the subject-matter of human investigation. M. Isidore G. St.-Hilaire, to whose recent treatise we are indebted for many of the preceding facts, describes in such eloquent language the characteristics of Buffon and Linnæus, that we are sure the following quotation cannot but afford pleasure to the reader:

"Linné et Buffon sont nés précisément dans la même année, et à quatre mois seulement de distance, l'un en Mai, l'autre en Septembre, 1707; mais cette presque identité de dates, la puissance de leur génie, la grandeur des services qu'ils ont rendus à l'histoire naturelle, sont les seules similitudes réelles que l'on puisse signaler entre eux. Linné naquit pauvre dans un petit village de la Suède guerrière et encore barbare de Charles XII; Buffon, au sein d'une noble et riche famille, dans cette France que le règne de Louis XIV, venait de faire si grande. Linné, contraint un instant de se mettre en apprentissage chez un ouvrier, eut à soutenir une longue et pénible lutte contre l'adversité: si Buffon eut besoin d'une ferme volonté, ce fut pour résister aux séductions de cette vie molle et oisive dont sa fortune et son rang lui offraient le privilège. Tous deux enfin avaient reçu de la nature des tendances intellectuelles plus diverses encore que les circonstances au milieu desquelles ils durent se développer; il fut dans leur destinée de se compléter l'un l'autre par l'opposition des qualités contraires, et de s'estimer sans se comprendre. Linné, aussi patient, aussi sagace dans la recherche des faits, qu'ingénieux à les coordonner; plus prudent encore que hardi dans ses déductions; ne dédaignant pas de se tenir longtemps terre à terre, perdu en apparence au milieu d'innombrables détails, pour s'élever ensuite d'un vol plus sûr vers les hautes régions de la science; habile à former hypothèses, mais ne se faisant pas illusion sur elles, et lors même qu'il les étend à l'ensemble de la création terrestre, ne se laissant pas éblouir par leur grandeur; assignant, avec une étonnante sûreté de jugement, à chaque notion son rang et sa valeur, comme à chaque être sa place; doué d'une persévérance qui ne fut jamais ni découragée par les obstacles ni fatiguée par le temps; aimant la vérité pour elle-même, et trouvant que son expression la plus brève et la plus simple est aussi la plus belle; recherchant surtout dans son exposition cette élégance propre aux écrits scientifiques, qui résulte de l'enchaînement des pensées plus que du choix des mots; enfin, sans cesser jamais d'être exact et concis, variant son style depuis la précision austère de la formule jusqu'à cette haute poésie dont la Genèse nous offre les plus sublimes modèles: Buffon, sagace, ingénieux à l'égal de Linné, mais dans un autre ordre d'idées; dédaignant les détails techniques, négligeant de multiplier autour de lui les faits d'observation, mais saisissant les conséquences les plus cachées de ceux qu'il possède, et sur une base fragile élevant hardiment un édifice durable, dont lui seul et la postérité concevront le gigantesque plan; se refusant à emprisonner sa riche imagination dans le cercle étroit des méthodes, et cependant, par

une heureuse contradiction, créant un jour une classification que Linné même put lui envier; s'égarant parfois dans ces espaces inconnus où il s'élance sans guide, mais sachant rendre fructueuses ses erreurs même; passionné pour tout ce qui est beau, pour tout ce qui est grand, et s'il ne termine rien, osant du moins tout commencer; avide de contempler la nature dans son ensemble, et appelant à son aide, pour la peindre dignement, les trésors d'une éloquence que nulle autre n'a surpassée: Linné, un de ces types si rares de la perfection de l'intelligence humaine, où la synthèse et l'analyse se complètent dans un juste équilibre, et se fécondent l'une l'autre: Buffon, un de ces hommes puissants par la synthèse, qui, franchissant d'un pied hardi les limites de leur époque, s'engagent seuls dans les voies nouvelles, et s'avancent vers les siècles futurs en tenant tout de leur génie, comme un conquérant de son épée!" (p. 71.)

The 'Systema Naturæ' of Linnæus accomplished three most important objects. First, the establishment of a binary nomenclature for organized bodies. This, although common among the earlier naturalists, was not employed with any system to denote, as Linnæus made it, the persistence of generic similitude in conjunction with specific difference. Secondly, Linnæus subjected scientific language to invariable rules; and thirdly, he arranged and classified both animals and plants according to a new and comprehensive plan. Reaping, as we do, the fruit of his labours, it is not easy for us fully to appreciate all their value, since none but himself can know the hours and years of toil devoted to the cultivation of so vast and so rich a field.

The 'Histoire Naturelle' of Buffon was a daring and grand attempt to convey the philosophy of zoology. Its author perceived the unity of plan amid the endless diversities of organic development; he traced the laws regulating the geographical distribution of living beings, their successive appearance on the surface of our earth, and the limited variability of species. The analytic labours of Linnæus, and the synthetic work of Buffon, concurred to raise Natural History and Physiology to the rank of ordained sciences. Without the system of the former, and the power which it supplied for legitimate induction, it would have been impossible for the latter to have arrived at his most brilliant illustrations of deduction. The two in combination present us with the method and the end of scientific investigation.

The close of the eighteenth and the commencement of the nineteenth centuries are crowded with the names and labours of men of whom the world is, with justice, proud. The names of Müller, Haller, Blumenbach, Spallanzani, Bonnet, Hunter, Duhamel, Humboldt, Cuvier, and Geoffroy St.-Hilaire, do but feeble justice to the list,—they are familiar to us in their association with great discoveries of fact, or with the institution of great principles. Our space will not permit the separate consideration of their works, but we shall have occasion to refer to several of them in connexion with the next portion of our inquiry into the "ideas of Life" which have prevailed at different periods of physiologic history, and which are prevailing now.

It appears to us that we must arrive at some definite notion upon this subject—*what we mean by Life*—before we can consider to any profit the position which the study and science of its phenomena is to take in the encyclopædic scale, and before we can fairly judge of the method by which those phenomena may be reduced to scientific laws. Our own com-

plicated existence presents us with two classes or spheres of life for contemplation and study; and it is absolutely impossible for us, created as we are, to place in the same category the phenomena of life which we observe in our own material organs and in the animals and plants surrounding us, and those processes of thought and feeling of whose existence we are conscious, and even more certain than of the reality of an external world. There appears to be so broad a line of distinction between that which knows and that which is known—between the intellectual or subjective, and the physical or objective life, that we may not lightly group their phenomena together, as being of the same kind and proceeding from the same source; although we are told, on the one hand, that all the processes of organic life are the result of an immaterial principle, the soul; and on the other, that all the flights of genius, and the “aspirations and sorrows” of the soul, are but the product of our material frame.

The views which, at different periods, have been entertained with regard to the nature of organic processes, have sometimes included those of intellectual life, at other times the two groups have been perfectly distinct; but although the particular notion held in respect of the one does not necessarily involve its application to the other, yet, as a general rule, that mode of considering either one which has obtained the most powerful hold upon the mind, has exerted some directive influence upon the course of ideas with regard to the other. It is our present intention to refer at first to the simplest phenomena,—those of organic Life,—or such as the man shares, in common with the animal and vegetable creation generally, with the polyp and the lichen, and which separate him and them from the inorganic world.

There have been two generic ideas, irreconcilably opposed to each other, upon this point. According to one, Life is the principle or cause of organization and organic action; according to the other, its result, the sum of its actions, or its effect. Subject to endless modifications of expression, one or the other of these ideas may be found underlying the physiology of all times of which we retain the record. As we proceed, attention will be directed to a third method in which we may conceive of Life, but for the present we shall limit ourselves to these two.

First:—Life as a principle, or cause. This has been the prevailing notion under many different forms, of which we shall notice three:

1. The most ancient idea of Life was intimately connected with the belief that all matter, as such, was inert, and incapable of activity until receiving into itself some immaterial, energizing power. This power was Life: an emanation from the One Central and Divine Life, which, using the objects of the material world as its instruments, moulded them to its own will, or in accordance with the one comprehensive plan of the universe which it inspired. The “Life that glows in star and clod” was of the same essence as that which we feel acting in our own bodies; it was far higher and nobler than the organism in which it dwelt; and, indestructible by the accidents of time and place to which the latter might be subjected, it could pass through various phases of (objective) development, giving for a while power and energy of action to the living man, and then glowing with beauty in the flowers that blossomed on his grave. These notions were prevalent during only the infancy of science,

when (as we have already pointed out) all the different branches of knowledge were cultivated and taught by the same man—at one time the priest, and at another time the sage.

Physiologically considered, Life in this sense pre-existed and pre-determined both the organism and its form. It was the poetic myth out of which the science of Biology was to rise, rather than that science in a developed form. With some modifications, this is the idea of Life to be found in the second volume placed at the head of this article. Mr. Wilkinson has, with consummate ability and considerable eloquence, written what is rather a poem upon the human body than a system of physiology. Considered as the former, his work must command attention and admiration; if looked upon as a scientific treatise, our opinion must be very different. We can most satisfactorily represent his views by the quotation of his own words:

“That which is life in humanity, that which is life in society, that which is life in persons and in the moral soul, is also life, and the only life, in the organs of the frame. Ends, motives, passions, affections, likings, loves, virtues, are human vitality, and there is none other; our being lies in cherished ends. . . . Remove these ends, and we stare without seeing, and sit in corners with hideous apathy and indecorum, miserably dishevelled and vegetalized; for life has nothing to do, and is taking its departure: as in metamorphosis, we are growing into trees, and the needy soil shall swallow us. Apply this to the body and its parts, and we find that the ends which it subserves in the order of things are its animating principles. They are not abstractions, but spirits embodied in works.

“. . . . We assume the soul, as also the existence of an imperishable humanity. It is a venerable creed, like a dawn on the peaks of thought, reddening their snows from the light of another sun,—the substance of immemorial religions, the comfort of brave simplicity, but the doubt of to-day, and the abyss of terrified science. . . . As the hand shapes the pen, and then writes with it, so the soul forms the body and then makes use of the properties resulting from the form. The connexion between the soul and the body is not more mysterious than the connexion between the pen-maker and the pen, excepting that our knowledge of the pen is so much more complete than our knowledge of the body.” (p. 344 *et seq.*)

Writing still farther upon the connexion of soul with body, Mr. Wilkinson remarks:

“We are apt, at first, to think that it is a single link or act; but this is an insufficient conception. There are as many different modes of connexion as there are wants in the soul, and organs, parts and particles in the body. There are as many different modes as there are possible species of contact in the great and the little creation. . . . This connexion of soul with body is no chaining of the living to the dead, like the horrid punishments of old times, but it is the live man freely working with the finest tools of nature—the chief musician in continual play upon the choicest instrument of music.” (p. 346 *et seq.*)

It is not our intention to criticise separately the expressions contained in the above quotations, or to point out their self-contradiction. The passages have been introduced in illustration of one manner in which the phenomena of Life may be considered; partly with the intention of forming a strong contrast to that which we shall notice in the sequel, and partly because Mr. Wilkinson is the most recent exponent of a mode of thought common among the naturalists of antiquity, but rarely met with in the present day.

This idea of Life involves either an egregious error, or a *petitio principii*:

we recognise some forms of action in all the objects of the material world, from the movements of our own limbs and the unfolding of flowers, to the direction of the magnetic needle and the simple falling of a stone; but until it can be shown that all these varied phenomena are the results of one and the same force, we do wrong in calling them by the same name. If we apply the word Life to designate activity of all kinds, we are at liberty to do so, and Paracelsus had equal right and reason to call the medicinal virtue of substances "their stars;" but we shall then have to invent some other terms to denote those processes of organic bodies which we now call vital. The question, "What is Life?" remains precisely where we found it, if we merely say that it is a special and irreducible form of action; but the question is answered incorrectly if we affirm, in the present state of science, that it is one of many forms of action, all of which are due to the working of the self-same force. The interdependence of equilibrium and motion among the several physical forces indicates their correlation, but it by no means demonstrates their identity; and the processes of organic life, as observed in the animal and the plant, are no less separable from them than from anything to which we can reasonably apply the expression—Soul.

2. Life has been held to be a distinct entity, or principle, residing in the organism, and peculiar to each class and individual; pre-existent to the development of the body, and compelling the various materials of which it is composed into accordance with its own will, or in conformity with some general but unknown laws. Paracelsus was the inaugurator of this idea, differing from that of the ancient sages in recognising the distinctness of this imaginary being from the general soul in Nature. Van Helmont and Stahl systematized this conception of the Archæus, and described its seat, clearly indicating that the body was created and maintained by and for its pleasure; and that without its presence and Will the body could do nothing, feel nothing, would be dead. Under various names this idea was dominant for a long period in the history of Physiology; at length the Archæus lost its individuality, and "animal spirits" numberless arose to take its place; a transition reminding us of the sailor's idea upon the origin of stars.

Doubtless many of the physiologists and physicians who made such constant use of the terms "animal and vital spirits," attached to such phraseology no notion of distinct entities, possessing wills and ways of their own; the words were merely used to express what we now mean by vital forces or vital properties: but others did not thus separate themselves from traditional authority, and the most extravagant explanations were given of the phenomena of health and disease, by their reference to the caprices and idiosyncrasies of these metaphysical creations.

In the present day we are by no means free from the same tendency of mind; and although we do not use precisely similar terms, nor speak of the animal spirits rising in wrath and contending with each other, we frequently hear of the "vital principle," and the "*vis medicatrix naturæ*," a phraseology which implies that these expressions are not unmixed with the idea of personality and discrimination in such abstractions. This arises very naturally from our tendency to link cause and effect in the world around us, as we find them linked in the world within, by the

agency of sensation, motive, volition, and those other faculties which constitute a self-conscious and spontaneously-acting personality.

With regard to this idea of Life, we have to remark that it is simply unnecessary, and gives no explanation of the phenomena. When the processes of vitality were less known, and more seeming disorder and apparent chance were passing constantly under notice, there was a strong disposition to refer these variations (irreducible to any general laws) to the caprices of minds in some points like our own; but now that extended observation has demonstrated order amidst ever-changing accident, and the progress of science has led us to believe that the so-called caprice and irregularity were the consequences of our want of acquaintance with the phenomena in question, we have no farther necessity for this assumption with regard to organic processes, feeling confident that they are conducted in accordance with laws and fixed principles as vast and unchanging as those which are to be observed in the orderly motions of the heavenly bodies, and in the occasional "falling star."

There was at an earlier period, but now there is not, any more reason for this impersonation of vitality than for the impersonation of gravitation, or electric force. The terms "electric fluid," and "magnetic current," still so commonly employed, indicate the presence of a very general conception that these forces have some definite and independent existence, apart from that of the material bodies through which they may be "conducted," or by which their presence is displayed. The questions whether force is but the product of, and inherent property of matter, or (as it has been expressed by one of our friends) "matter is frozen force," are beyond our power for solution; they may interest us for a time, and either notion may be made the basis of amusing theories of the universe; but what we do know is, that our knowledge of the two is mutually interdependent, and that we gain no farther insight into either by imagining their separation. If, then, by such expressions as vital principle or force, nervous fluid, plastic principle, &c., &c., we mean nothing more than the particular properties of special bodies recognisable by their proper characters, we have no objection to the phrases, provided that the organic processes of living beings are held to occupy a relation to the vital force similar to that which the processes of inorganic bodies hold to electricity and gravitation.

3. The idea of Life advanced in the 'Hints' of Coleridge, is very different from those which we have already considered, agreeing with them, however, in representing life as the cause of organization, and the determining power of organic form. The views of this author may be gathered from the following passages:

"What is Life? Were such a question proposed, we should be tempted to answer, what is *not* life that really is? If a man would analyze the meaning of his own words, and carefully distinguish his perceptions and sensations from the external cause exciting them, and at the same time from the quantity or superficies under which that cause is acting, he would instantly find himself, if we mistake not, involuntarily identifying the ideas of quality and life. . . . The most comprehensive formula to which life is reducible, would be that of the internal copula of bodies. . . . The power which discloses itself from within as a principle of *unity* in the *many*. . . . I define Life as the *principle of individuation*, or the power which unites a given *all* into a *whole* that is presupposed

by all its parts. The link that combines the two, and acts throughout both, will, of course, be defined by the *tendency to individuation*." (p. 42.)

After pointing out some of the conditions which regulate the intensity of Life, such as the number and separateness of integral parts contained in a given whole, Mr. Coleridge enters upon the illustration of his theory, a quotation from which will most fully convey his meaning:—

"If I were asked by a philosopher who had previously extended the attribute of life to the *Byssus speciosa*, and even to the crustaceous matter or outward bones of a lobster, &c., whether the ingot of gold expressed *life*, I should answer without hesitation, as the *ingot* of gold, assuredly not, for its form is accidental and *ab extra*. It may be added to or detracted from without in the least affecting the nature, state or properties in the specific matter of which the ingot consists. But as *gold*, as that special union of absolute and relative gravity, ductility, and hardness, which, wherever they are found, constitute *gold*, I should answer no less fearlessly in the affirmative. . . . Rising in the ascent, I should take, as illustrative of the second step, the various forms of crystals. . . . The third step is presented to us in those vast formations, the tracing of which generically would form the science of geology, . . . to be connected with the present order of vegetable and animal life, which constitute the fourth and last step, in these wide and comprehensive divisions. . . . In man, as the highest of the class, the individuality is not only perfected in its corporeal sense, but begins a new series beyond the appropriate limits of Physiology." (p. 48.)

There are two forms of objection which we take to this mode of considering Life. The first is that, like that of the ancient philosophers, it places under one name phenomena essentially distinct; and that we have still to discover in what respect the so-called vital differ from physical forces. There are differences beyond those of degree between the properties and actions of living organic bodies, and of inorganic; and these differences we cannot fail to recognise, although told by Mr. Coleridge that our idea of them is "grounded on a mere assumption." It is that which constitutes the *differentia* of living and dead, which we are endeavouring to comprehend, and it does not destroy that *differentia* to show that the two classes of bodies have some other tendencies in common. We must therefore reject this mode of viewing Life as a scientific process, although we may delight in feeling with poet-philosophers of antiquity, or with the great poet-philosopher of our own day, that all the activity and repose, the beauty and the life we see around us, are but varied fragmentary hints of one sublime and universal plan, the revelations of a Presence and a Power

" Whose dwelling is the light of setting suns,
And the round ocean and the living air,
And the blue sky, and in the mind of man;
A motion, and a spirit, that impels
All thinking things, all objects of all thought,
And rolls through all things."

The second objection which we take to the theory of Coleridge is, that making life identical with quality, and expressing it as "the tendency to individuation;" it refers rather to chemical constitution than to anything else. It is merely a recognition of certain particular groups of properties, by which one simple or compound body may be differentiated from another. If that which makes gold and silver differ is what Mr. Coleridge calls their life, it is what we are accustomed to term their chemical constitution. The definition of Bichat, that life is "*l'ensemble des fonc-*

tions qui résistent à la mort," is very severely criticized by the author we are considering; he finds in the expression no "other meaning than that life consists in being able to live." There is, however, in Bichat's too-much extolled and too-harshly criticized definition more than the *argumentum in circulo*; there is the idea of "resistance" to that (death) which is disintegration, or the absence of a tendency to individuation, or that tendency overcome by more simple and more powerful affinities. The ideas of resistance to disintegration and of tendency to individuation are (each necessarily implying the existence of the other) not so widely separated as they may appear at first sight, nor can we discover, as the author of the latter expression does, its vast superiority to the former.

We pass now to the second mode of considering life—viz., as the result, effect, or product of material properties and conditions. This view has been held in very different forms, but there are two generic ideas as their basis—one being, that vital phenomena are but the modification of more widely-distributed physical forces; the other, that they are absolutely irreducible either to physics or chemistry, and must be considered as the special product of a special structure—the organic body.

1. The chemical explanation of life dates back to the time of Paracelsus, who conceived first the application of chemic changes to unravel the mystery of organic processes. We have already seen that Paracelsus and his followers, though referring thus the phenomena of life to their favourite science, imagined all vital processes to be under the direction and control of a spiritual entity, the Archæus; but this idea of the Archæus was lost in the progress of time, and chemical changes of fermentation and decomposition were held to account for all the functions of vitality. Sylvius systematized these conceptions, and both he and his successors carried their speculations to the very lowest depths of absurdity.

2. Mechanical explanation of vital processes was attempted by Descartes, and the disciples of his teaching, in their endeavours to afford mathematical precision and certitude to all branches of knowledge,—Boerhaave, Sauvages, and other distinguished men, followed in this track; the most preposterous conclusions were arrived at, and confusion and disappointment were the result. According to the teachers of this class, digestion was simply a process of trituration; the circulating organs constituted an hydraulic machine, of which the heart was a suction-pump; and with complicated mathematical formulæ, the force of the heart was shown to equal 180,000 lbs.!

3. The third mode of viewing vitality is, considering it to be merely a modification of electricity. This idea is only of recent date, since the remarkable discoveries made at the close of the last century. The readiness with which electric stimulation may become the occasion of either muscular contraction or sensation, led many hastily to arrive at the conclusion, that electricity and the nerve-force (meaning by that vital force) were identical. Most ingenious and elaborate treatises have been written to prove this point, but the phenomena of electricity in the living organism are now being viewed from a different stand-point, and we are becoming acquainted with their laws.

The three forms which have successively represented the first generic

idea of life as a result of material conditions have been assumed, as one or the other physical science has made the most rapid progress and has absorbed the attention of the day. Their error is the same—the confusion of correlation with identity. The organism is a physical body, and as such is subjected to physical laws, and is the medium for displaying the action of physical forces; but it possesses and displays others for which these (physical forces) can render no account. Chemistry, electricity, and mechanics may express some of the results of vital action; they are, at the same time, the instruments and the materials employed, but they by no means explain the process, nor can they convey all the results. Vital actions and vital forces hold a relation to physical actions and physical forces similar to that which is maintained by the latter to each other. The position of a bar of iron is affected by the magnet; the attractive power of the latter may be developed from electricity or galvanism, the accompaniment of chemic change, but their properties cannot be reduced to one; the disturbance of chemic equilibrium may be displayed by physical change, by heat, light, electricity, or magnetism, but the latter are not the products of the former. So with vital action: it may be called into exercise by any of the above, it may display itself (become phenomenal) through their agency, but it is not to be considered, on the one hand, as their cause, or, on the other, as their effect. If we gain nothing by the assumption of a separable “vital principle,” we lose much by the attempt to reduce vitality to physics. Vast as the additions are which are constantly made to our knowledge of the material changes taking place in the organic body, there is a line beyond which, in this field of investigation (as in every other) the inquiring mind may press, but it is only into darkness and deep mystery, from which it must return, baffled and humbled by a consciousness of the little that is known compared with the infinity which lies beyond its grasp.

The second generic idea has assumed another form: life has been considered to be the result of material organization, but vital actions have been regarded as the special characteristics and properties of a particular structure—the organized body. It has been said, and, we believe, with perfect truth, that these functions are utterly irreducible to any physical forces, and that we must consider them as being equally distinct from the latter as they are from one another.

The recognition of vital properties may be said to have been hinted at by Glisson, but developed by Haller, and made the basis of a new era in physiology, under the able leadership of Barthez, Blumenbach, and Bichat. The expressions which have been used to define life according to this idea have been various. It has been denominated “a collection of phenomena in an organized being” (Richerand); “the organization in action” (Béclard); “the special activity of organized beings” (Dugès); “a double interior movement of composition and decomposition” (De Blainville); Bichat’s definition we have already quoted—and these are sufficient to show the general similarity of idea: some expressing life as neither cause nor effect, but merely a special collocation of phenomena; others indicating its dependence upon the organism for its existence.

M. Auguste Comte, the founder of the “Positive School,” adopts the definition of De Blainville, but adds that something more ought to be said

about the medium in which the organism is placed. In the third volume of his 'Positive Philosophy,' it is not easy to see that M. Comte clearly distinguishes between vitality and chemistry. In adopting the definition of De Blainville, life is considered as a complex interior process of composition and decomposition, and is thus made identical with its most general phenomenon. This, although in accordance with the whole system of M. Comte (inasmuch as it passes over the question "What is life?" in order to answer these two others—"What are its most general phenomena?" and "What are its laws?"), is unsatisfactory as a definition, and is open to the objection of being, at the same time, too inclusive and too exclusive. It would be difficult to apply the term "life" to every "double interior movement of composition and decomposition;" and it has no application at all to "irritability" and "sensibility," the special functions of animal vitality. In the 'Système de Politique Positive,' M. Comte clearly extricates himself from every suspicion of confounding vital phenomena with any other, and hurls somewhat angry invectives at those who have committed the mistake.

In the natural sciences (as usually understood) the method of M. Comte is that which has been followed, more or less closely, by all sincere investigators of truth since the time of Bacon and Descartes; but the theological and metaphysical beliefs which have mingled with the various branches of human knowledge are regarded by him as the modes in which the study of all sciences may be prosecuted, rather than any part of the sciences themselves. Thus he speaks of the theological and metaphysical "stages" as preparing the way for the "positive" final stage, in which the scientific man gives up all search after the causes, ends, and essences of things, as well as all belief in the existence of a Supreme Mind and Will, or in the existence of metaphysical entities, and seeks only to know the order of events and the sequence of phenomena. The deeply-lying question, Whether all our knowledge is, and ever must be, of phenomena alone? is not one the discussion of which would be appropriate in this place; but we shall endeavour in the next article to point out the manner in which M. Comte bridges over, by a "cerebral theory," the great gulf between the physical sciences and History, or the science of man's sociologic development.

With regard to the idea of Life as the result of organization and its media, we have only to observe that, although we have no acquaintance with it apart from them, they do not fill up the idea which is necessarily involved by its phenomena. The organism and the medium may be present, but the body may be dead, and neither micrologist nor chemist can tell us why. We have confined our attention mainly to those organic processes which man shares in common with the animal and the plant, and these are inexplicable as the *result* of the curious mechanism through and in which they are displayed; but the higher phenomena of animal and intellectual life transcend all references to their material embodiment, and lead us to another field of labour and another sphere for thought. The position, that life is the result of organization (says Coleridge) "seems little less strange than as if a man should say, that building, with all the included handicraft of plastering, sawing, planing, &c., were the offspring of the house; and that the mason and carpenter were the result of a suite

of chambers, with the passages and staircases that lead to them. To make A the offspring of B, when the very existence of B, as B, presupposes the existence of A, is preposterous in the *literal* sense of the word, and a consummate instance of the *hysteron proteron* in logic."

We cannot, then, consider the vegetative life as either the cause or effect of organization, but as its special property, its vital endowment, differing in kind from every other, and utterly irreducible to the laws of ordinary physical force. We conceive that this vital property holds a relation to the organic body similar to that which subsists between magnetism and the magnetic needle. As we are unable to recognise magnetism without the magnet, although we may have the needle without the magnetic action, so we are unable to recognise life without the organism, although we may have the organism without life.

If the phenomena of vegetative life are irreducible to physical and chemic laws, those of intellectual and animal vitality are equally distinct from the processes of the nutritive sphere, although changes in the latter may be the mechanical means for their development. Our minds constantly seek for unity in their explanation of all the phenomena with which they have to deal; but until we can arrive at far higher and more complete realization of the truth which we feel is one, though revealed to us now in fragments which we often try and as often fail to adjust, we are in danger of placing incorrectly the partial knowledge we have gained.

(To be continued.)

J. Russell Reynolds.

REVIEW II.

On some Diseases of Women admitting of Surgical Treatment. By ISAAC BAKER BROWN, F.R.C.S.—London, 1854. 8vo. pp. 288.

MR. BROWN remarks in his preface, "There is no branch of surgery more open to improvement than that which relates to those accidents and diseases incident to the female sex which admit of no relief except from the hand of the surgeon." It may with equal truth be stated, that no branch of medical science has of late occupied the attention of the profession more than that relating to the female sex. Whether a proportionate amount of benefit has arisen out of such inquiries, and the proceedings resulting from them, is more debatable ground, and worthy of strict investigation. Advancement there has truly been in the number and kinds of manipulations practised upon the generative organs of females; and could our forefathers know what is being done in the present day, from the simple operation of applying caustic to the os uteri to the heroic proceeding of passing instruments into the Fallopian tubes, inclusive of all the various methods of dilating the os uteri and teaching the barren womb to cease from its barrenness and perform its proper childbearing function, they would be as much astonished at the march of intellect in the medical profession as at the wonderful operations and effects of steam conveyance and machinery. They would be at a loss to conjecture the probable consequences of the more active proceedings of their successors; and might entertain some apprehension, perhaps, lest the

high tone of morality they so much admired in the weaker sex might not only not be enhanced, but perchance damaged, and a morbid desire for the investigation of "womb diseases" engendered. We are inclined to the opinion that even we ourselves might well pause to consider the effect of these innovations—to make out, if we can, how much good has resulted; what amount of injury, if any, has been inflicted, and how such injury may in future be prevented—to endeavour to ascertain whether womb diseases be in reality so frequent as would be inferred from the number of young women presenting themselves at our hospitals and dispensaries as the subjects of them, or whether they are not influenced in the description they give of their ailments by a morbid desire for the usual manipulations and examinations; whether, again, the natural desire of married females to have children does not lead them, under the prospects held out to them by "womb doctors," to submit to operations not only useless but sometimes positively injurious. Are such proceedings followed by the benefit they are stated to secure, or are they a form of professional quackery calculated seriously to injure the tone of public morals? We fear it must be acknowledged that whatever may have been the amount of good effected, a great deal of evil has also crept in; and the subject is highly deserving of the attentive consideration of every high-minded and conscientious practitioner.

In reviewing the work before us, however, we have not to deal with imaginary diseases, nor with operations admitting of question in a moral sense; but with stern realities—a class of diseases than which none can more embitter life, or be more difficult to cure; and the profession will feel themselves indebted to Mr. Brown for the energy and perseverance he has displayed in endeavouring to afford relief where the only alternative is misery.

The volume comprises thirteen chapters, under the following heads:—
1. Ruptured perineum; 2. Prolapse of the vagina; 3. Prolapse of the uterus; 4. Vesico-vaginal fistula; 5. Recto-vaginal fistula; 6. Lacerated vagina; 7. Polypus of the uterus; 8. Stone in the female bladder; 9. Vascular tumour of the meatus urinarius; 10. Imperforate hymen; 11. Encysted tumour of the labia; 12. Diseases of the rectum resulting from certain conditions of the uterus; 13. On ovarian dropsy.

Under the head of "Rupture of the Perineum," Mr. Brown describes an operation which he has many times performed with success, and produces eighteen cases, in all of which, one fatal one only excepted, a perfect cure of this troublesome accident was accomplished. It is difficult to arrive at a just conclusion as to the frequency of this occurrence, but our own experience would lead to the inference that it is very rare, and might be still more so with proper care and precaution. A careful attention to the generally accepted rules for supporting the perineum during labour will, in almost every instance, prevent the accident, or at all events limit it to an extent for which no other treatment than cleanliness and position will be required. It seldom takes place when the globular head of the child is the distending medium, unless the birth of the child be very forcible and sudden, and even then proper support will prevent it. The use of instruments is no doubt the most prolific source of ruptured perineum, and, as far as we have seen, the forceps now in such general use

(Assalini's) are the most likely instruments to produce it; they may be more easy of application, and more conveniently locked, than those recommended by Smellie, Davies, Blundell, and others; but they possess the disadvantage of having their handles widely separated from each other when applied, by which the perineum is put upon the stretch before the head comes down upon it, and is irregularly dilated by the forceps instead of uniformly by the round head of the child. We have witnessed the accident several times under the use of these instruments, and believe that forceps whose handles unite, as it were, in close contact with the presenting part of the head, and especially the vectis, are much less likely to occasion rupture of the perineum. It must, however, be admitted, that under any management whatever, rupture will sometimes occur, and in these rare cases the operation proposed by Mr. Brown appears to be very successful. We shall describe this operation in his own words, after quoting what our author says as to the means of preventing the accident:—

“Where rigidity of the perineum opposes the advance of the child, various remedies have been proposed to overcome it, as bloodletting, tartar emetic, warm fomentations, and greasy substances; but since the introduction of chloroform into practice I have never resorted to any of them, because I have found that in ten minutes, in the very worst cases, the parts have become dilatable when that agent is administered by inhalation.”

Mode of Operating.—“The patient should be placed in the position for lithotomy, the knees well bent back upon the abdomen, and all hair closely shaved off about the parts. The sides of the fissure should be held by an assistant, so as to ensure sufficient tension for the operator; a clean incision is now to be made about an inch external to the edges of and equal to the fissure in length, and sufficiently deep to reflect inwards the mucous membrane, and so to lay bare the surface as far as another incision on the inner margin. The denudation of the opposite side of the fissure is then to be practised in a similar manner, and the mucous membrane from any intermediate portion of the recto-vaginal septum to be also pared away. This denudation must be perfect, for the slightest remnant of mucous membrane will most certainly establish a fistulous opening when the rest of the surfaces have united. So soon as this stage of the operation is completed the sphincter ani is to be divided on both sides, about a quarter of an inch in front of its attachment to the os coccygis, by an incision carried outwards and backwards. The incision should be made by a blunt-pointed straight bistoury, which, having been introduced within the margin of the anus, guided by the forefinger of the left hand, is quickly and firmly carried through the fibres of the muscle and through the skin and subcutaneous areolar tissue to the extent of an inch, or even two, external to the anal orifice. The degree of relaxation to be sought must be regulated by the extent and character of the laceration; it being remembered, that the freer the incision the greater will be the amount of relaxation obtained. In every case muscular traction must be destroyed, for so long as it exists it will oppose the union of the parts.

“The sphincter having been divided in the manner just stated, the thighs are to be approximated, and then the quill sutures introduced. The left denuded surface and tissues external to it being firmly grasped between the forefinger and thumb of the left hand, a strong needle carrying a double thread is plunged, with the right hand, through the skin and subjacent tissue an inch external to the pared surface, and thrust downwards and inwards beneath it until its point reappears on the edge of that surface; it is then introduced at the corresponding margin of the denuded space of the opposite side, and made to traverse beneath it in a direction upwards and outwards until it escapes at a point equi-distant from

the external margin with that at which it entered on the left side. Each of the three sutures is to be introduced in the same way, the one nearest the rectum first. The sutures are double, to allow them to enclose the quills, or (as actually used) the pieces of elastic catheter or bougie, around which they loop on one side and are tied over by their free ends on the other. For sutures I prefer stout twine, well waxed, to silk, as I believe it to be less irritating and productive of less suppuration.

"Having firmly secured the three sutures upon the bougies, the sides of the fissure become approximated—the denuded surfaces in apposition. To bring together the outer margins, along the line of the skin, it is advisable to pass three or four interrupted sutures. If this be carefully done, union of the skin will speedily take place, and that of the deeper parts be materially facilitated. I should recommend, previously to bringing the operation to a close, that the forefinger of the right hand should be passed into the vagina, and that of the left into the rectum, so as to ascertain that apposition is complete throughout. Lastly, the parts having been well cleansed by sponging with cold water, a piece of lint steeped in cold water is applied, and over it a napkin, kept *in situ* by a T bandage.

"The patient having been removed to her bed, should be placed on her left side on a water cushion, with the thighs and knees close together, and flexed on the abdomen. Perfect quiet enjoined, and cold-water dressing continued. Ice given to suck for twenty-four hours is refreshing, and allays febrile reaction and nausea. Two grains of opium should be given at once, and one grain repeated every four or six hours. Beef-tea and arrowroot may be given within the first twenty-four hours, but not wine, unless there are signs of flagging. After the first day, four ounces of port wine may be allowed; and a generous diet, chops, strong beef-tea, &c., after the second or third day. It is of great importance to draw off the urine by the catheter every four or six hours, for three or four or more days after the operation. On the eighth or ninth day, if the healing go on satisfactorily and the strength of the patient be equal to it, she may be allowed to pass water, resting on the hands and knees, so as to prevent, as far as possible, its contact with the lower or sutured surface of the vagina. The deep sutures should be removed on the third or fourth day in hospital patients; in private cases, on the fifth or sixth. I have found their retention after the periods named of no service, but rather mischievous, by their tendency to suppurate and slough—results of more rapid occurrence in hospitals than elsewhere, hence the earlier date proposed for their removal in hospital cases. On the sixth or seventh day the *external* sutures may be taken away: care must be taken not to separate the thighs, for it is necessary to keep up their apposition for some time. The opium should be persevered with, so as to keep the bowels constipated for two or three weeks after the parts have united. When union has become firm and complete, the bowels may be relieved by injections of warm water with castor oil, and by the latter given by the mouth. Attention should be paid during the passage of the first evacuation, and support given to the restored perineum if any hardened masses should cause stretching. Should adhesion, from any accident, not be complete throughout, and a fistulous opening persist, the actual cautery is the quickest and surest means of closing it; but the application of a caustic or stimulating substance may be tried." (pp. 35 *et seq.*)

We have thus given our readers a detailed account of the operation as applied to cases of some standing, where cicatrization has occurred; with respect to recent cases of the accident, the only variation of the plan is in the omission of the otherwise necessary denudation of the margins of the fissure. The chief novelty in Mr. Brown's operation is the division of the sphincter ani muscle, in lieu of Dieffenbach's elliptic incisions on either side the median line, to prevent too much stress upon the sutures.

In speaking of the surgical history of the subject, our author has not given sufficient credit to the Germans for their knowledge of this accident and its treatment. Siebold, in his '*Handbuch zur Erkenntnitz und Heilung der Frauenzimmerkrankheiten*,' dated 1826, gives a full description of the causes, degrees, effects, and treatment, of lacerated perineum. To promote the healing of the wound, in slight cases, he recommends the patient being placed upon her side, inclined more forwards than usual, in order that the lochia may flow anterior to the wound instead of over it, with the thighs tied close together. The urine to be passed kneeling. Clysters and aperients, if required, to prevent too much effort at stool; and great attention to cleanliness. If the rent extends to the sphincter ani, or through it, he advises an operation, consisting of paring the edges of the fissure as for hare lip, and uniting them by sutures, and if necessary, denuding also the recto-vaginal septum and including it and the external coat of the rectum itself in the ligatures. The bowels are to be well relieved and the diet regulated the day before the operation; and afterwards, one or two grains of opium are to be given, to prevent action of the bowels during the first few days. The after-treatment consists of keeping the patient several days upon her side in bed, with the thighs tied together, drawing off the urine frequently with the catheter, and giving meat broth and fluid nourishment. On the fourth day the wound is to be examined and cleaned, and caustic applied to any portion of the wound not found to be united.

In Busch and Moser's '*Handbuch*,'* under the head of "*Damm*," there is an elaborate article on the perineum, including an anatomical description of the parts, and the nature, causes, and effects of rupture, much in the same order as in Mr. Brown's work. The article occupies forty-five closely printed pages, and contains a great deal of information upon the subject. The various operations for the cure of rupture of the perineum are described at length, and a just comparison made of the different methods employed. The means of preventing the accident, and the after-treatment after operation, are also dwelt upon, and the literature of the subject discussed. No mention is made, however, of division of the sphincter ani muscle as a part of the operation; but when the parts are too tight, to prevent the risk of the sutures ulcerating through, the incisions of Dieffenbach are recommended. The plan of giving opium to prevent any action of the bowels for several days after the operation, is considered useless and even injurious, and the use of clysters of thin fluid to keep the rectum free, much preferred. We are disposed to think, from the perusal of Mr. Brown's reported cases, that, in some of them, less febrile and sympathetic disturbance would have followed the operation if the bowels had been carefully emptied by means of injections; and can scarcely understand why this should be more objectionable, if cautiously managed, than "injecting tepid water three or four times a-day into the vagina, to ensure cleanliness, and to prevent the irritation of the united surfaces by the lochia," especially after the resistance of the sphincter ani has been removed by the division of that muscle. But it is due to Mr. Brown to state that he makes a great point of keeping the bowels perfectly quiet, allowing no action; and considers that the opium

* Vol. ii. p. 1 et seq.

“proves actually beneficial by allaying irritation, by controlling inflammation, and by generally favouring the healing process.”

Mr. Brown considers his method of treating ruptured perineum applicable in all instances advanced to 'the extreme degree; that the worst forms of laceration, of however long standing, may be cured by the operation; that immediately on the occurrence of the accident it should be resorted to; and that subsequent parturition is possible without injury to the restored perineum; and these several propositions are in a great degree established by the very interesting and successful cases he has detailed.

The next subject treated of is Prolapse of the Vagina, which, if it be of long standing, and occur in females beyond the period of child-bearing, is to be cured by an operation which will be sufficiently described in the history of the following case of vaginal cystocele:

“CASE.—M. J., aged 52, has had ten children. She was admitted in St. Mary's Hospital, Feb. 14, 1853, suffering from severe prolapsus of the vagina and bladder, which first began to trouble her nine years ago, after her last labour. On the least exertion of walking, or even standing, or coughing in the recumbent position, the tumour came down and protruded through the external orifice of the vagina, to the size of a large fist. On lifting up this tumour, when so extended, there were seen, on the under and posterior surface of the os uteri, which was dragged down by the vagina, two or three ulcerated spots, produced by friction against the posterior wall of the vagina. The patient could, when reclining on her back, replace the tumour. She had a cough from chronic bronchitis, which she generally had in winter, complained of feeling weak, and her appetite was capricious. This patient having been prepared, by emptying the bowels, was, on Feb. 15, placed under the influence of chloroform, and then put in the position for lithotomy, each leg being held by an assistant, a third assistant holding up the tumour with Jobert's bent speculum, and pressing it under the pubes in its natural position. A piece of mucous membrane, about an inch and a quarter long, and three quarters of an inch broad, was dissected off longitudinally, just within the lips of the vagina. The upper edge of the denuded part being on a level with the meatus urinarius, the edges were drawn together by three interrupted sutures, this being repeated on the other side of the vagina. The next stage of the operation consisted in dissecting off the mucous membrane, laterally and posteriorly, in the shape of a horse-shoe, the upper edge of the shoe commencing half an inch below the lateral points of denudation, taking care to remove all the mucous membrane up to the edge of the vagina where the skin joins it. Two deep sutures of twine were then introduced, about an inch from the margin of the left side of the vagina, and brought out at the inner edge of the denuded surface of the same side, and again introduced at the inner edge of the pared surface of the right side, and brought out an inch from its margin; thus bringing the two vascular surfaces together, which were then kept so by means of quills, as in the operation for ruptured perineum. The edges of the new perineum were lastly united by interrupted sutures, and the patient placed in bed on a water-cushion. Two grains of opium were given directly, and one grain every six hours; simple water-dressing applied to the parts; beef-tea and wine for diet. A bent metallic catheter, to which was attached an elastic bag to catch the urine, was introduced into the bladder; by this means the bladder was constantly kept empty. This patient progressed satisfactorily from day to day without a single bad symptom; and on the 22nd, the deep sutures were removed, and the parts were found firmly united. The lateral interrupted sutures were gradually removed, and firm union found to have resulted.

“Feb. 26. The deep union was perfectly sound, about three quarters of an inch thick, the lateral wounds well contracted; the tumour could not be brought down by coughing.

"March 8. The parts were all firmly healed; the patient was much improved in health, with a very cheerful aspect of countenance. She could walk about without inconvenience, and no amount of exertion produced any prolapsus. She could empty her bladder with comfort; and all the leucorrhœal discharge, which was so distressing before the operation, had entirely subsided; the offensive smell of the urine had also departed. On passing the finger into the vagina, the os uteri could be easily felt in its normal position, and the ulcerated spots which formerly existed on its surface were healed.

"On the 10th she was discharged, cured, and resumed her duties as domestic servant."

This was a very successful case; and the principle of this operation is said to be equally applicable to the cure of prolapse both of the posterior wall and of the entire circumference of the vagina; and also, with some slight modifications, to the relief of prolapsus uteri.

Vesico-vaginal Fistula.—On this subject but little new light is thrown by Mr. Brown's experience and practice; neither have his attempts at cure, although highly praiseworthy for the patience and perseverance they display in a good cause, been much more successful than those of others who have preceded him. His own views are well stated in his concluding remarks, in which he says—

"The preceding cases will illustrate the various points of difficulty which are met with in the treatment of this distressing lesion; and although they do not exhibit a great amount of success, they may fairly be looked upon as valuable illustrations of our present knowledge and practice; and I still look forward to a greater amount of success, by steady observation and persevering efforts, which the late improvements in surgical science certainly justify, especially as the difficulties are rather mechanical than pathological. It cannot be concealed, however, that it requires no ordinary amount of perseverance and determination to bear up under the vexatious disappointments which are constantly occurring in the hands of the most painstaking operators."

The mode of operating preferred by Mr. Brown is, after paring the edges of the wound, to employ the form of suture used by Dr. Sims of Boston, U.S., called the "clamp" suture, a full description of which will be found at p. 93 *et seq.* of the work before us. Before taking leave of this subject, we cannot but express our concurrence in the following observations respecting the prevention of such a horrible calamity, borne out as they undoubtedly are by our own individual experience:

"As far as my experience goes," Mr. Brown observes, "the prevention of this lesion is very much under the control of the accoucheur; and I cannot but consider that, with ordinary care, by keeping the bladder empty, and still more, by never allowing the head to remain long in its passage through the os externum, this serious injury would not so often occur. I am aware that in thus advocating the early delivery of the head, I am opposed to many of the most eminent obstetric writers. Still, when I reflect on the very many cases which have come under my notice, and find that in almost every case this accident has occurred after protracted delivery, I am strengthened in my own opinion."

The distressing and repulsive nature of the consequences of this accident, rendering the innocent sufferer a burden to herself, and an object of disgust to others, though she may be, at the same time, *in all other respects*, healthy, and capable of enjoying the blessings and comforts of this life; perhaps, too, of a constitution that may be tenacious of life, supporting her for a considerable length of time, in spite of the mental agony and

bodily discomfort she is doomed to endure; these considerations, we repeat, ought always to be present to the mind of an accoucheur, leading him to the minute observance of every preventive means which careful attention to the subject can suggest, in order to avert so direful a calamity. And we unhesitatingly believe that no circumstance is more to be dreaded as a cause of the disaster, than an unusually long-continued pressure upon the soft parts by a firmly-impacted child's head.

A Recto-vaginal Fistula is a somewhat more manageable disease than the preceding, and our author introduces three successful cases; two of which he treated with the actual cautery, and the other by passing a piece of lint through the opening, and allowing it to remain until it caused inflammation sufficient to produce a healthy granulating surface. In the after treatment of all these operations, great stress is laid upon keeping the bowels confined by the frequent administration of opium.

The chapter on Polypus of the Uterus is introduced principally for the sake of advocating a modification of the usual form of operation—namely, that of removing the polypus, either by a pair of curved scissors or a blunt-pointed bistoury, immediately after the ligature has been applied. The object of this is to prevent the absorption of secretion from the putrid mass, and the consequences of such absorption. It is obvious, however, that even here some portion of putrid substance must remain in contact with that still possessed of vitality, until the ligature has come away. And when we bear in mind that Gooch's beautifully simple canula can be applied without any forcible alteration in the position of the uterus, or dragging down of its interior to bring the pedicle within the reach of instruments (we here refer more particularly to those polypi which are situated within the cavity of the uterus, and attached near the fundus), there is probably no safer or more effectual operation for the removal of such tumours than the one recommended by Gooch, especially if the ligature be composed of what is called "gold twist," which will cut through a pedicle of even large size in a very short period of time. If the tumour be removed from the vagina as soon as the ligature is away, and the vagina carefully washed out by means of a syringe, as often as required, the chance of the operation being followed by purulent absorption and phlebitis, is probably less than that of evil consequences arising from the disturbance to the uterus necessarily occasioned by seizing the polypus with a pair of vulsellum forceps, and bringing it down within the reach of manipulation such as our author describes.

In his observations on Imperforate Hymen, Mr. Brown is of opinion, that the entire removal of the hymen, by a circular incision at the point of its junction with the labia, is the best way of operating; and suggests, that when the usual crucial or stellate incision is performed, vaginitis may be set up by the friction of the divided portions upon each other, produced by every movement of the body. It is difficult to determine why such an apparently simple operation as division of the hymen should be so frequently attended with serious and sometimes fatal peritoneal inflammation. Langenbec attributes the tendency to inflammation to long retention of the menses; and, with reference to a case reported in a recent publication,* the writer of this article raised the question, whether

* Cases 1 Midwifery. By J. G. Crosse, M.D., &c., p. 144.

in such cases it might not be advisable to make a simple puncture to evacuate slowly the retained fluid, and leave the patient to recover entirely from the effect of retention of the menses, before any other means are taken to open fully the passage into the vagina?

We have already carried our observations to such a length, that we must now refer our readers to the work itself for further information of its contents; and we can do this with the more satisfaction, because we believe it to be well worthy of attentive perusal. The concluding chapter, on Ovarian Dropsy, though containing but little of novelty, presents an exceedingly good general view of what is known upon the subject up to the present period. And in closing our remarks, we feel bound to state, that whatever may be the intrinsic value of our author's opinions and practice, both of which are entitled to much consideration, we can scarcely praise too highly the extreme care and attention he devotes to every minute circumstance that appears likely to promote success in his operations. If operations of a delicate nature, performed upon parts easily susceptible of injury, are to be successful, the patient must not only be prepared beforehand, and judiciously managed afterwards, but all unnecessary waste of time during the operation, all needless manipulation, all uncalled-for exposure, must be scrupulously avoided; and in all these points, before, during, and after operations, Mr. Brown appears to exercise that diligence, carefulness, and perseverance so essential to aid the surgeon in bringing his cases to a prosperous and satisfactory termination.

E. Copeman.

REVIEW III.

Handbuch der Speciellen Pathologie und Thérapie. Redigirt von RUD. VIRCHOW. Band 1, Heft 1.—Erlangen, 1853.

Manual of Special Pathology and Therapeutics. By R. VIRCHOW. (The Chapters on *Fever and Inflammation*.)

SINCE the earliest periods in the history of Medicine, the two most constant and singularly remarkable morbid processes manifested by the human body -- those which have most strongly arrested attention, which have most invited observation, and which, as inquiries became multiplied, and the elements of Medicine as a science were gradually brought together out of the chaos of isolated facts, gave rise to the most profound theories and abstruse speculations of its various schools and professors—are, without question, the groups of diseased phenomena which, to the lasting detriment of science and the long-continued impediment to the advance of true knowledge, were early designated by the metaphorical names—Fever and Inflammation. Contrary to what might be at first supposed, the human mind, in many of its earlier efforts in various branches of inquiry, would appear to have attempted to grapple rather with the abstract than the concrete; to have overlooked, or, at least, to have given but little attention to the immediate, the positive, and the tangible, which it valued and used chiefly as furnishing the basis for speculative opinions, imaginative and often fanciful and extravagant theories. In this speculative process, the ideas formed about one subject of inquiry were eagerly grasped at to illustrate or explain those

of another, when any parallelism of phenomena or resemblance in conditions, however non-essential in either or both, could be traced. To this source is undoubtedly to be attributed the vast number of metaphorical and abstract terms so common in all languages, and which so particularly abound in the nomenclature of medicine. Thus, one set of phenomena, about whose nature, connexion, and causation, no definite knowledge existed, was freely used to illustrate, explain, and, in many instances, to give names to another set, till a metaphorical verbiage came extensively to supply the place of positive ideas; while ignorance and error lay concealed beneath a false, unreal, and superficial science that dealt only in words. But let us not take credit to ourselves for greater wisdom in our generation than we are entitled to. To this day the consequences of this false system of acquiring knowledge remain. It cannot be denied that the same erroneous direction of mind still prevails, if not generally yet only too extensively, and perhaps more so in medicine than in any other department of human knowledge.

The constancy of their phenomena, and the frequency, extent, and great influence of these two diseased processes—Fever and Inflammation—on human life, soon engaged attention. One phenomenon they possessed in common—that of increased temperature; the most obvious parallel for this was to be found in the heat produced by fire, and in this resemblance originated the names which they have since retained in all languages. In many respects the two processes presented great and striking points of resemblance to each other; it must likewise have been soon found that they often co-existed; and it is only to be wondered at that they so early came to be recognised as distinct. In the combination of both, a kind of debatable territory was found; and in the term “inflammatory fever,” still retained, may be yet seen the immense difficulties, if not impossibility, of assigning absolute limits to each.

In all times, these two great morbid processes have engaged the ablest minds ever devoted to the science of medicine; theory, observation, and experiment have been enlisted together to elucidate their phenomena, and each new addition to the various agents of scientific medical research has been brought to bear on their investigation. How far we have advanced in this inquiry, how far our knowledge of these great diseases may be now regarded as definite, complete, and satisfactory, and answering the requirements of our science, after the combined labours of modern physiologists, pathologists, and clinical investigators have been devoted to them: after the brilliant results which have been obtained by chemistry and histology, we shall place before our readers in a critical examination of the pages now about to be brought under consideration, which emanate from one of the master-minds of modern medicine, himself an original inquirer of the highest order, and one than whom, perhaps, no other living is so well qualified to appreciate and utilize the contributions from all branches of medical science which pour in to elucidate the pathology of fever and inflammation.

Of all the various phenomena presented by fever, that of the elevation of the temperature of the body, in whole or part, is, perhaps, the most constant and remarkable, and has also attracted most inquiry. With Galen, the *calor præter naturam* was an essential phenomenon; many have, however, refused to recognise it as either constant or essential. While its

determination rested on the fallible evidence of sensation and the application of the hand, it was obviously impossible that the question could receive any very definite solution. De Haen was the first to employ the thermometer to ascertain the temperature in fever, and his observations went to show that, even in the rigors, there was an increase of temperature, in some cases even so much as 2, 3, and even 4° R.; and that the slightest febrile conditions are attended with an increase of heat, which is, also, sometimes the only observable phenomenon. Gierse, Roger, Traube, Zimmerman, Baresprung, and others, have repeated and confirmed these experiments: so that Virchow remarks, it is no longer doubtful that, in the *calor præter naturam*, as stated by Galen, is the substance of fevers. The rigor is but a peripheral phenomenon, and while the outer parts are frozen, the inner burn. The blood is to be regarded as the seat of the febrile temperature, and the more blood a part contains, the warmer it becomes, and thus the term "fever" may be regarded as not altogether figurative, febrile or pyretic diseases being those in which an increased combustion takes place. We shall not stop here to examine the various theories which, from age to age, were adopted to explain the phenomenon of the increase of temperature in fever. Certain physical conditions, such as increased velocity and friction of the blood-particles, cannot be altogether excluded from consideration; but it must, in the present state of science, be admitted, that the chief source of increased temperature in the human body is to be sought in the chemical development of heat attendant on the nutritive process; and to the increased consumption of organic material is to be attributed any increase of temperature. But in fever, it is not alone the materials supplied by nutrition that are subject to combustion, but also actual constituent elements of the body; not only the fluids, such as blood, parenchymatous fluids, oil, and fat, but even the solids, as muscles, glands, and bones.

The increased consumption of material has also been assigned to an increased respiratory action as its cause, the exhaled carbonic acid being taken as the standard of measurement; and the few exact observations which have been made, appear to show that there is an actual increase in the process of oxidation. This, however, is but an imperfect mode of estimating the amount of oxidation, for many organic substances become oxidized without the production of carbonic acid; on the other hand, Moleschott has well remarked, that the origin of heat in the body is not to be solely attributed to the combination of oxygen with organic materials, but that in the separation of bodies which become only in part oxidized, is to be found a rich source for the production of animal heat.

If, with Virchow, we regard heat as the pathognomonic symptom of fever, and consider it to be dependent on an increased consumption of the constituents of the body, we have yet to seek the cause of this increased action in the system. This cause, the *causa proxima* of fever, Virchow regards as internal, and connected with the body itself, in contradistinction to the external causes acting on the body from without. Various changes in the constitution of the blood, productive of a *materies acris*, fever-stuff, the pyretogen of Eisenmann, have been assumed as the cause of fever; and while we may hold that, in many cases, some such change is actually produced, we are not yet in a position either to determine its precise nature or to understand how it is itself brought about. Some facts, determined by recent

experiment, would appear to show that the proximate cause is not to be sought in the blood alone, and to point to deranged conditions of the nervous apparatus as more likely to furnish us with a solution of this question.

There is no reason for seeking other sources for the fever heat than those which furnish the natural heat; only it may be considered, that in fever, the usual processes are in excess, and this excess may be traced to either of two sources. We may suppose that certain nerves preside over the development of heat, or that its production is moderated by certain nerves; in the former case the febrile heat is to be attributed to an increased, in the latter to a diminished innervation. The experiments of Becquerel and Breschet, and Helmholtz, show that nervous excitation can produce muscular movement, and that this is attended by increase of temperature. In opposition to this, Bernard has proved, that section of the sympathetic in the neck is followed by rapid increase of temperature in the corresponding half of the head. Taking into account the various phenomena which attend the onset of a fever, characterized, as it is, by weakness, prostration, and loss of energy, especially in the muscles and sensitive parts, Virchow concludes that the elevation of temperature, which is found to be amongst the earliest and most constant of the symptoms, is to be regarded as a paralytic phenomenon, produced by loss of power in the nerves, which constitute the natural moderators of the development of heat. With regard to this moderator-function but little is known, and it seems to be equally assignable to opposite portions of the nervous system; there appears, however, to be considerable reason for selecting the *vagus* as one of the most important agents in the production of the febrile phenomena. Besides the elevated temperature, the alterations in the circulation, and the movements of the heart, the lesions of digestion, anorexia, vomiting, &c., as well remarked by Virchow, fall within the domain of the physiological influence of the *vagus*. E. H. Weber has shown that irritation of the *vagi* causes an arrest of the heart's action; while it is long known that, after section of these nerves, a remarkable acceleration of the pulse takes place. The lesions of the heart, lungs, and stomach thus fall into one category, and, as observed by Traube, a large number of the symptoms of fever may be referred to a weakened, more or less paralytic, condition of the *vagus*. This observer has found that *digitalis* acts as a stimulant on the regulator nerves of the heart, and concludes that the diminution of temperature which he has noticed after its employment is produced by the diminished velocity of the blood-stream. Ludwig and Hoffa have ascertained that, by moderate irritation of the *vagi*, the lateral pressure of the blood in the arteries is lessened; while Volkmann and Fowelin have shown that it is increased after section of these nerves. These very remarkable physiological experiments demand especial consideration, as they not only much modify our previous views of nervous action, but supply new and important therapeutic indications. It is to be observed, however, in connexion with the experiments just detailed, that those of Lichtenfels and Frölich appear to show that, in the normal condition, the frequency of the pulse and the temperature are independent of each other; Virchow, therefore, concludes that the fever temperature is an essentially compound phenomenon, dependent on nervous, physical, and also nutritive conditions. In estimating the conditions of the circu-

lation, some points here noted are worthy of consideration; the heart's contractions will be found an imperfect criterion of the state of the circulation in many instances, as rapidity of its beats is often found to be attended by an incompleteness and weakness of its action; and so imperfect may the single contractions be, that the mass of the blood, instead of moving with increased velocity, is retarded, and flows more slowly. 'The state of the circulation is, therefore, to be estimated not so much by the energy and number of the heart's contractions, as by the general tension of the whole vascular system. The question of the tone of the vessels, and its alteration in fever, has been often already considered, and a large part assigned to the vaso-motor nerves in the production of such changes. In regard to the phenomena under consideration, the cerebro-spinal and the sympathetic nerves appear to present somewhat different properties: thus, in paralysis of the former, there is always a diminution of temperature in the parts supplied by them; while the sympathetic nerves retain their power even in cases of complete paralysis of the cerebro-spinal system, and this power may be increased when the branches connecting them with the spinal marrow are paralyzed, and this may likewise be accompanied with an increase of temperature. If any centre for this regulating nervous force is to be sought, it would appear, most probably, to reside in the medulla oblongata, near the roots of the vagi.

Fever, then, in the opinion of Virchow, essentially consists in an increase of temperature, which is caused by an increased consumption of material, and appears to have its origin in changes in the nervous system; these changes may be considered to affect the regulating or moderating function of the nerves, and thus to be the cause of the increased consumption of material. He believes that every given disease and every lesion may pass into a fever if it invades the regulating centres of the consumption of nutritive material. If these centres are to be sought in the nervous system, an abnormal condition of tension must be admitted, which is called into play by the causes exciting the fever, and which finds no solution in the natural processes. As the force of the moderating centres is arrested, the consumption of material advances, and, in proportion, the heat of the body increases, and the particular point of the commencement of fever is reached. At first we see only the weakening of the corporeal and mental powers which follows directly on the condition of tension.

On the subjects of crisis and critical days, medical opinion has undergone much change in modern times; many have gone so far as to reject altogether the belief in critical days, and to regard the crisis, when it occurred, rather as the result than the cause of the cure. Recent observations, especially those with the thermometer, show that the excretions in the course of febrile diseases have very various significations; that they may be regarded as co-effects of the lesion, being sometimes without influence on its termination; sometimes accompanied by exacerbation, when they may be called symptomatic; and sometimes appearing to be the means of resolution, when they may be called critical; and, lastly, they may occur subsequent to the period of resolution, and as an enforcement of it, when they will be designated post-critical. Thermometric measurements would appear, however, to lend considerable support to the doctrine of critical days.

Such is a brief outline of the views which now appear most tenable with

regard to the nature of fever; much is yet left undefined, and there is, also, much which will present itself to the mind of every practical physician as difficult to be brought within these limits. To those especially who have pursued close and accurate clinical studies of typhus fever, many points in its pathology will, doubtless, appear to be incapable of solution by the views of Virchow.

Of the indications for the treatment of fever dwelt on by Virchow, we shall here notice only those directed to the nervous system. These embrace the use of the so-called nervine medicines, to which belong digitalis, quinine, arsenic, and a number of other substances, chiefly from the group of vegetable alkaloids and the metals. The action of many of these medicines in lowering the temperature in fever has been fully established. As already noticed, Traube has shown it to be probable that digitalis acts on the medulla oblongata and the roots of the vagi. Duméril, Demarquay, and Lecointe have sought to prove that quinine acts directly and powerfully on the sympathetic system, as already suspected by Lobstein. These results are yet but small; still, in our minds, they foreshadow the approach of a rational and scientific system of therapeutics for fever.

INFLAMMATION constitutes in some respects the analogue, in others the opposite, of Fever; next to fever it must be considered the most remarkable of all pathological processes. Virchow describes it as one of the general forms of phenomena under which a number of most different local diseases may manifest themselves. As in fever, the most remarkable phenomenon of inflammation is an elevation of temperature, which in the former was considered to be general, while in the latter it is only local. We shall omit here all notice of the various doctrines held as to the essential characters of inflammation, from the symptomatic quatrain of Celsus to the fluxus, stasis, constriction of the capillaries, obstruction of the blood, and *error loci* of the globules. These points will be found discussed at length in the original. Two most opposite conditions of the bloodvessels, spasmodic action, and paralysis of their walls, have been assigned as the cause of some of the most essential phenomena of inflammation, and each has received the support of able investigators. It is now, however, pretty generally agreed on, at all sides, that the local alterations of the circulation, in themselves neither simple nor uniform, are insufficient to define inflammation; it may be stated that not any one of the many phenomena assumed by various observers to be essential, is constant, and that even opposite states of the blood and bloodvessels may present themselves in succession within a short period in the same inflamed spot. The experiments of H. Weber, in particular, show that the paralytic as well as the spasmodic states of the vascular walls are of secondary importance in relation to the stasis: and, on the other hand, lend much weight to the so-called attractive theory. This theory is based on certain physical relations found to exist between the blood, the vascular walls, and the parenchyma. In estimating this attractive force, we have to take into account many necessary physical conditions of the circulating fluid itself, as to viscosity, cohesion of its particles, and the molecular attraction exercised between them and the walls of the vessels. In accordance with these principles, it may be stated in a general way that the more the blood adheres together, and the less it is attracted to the vascular walls,

the more easily will it pass the capillaries; but the more its cohesion becomes diminished, and the greater the molecular attraction between its particles and the surrounding walls, the more readily will it stagnate. This, then, is the basis of the attractive theory. Thus an inflammatory irritant may attack a certain tissue, produce in it chemical or physical changes, and in this way induce an altered molecular attraction of the blood, an endosmotic or exosmotic current, or a change in its internal cohesion, and therewith a scattering of the corpuscles throughout the stream, an attraction of them to the walls of the vessels, and thus finally an obstruction to the current. Thus considered, Virchow accepts the attractive theory as capable of explaining the stasis, but not therefore the inflammation.

The phenomena of stasis have been frequently and accurately observed, and as well described, so that we need not here repeat the details of the diminished velocity, partial intermission and oscillation of the stream, gradual disappearance of the intercellular fluid, relative increase of the blood-corpuscles, and their lessened diameter, till they fill the entire vessel, and the whole mass stands still. Coagulation of the fibrine has been asserted to be one of the first and most necessary steps (Cruveilhier); but so far is this from being the case, that Paget has found the blood still fluid after three days' stasis. The most constant phenomenon of stasis is the thickening of the blood, and this is induced by the loss of the watery constituents of the plasma.

But the essence of inflammation is not to be found in the local lesions of the circulation, and we must therefore take into consideration the changes in the surrounding tissues, which also participate in the inflammatory action. The most important question connected herewith is that of the seat and nature of the *exudation*, about which much misconception has prevailed. Virchow has shown that the exudation is to be regarded only as the nutritive fluid which has passed from the blood into the elements of the tissues, and is not to be separated from them or isolated—which has in fact become part of these elements, being received within their cells or basic substance, and which can hardly be regarded as distinct from them. The exudation must not be regarded as the proper product of the inflammation, which is rather constituted by the materials resulting from the destruction of the tissues, though it cannot be denied that the exudation itself may undergo changes.

The origin of the exudation is traceable to two sources. It may be regarded as the expressed blood-fluid resulting from the mechanical pressure under which the blood moves in the finer vessels; or it may be considered as the result of an increased attraction of the blood-elements by the tissues, an increased diffusion of material, in fine, as a nutritive educt. These views require some modification in regard to the free superficial exudations, which have some similarity to secretions. Virchow gives this subject a special examination, in which we regret we cannot follow him.

In the opinion of our author, then, the inflammatory process in its proper essence is to be regarded as a local lesion of nutrition (a view, as it appears to us, long shared by others), and, like all local lesions of nutrition, depends on the interchange of materials between the blood and tissues.

It yet, however, remains to be inquired, how this local lesion of nutrition differs from others, as all such lesions manifestly do not constitute inflammations. The local elevation of temperature has been long regarded as the criterion and proper clinical expression of the inflammatory process, and, as is well known, formed one of its four cardinal phenomena in the symptomatology of Celsus. Experiment and observation give somewhat conflicting results as to the exact conditions of temperature in inflamed parts; the weight of argument, however, appears to bear on the side of an absolute increase; this has been determined in one case by Hunter, and more recently by the thermo-electric experiments of Becquerel and Breschet. Virchow considers it highly probable that heat is actually produced in inflamed parts, but that the temperature of the foci of inflammation is to be regarded as the expression of two distinct sources of heat, one of which is to be sought in the blood, and the other in the parts themselves. The local elevation of temperature produced by the influx of blood, especially of heated febrile blood, must on its side contribute essentially to the increase of the local metamorphosis of tissue. The nutritive processes must certainly be influenced by an elevation of 2° or 3° R. in the temperature of the parts in which they are taking place, and new chemical relations must be formed.

Looking to the assemblage of phenomena which are combined in the inflammatory process, it will be seen, says Virchow, that they exhibit a very close relation to those of irritation; and that between inflammation and irritation only a quantitative difference is to be found, the former following step for step on the latter, and their phenomena being distinguished only by the characters of relative greatness and extent. The phenomena of irritation, though also dependent on material changes of the parts, have usually more a functional dynamic character; those of inflammation depend more on nutritive lesions. As long as the lesion continues functional, we speak of irritation; when, however, it extends to nutrition we call it inflammation.

In the rapidity and excessive force with which the nutritive changes are wrought, will be found a characteristic of the inflammatory process which distinguishes it from ordinary lesions of nutrition, and gives it the appearance of an increased combustion of material; to this may likewise be added the increase of temperature of the exudation, which has been shown to be essentially but an increase of the natural nutritive fluid passing out from the blood to the tissues, and becoming incorporated with their elements: only a portion of it is, therefore, to be ascribed to the inflammatory action as its origin.

Another important question in connexion with the phenomena of inflammation, is that of hyperinose (Franz Simon), or, as it is more intelligibly named, increase of fibrine. It is unnecessary here to pass in review the various opinions with regard to slowly coagulating and hyper-oxidized fibrine, the oxyprotein and bradyfibrin of authors. Various kinds of fibrine have been assumed to exist, and also various modes of disease in fibrine. Virchow has long been an antagonist of these views as to chemical differences in this substance, all the observed differences in the mode of coagulation being referrible to mechanical conditions, such as the viscosity of the blood-fluids, and the relative quantity of the

fibrine. This substance is usually submitted to examination in its coagulated state, and much of the difficulty of exactly determining its nature depends on this circumstance. Virchow proposes to call the substance, which just as it appears to view becomes coagulable by the action of oxygen, fibrinogen. We cannot see that any particular advantage is likely to attend the introduction of a new name, unaccompanied by any additions to our knowledge of the substance thus designated. However we may regard the question of the existence of specific forms of fibrine, it must be admitted that an increase in the bodies belonging to the fibrine group, very generally though not constantly attends inflammatory action, though not as a specific and peculiar process, for the same is found to be the case after abstraction of blood.

There appears, then, to be no specific ontological character for inflammation, either in the conditions of the circulation, the state of the blood, the phenomena of pain, heat, tumor, or redness; neither has it any *specific* difference from other lesions of nutrition, but is characterized by its extent, by the rapidity of its course, and especially by the character of the lesions it produces; its destructive tendency is that by which it is most remarkably distinguished from the simpler lesions of nutrition, and here especially the pathological differs from the physiological process.

It will be seen that inflammation constitutes a very complex process, the essence of which it is excessively difficult, if not impossible, to determine and give expression to. It is also much modified in the phenomena by which it manifests itself according to the dignity and peculiar properties of the parts in which it is developed, and thus will sometimes possess one, sometimes another dominant character. Much of this difference of its phenomena is likewise to be attributed to the nature of the irritant which calls the inflammatory process into action, and also much to the pathological predispositions of the part affected. These predispositions embrace those minor forms of lesion which are consistent with the condition of health, but yet induce a certain weakness and a greater capability for becoming the seat of more extensive and important changes, and moreover form an obstacle, often considerable, to resolution.

With these local predispositions may likewise be grouped those of general influence, and which have a specific dyscrasic character, such as the scrofulous, syphilitic, gouty, as well as the rheumatic and septic; and though we may not in the present day admit a special *materia peccans*, syphilitic, gouty, or scrofulous, as existing in the blood, we have yet reason to believe that in many cases foreign heterologous substances are developed, in the seat of inflammation, which give to the further course of the disease a special character. According as these changes result in the elimination of the foreign substances from the blood, or on the other hand induce an impure condition of this fluid, may the inflammation be regarded as benign, depurative, or critical in its character; or on the contrary, it may possess an unfavourable or malignant influence. Another group of inflammations is, however, to be recognised, in which, from the direct introduction of a specific irritant from without, the character of the inflammation is altered from the first, as we see in the whole range of toxic inflammations.

The sthenic inflammation is to be regarded as the pure form of the

process when it takes place under favourable conditions. The asthenic may be regarded as pure inflammations occurring in weakened parts or bodies. Another form may be recognised, the hypersthenic, whereby is understood that form of inflammatory lesion of nutrition in which, either from a particular predisposition or a peculiar kind of irritant, there is such a development of force, or such a remarkable metamorphosis of tissue, that the inflamed part is entirely destroyed, or that a constant development of new inflammatory products takes place; to this group Virchow refers many of the irritable inflammations of English authors.

The results of inflammation and its sequelæ are deferred for subsequent consideration, and the remaining pages of this section are devoted to the subject of treatment, wherein we find nothing to engage our attention at present.

We are conscious that we have in the preceding pages given but an imperfect view of the opinions of this eminent pathologist on these, perhaps, the two most important subjects in the domain of medical science. We trust, however, that we have succeeded in giving our readers a just idea of the broad and truly philosophic manner in which he treats them. We are far from saying that the last hand has been put to the picture in either case; but when we consider the immense difficulties of the subjects, their rank in pathological science, and above all, the way in which the determination of their least essential elements is dependent on the state of collateral departments of our science, it is not to be wondered at that our knowledge is not more precise and definite. That the concentrated lights now thrown on them have in many instances but shown us new difficulties and new paths to be explored, must be admitted on all hands; yet, if we have not gained a complete or perfect knowledge of what fever and inflammation are, we may safely say that we have found the right road, and that the goal lies before us, remote though it may be; and the student who takes the essays of Virchow for his guide, will at least be saved from error, for in his large philosophic views erroneous doctrine and dogmatic fallacies find no place.

Robert D. Lyons.

REVIEW IV.

Traité Clinique et Pratique des Maladies des Enfants. Par MM. RILLIET et BARTHEZ, anciens internes lauréats de l'Hôpital des Enfants Malades de Paris, &c. Ouvrage couronné par l'Académie des Sciences et par l'Académie de Médecine. Deuxième Edition, entièrement refondue et considérablement augmentée.—Paris, 1853. 8vo. Tome premier, pp. 844. Tome deuxième, pp. 748.

A Clinical and Practical Treatise on the Diseases of Children. By MM. RILLIET and BARTHEZ. Second Edition.—Paris, 1853. First and Second Volumes.

THAT student is young indeed in the department of pædiatrics who requires to be informed that the authors of the 'Clinical and Practical Treatise' have earned therein a position not surpassed by that of any pathologist of the present day. As systematic writers, sound teachers, and able practitioners, we believe this judgment to be fairly their due,

though the reputations of Trousseau, Barrier, Valleix, Mauthner, and others, be not at the same time forgotten. Ten years ago, MM. Rilliet and Barthez laid before the profession the result of their united labours in the form of three goodly volumes, numbering in the total about 2400 pages. Now they present us with a new and greatly improved edition of their work, of which two portions are before us, and the remaining third we hourly expect.* It is pleasant, amidst much professional selfishness and jealousy, thus to witness two eminent masters in their *spécialité* continuing their conjoint exertions as labours of love both to their common science and reciprocally to themselves.

“Separated by distance, but ever united by community of thought, we have continued to labour with perfect identity of view towards the perfectioning of our work.” (p. 3.)

The *one* remaining in the arena of his earlier toils, actively connected with the scientific movement animating the medicine of the capital of France; the *other* called to exercise the practice of his art in a distant city, and led to follow the important teachings left as an heritage to the faculty of Geneva by the Odiers and Jurines. We repeat, and with all indifference to any charge of hypersentimentalism, that, constantly reminded as we are of the “poverties of the spirit” (to use no stronger terms) entering into the ties of our professional and social connexion, we know not whether it is more a source of pleasure to us thus to witness the almost paternal union of these *anciens internes*, or of profit in recalling how it should be equally our own disposition: “σπουδαζοντες τηρεῖν την ενότητα του πνευματος εν τω συνδεσμῳ της ειρηνης.”

From the agreeable associations connected with MM. Rilliet and Barthez, the high position they occupy as authorities on the pathology, &c., of early life, and from the great value of much now superadded to, or interwoven with, the text of their former edition, it might be supposed that we intended to enter upon a critical or analytic exposition of the chief novelties presented by the volumes before us. But this is not the case, and chiefly for the following reasons: very many, if not nearly all, of the later investigations and more matured opinions of the authors in relation to individual diseases, have already appeared, in the form of papers (of great value), in the journals of the day.† These monographic essays have ere this produced an influence upon the branch of medicine to which they related—an influence, however, not the less valuable because excited previous to the systematic union and publication of their contents in the new edition of the great and laborious undertaking now upon our table. This influence, it is hoped, must have been also felt by the readers of this journal, in which due account has been taken, either by express statement of the progress of our authors, or by the more silent weight their particular investigations have exerted in modifying our own opinions and judgments. Further, we would remark that the very voluminous character of the ‘Treatise’ would forbid, under any circumstances, a *detailed* analysis of its emendations; and, moreover, it is not our practice in general to do more with second editions than to treat them very cursorily, pronouncing on them simply an *ex cathedrâ* judgment relative to the

* The concluding volume has since appeared.

† Archives Générales de Médecine. Gazette Médicale.

more important changes the original work has undergone. Forbidden, then, as we feel ourselves, to dwell upon the histories of individual diseases, yet we cannot pass over the important treatise of MM. Rilliet and Barthez with but a simple record of its new appearance. Out of compliment to the great reputation of the authors, and from the importance of some circumstances modifying the general pathologic doctrines of the present volumes, we purpose selecting a few topics worthy of some consideration, and as forming a fitting propædæutic to the study of the details of the 'Treatise' by those who, having entered upon the study of medicine within the last decade, have to form their acquaintanceship with MM. Rilliet and Barthez through the medium of the new edition.

The first circumstance we would refer to as having enlarged and modified the experience of our authors, is their having, since their first publication, enjoyed the advantages afforded by private practice. The results thus gained we find now added to those derived from their close and extended hospital instruction. A professional education, limited in its clinical departments chiefly to either public or private duties, cannot fail of presenting disease under a partial aspect, though if the treatment of it is afterwards to be prosecuted under the particular phase in which its pathology has been studied, no great detriment may ensue either to patient or practitioner. But without derogating from the knowledge and abilities of those whose study and practice have been necessarily thus circumscribed, we may safely maintain, that to such as are desirous of prosecuting the study of medicine on a broad and comprehensive scale, of viewing it in all its relations and dependencies, and, moreover, of becoming, like our authors, teachers of its pathology and practice, then the results of civil and hospital, public and private, practice must be duly joined together. This necessity bears with particular force upon the branch we are discussing, in which the influences of hospital hygiene are seen to be exerted in a peculiar and forcible manner, on the one hand giving rise to several affections not seen at all in private, and on the other importantly modifying those which are common to the patients of both classes. It is in the French capital, perhaps, that the greater contrast is observed between the two, for there the large and crowded hospitals for sick children and foundlings allow full scope for the operation of the deleterious influences springing from the consolidation of numbers within limited space, and from the artificial feeding and unnatural kind of life led by their little inmates. From these and analogous circumstances, erysipelas, peritonitis, gangrene of the lung, diphtheritic forms of croup, &c., inflammation of Peyer's glands, muguet, malignant coryza, and sclerema, are seen in such force and variety, that no extent of private practice amongst the paying classes of society could ever exhibit. As MM. Rilliet and Barthez remark:

"Under the influences of an abominable hygiene, we find that interminable series of grave disorders originate and spread which we far more rarely witness in private practice. Some of these affections are evidently the result of a prolonged stay in over-crowded wards. We had aforetime coined a word to express them which we may be permitted here to recal, notwithstanding its etymology be not strictly grammatical, viz., that *d'hôpitalité*." (p. 7.)

"The prognosis of the diseases of new-born children, in particular, deduced from observations made in the hospital for foundlings, is of perfect exactitude, so

far as it relates to the patients treated in this institution. But we cannot generalize upon such facts as these without forgetting those judicious words of Baglivi: *sub sole romano scripsi*. The more we advance from early infancy, the closer do the diseases of the hospital approach those of private life; but still it is necessary to draw a very strict demarcation between primitive and secondary disorders, to enable the facts gathered in the establishments appropriated to the treatment of the diseases of second childhood to be made available in practice." (p. 30.)

The public practice in a great town like London, amongst the children of the lower classes living near the banks of a muddy, filthy river, in localities, too, where the exigencies of space and of high rent oblige the parents with their children to inhabit a single small room, we believe to approximate most nearly to the practice of the hospitals of the Continent. The miasmatic atmosphere there generated is here witnessed, and nearly the same reunion of anti-hygienic circumstances creates the like *veritable climat pathologique* dwelt upon by our authors (p. 7). To these facts our own experience will bear witness; and we may refer also, in illustration of them, to the remarks of Dr. West, in his 'Lectures' on croup and diphtherite, diarrhoea and dysentery.* But if, on the one hand, the hospital and public practitioner may thus witness particular forms of disease on a large scale and in severe intensity, be able likewise to avail himself of continuous necroscopic investigation, and possess, to a great extent, a control over the carrying out of measures which the friends of private patients would often object to; the advantages of *civil* practice, on the other hand, are not the less evident.

"The family attendant witnesses the child's birth and development; he is acquainted with its hereditary tendencies, can follow its progress in life, and by its past course judge of its future. Called in most frequently at the outbreak of the malady, he himself observes the fugitive but important symptoms characterizing the first period of the disorder, a careful mother watches the punctual execution of his orders, and places him in possession of a complete knowledge of all that has occurred in the intervals of his visits."

"A great part of these advantages is denied to the hospital physician." (p. 4.)

We took occasion in a former article (vol. xi. p. 126) to express our belief that a refined analysis must lead to the doctrine that all diseases (with the exception of certain traumatic ones) have their genesis in some change or modification of *power*. In other words, that they are *primordially functional* in character, however rapidly visible structural alterations might show themselves during the course of any one of them. Side by side with our own opinions stood those of another writer,† in which a like doctrine was affirmed in its particular relations to zymotic diseases, it being shown that in the latter the influence of the ferment is not material, but *dynamic*; not consisting in the introduction or substitution of components, but in the *propagation of force*. We now lay emphasis on the fact that, whilst it is to be fully and fairly admitted that structural changes or anatomic lesions ensue very early, or even *appear* to coexist from the first, in many diseases, apparently giving rise also to the more prominent of the symptoms, and sufficiently explaining the cause of death;

* Lectures on the Diseases of Infancy and Childhood, second edition, pp. 233—253; 229—241. See also some observations by M. Rilliet in vol. xviii. of the *Journal für Kinderkrankheiten*, p. 378.

† British and Foreign Medico-Chirurgical Review, vol. xi. p. 163.

yet it cannot be denied that in many other instances such changes and lesions as may arise do not explain the symptoms, and are equally deficient in rendering a satisfactory account of the "why and because" of the fatal event. Whilst the records of morbid anatomy must ever form a most important part in explaining the nature of a vast series of diseased actions from which the open manifestations or symptoms spring, and in often sufficiently accounting for the arrestation of all vitality, yet it is clear that the archives of the *mortuarium* will for ever fall short in elucidating what is most essential, and as yet hidden in darkness. Nor do we see that the present (in some cases exaggerated, as it appears to us) application of the microscope towards a minuter and more recondite analysis of structural changes invisible to the unaided eye, will lead us a step nearer to the essential nature of many affections, although it may establish the presence of material changes, of whose existence before we were entirely ignorant. We say that we are really unable to perceive any magic power of solution brought into existence by this instrument over the *dioti* and *oti* of many diseases hitherto, and as yet, unreached by our analysis, and which still appear to stand within the limits of the knowable, though only to be solved, as we believe, by the use of a calculus of a totally different character. It has been well said by a great writer,* the force of whose saying a translation would not do justice to, that—"Was man wahrhaft *verstehen* will, von dessen *entstehen* muss man einen deutlichen Begriff haben." Here the object to which we should be striving appears to us not to reside in the lesional curiosities laid bare by the "achromatic objectives," but in the functional energies which give rise to them, or go before their production; and still more essentially (though here perhaps we may be even at fault) in the antecedent *nisus*, a power which initiates these very functions. So often as medicine will persist in believing that the utmost step to which we can generalize must be the landing ourselves on some anatomic alteration, so often as it seeks a treatment alone based upon or directed to it, so often we believe it will miss its mark. This, however, is no new doctrine; but we venture to repeat it, though its spirit has been taught long ere this. It was affirmed by Lobstein,† repeated by Professor Alison,‡ and again forced upon our notice by Dr. P. M. Latham.§ Holding the opinions we have just expressed, it was with much satisfaction we came upon the following paragraphs in the preface to MM. Rilliet and Barthez' work:

"The mode in which we treated our subject in the first edition of this treatise secured us the reproach of being too anatomical; now perhaps we shall be found fault with for having devoted too great a space to questions of doctrine and ætiology. But we would reply, that on leaving the benches of the lecture-room, we, along with a great number of our illustrious predecessors, have been brought to recognise the truth, that the autopsy is not the climax in medicine, and that the study of the dead body is not alone able to present us with the key to the phenomena of life."

"But we would wish not to be misunderstood, nor accused of professing for pathological anatomy a disdain which is farthest from our thoughts. . . . But as years have run on, the horizon has become extended and the mind enlarged. We have asked ourselves whether, under the influence of pre-occupations too exclu-

* Goethe.

† *Traité d'Anatomie Pathologique*, liv. 1. § 299.

‡ *Outlines of Pathology, &c.*, p. 39.

§ *Lectures on Diseases of the Heart*, vol. 1. p. 82.

sively anatomic, we have not often taken the effect for the cause, the result of the affection for the affection itself." (p. v.)

Along with this modification of their views, the authors have been brought *pari passu* to the fuller adoption than before of a humoral pathology; or, to use their own expression, "Without entirely quitting the path of solidism, we have made one step nearer humorism and vitalism." (p. v.) Humoral pathology has been so fully and lately discussed in the pages of this journal (vol. xi. p. 5 *et seq.*), particularly in its relations to *fever*, &c., that we need presume but very few remarks upon it here. With the gradual return to its doctrines made within the last twenty years by a very large proportion of the more eminent members of the schools of Europe, there has been a greater tendency to regard the diseased conditions of organs and parts as merely the local signs of more general disorder, or of some disturbance or contamination of a general system in the frame, the former being looked upon as merely the visible outbreak of the tumultuous spirit within. This tendency is acknowledged by MM. Rilliet and Barthez as holding an increasing growth in their own minds, and as having considerably influenced the opinions and judgments propounded in their present volumes. They observe that from the first they were always disposed to attach far more importance to the changes affecting the general health than to the topical disorder, but that now,

"Guided by the principle that Nature is niggard of causes but prodigal of results, we are convinced that the numerous local affections so sharply separated from each other, according to the present dominant ideas, are only the result of a small number of general morbid conditions." (p. vi.)

To discuss this matter in detail is by no means within the scope of our intentions; we shall simply refer to a few points connected with the pathology of children bearing upon it, or to such as a perusal of the pages before us have impressed upon our memory as worthy of comment, with the above statement of our authors in view. Before doing so we may just recal to mind, that as the pathologic relationships of the blood become more developed, the more are we inclined to seek in its mutations of vital forces and componency, the genetic sources of a very considerable number of local diseases. The abnormal conditions which this fluid may present may be derivable from hereditary transmission or endowment, constituting one great variety of diathetic maladies; whilst endemic and epidemic sources of zymotic and other poisons give rise, from their contamination of the circulating mass, to another great class of constitutional affections. The subjection of the living organism, especially during its earlier years, to the various well-known anti-hygienic influences, by which the offices of nutrition and assimilation, &c., are seriously perverted, bring about the cachectic and anæmic states, and other general and fertile sources of numerous chronic forms, especially of local malady. It is probable that another source, or constitutional expression, of a certain class of affections, is to be looked for in some morbid general condition, of a dynamic character, of the nervous system, and totally irrespective of its occasional production by a more primary abnormal condition of the blood itself. In this greater or less general nervous affection, more commonly met with in women and children, the sensations and emotions become intensely felt, their reflex action on the system or special organs extreme,

the voluntary efforts of mind and sustained exertions of the voluntary muscles difficult, strange, and irregular, and the "*animus nec sponte varius et mutabilis*."* We are fully aware that we stop short, in comparison with some, in thus hesitating to resolve almost all the *neuroses* into a primary morbid condition of the blood, either of the character of cachexia, or as dependent on some special *materies morbi* or toxic element in it. But a careful consideration of several facts, such as have been well insisted on by Mr. Carter† especially, makes us pause before we surrender up *everything* to the blood. It may be admitted with Andral‡ that the impressibility of the nervous centres often becomes the more evident as the quantity of blood diminishes and the muscular system becomes weakened, and that in this state the least painful form of hyperæmia may give rise to the most serious functional disturbances of the cerebro-spinal axis; with Alison,§ that a morbid increase of the activity of the changes in any portion of nervous matter may readily become attended by an increased determination of blood, or other change in the circulation of the part; and with Dr. Laycock and others, that certain diathetic conditions of the blood are very favourable to the outbreak of the neurotic affection. Nevertheless, with these admissions, as also, of course, with that of the fact of there being a great number of the *neuroses* which undoubtedly are but the manifestations of a primary morbid state of the circulating fluid, whether of a cachectic or toxic character, we do not yet feel warranted in believing that *all* are so.||

In childhood, epidemic influences and infection will be found to be the sources of the greater number of its acute diseases. It is necessary to bear in mind, however, the marked differences which exist between those affections which are merely epidemic, and such as are both epidemic and infectious.

"The former may be seen in quite the young infant, as well as at puberty, as evidenced by the tracheo-bronchial and gastro-intestinal catarrhs; whilst the truly-infectious diseases—those which we shall describe under the name of general acute specific maladies—are almost special to children who have exceeded their first or even second year. The eruptive or typhoid fevers, whooping-cough and mumps, serve as examples." (p. 8.)

The almost complete immunity of very young children from certain affections, chiefly of the zymotic class, has been explained by their comparative isolation or protection from the contiguity and sources of the particular morbid poison. The validity of this reasoning is to some extent admitted by MM. Rilliet and Barthez; but they maintain it does not explain the whole of the matter, and that it is necessary to suppose, that at the commencement of life the organism is not sufficiently prepared "for the hatching of the morbid germ that contagion deposits there."

"Is it not remarkable that the same observation applies to tuberculisation and some other hereditary diathetic maladies, which demand a certain development of organs before they can exhibit their open manifestations?" (p. 8.)

While the local anatomic changes and circumscribed lesions of organs

* Vide Alison, op. cit., p. 710.

† On the Pathology and Treatment of Hysteria, p. 79, &c.

‡ Precis, tom i. p. 18.

§ Op. cit., p. 679.

|| See also On the Prevalent Treatment of Disease, by F. C. Skey, F.R.S. London, 1853, p. 26.

are about the same (or analogous) in the child as in the adult, yet the cases are, perhaps, much more frequent in infancy than at any other age in which death happens; and yet no such change of structure, or even any evident alteration sufficient to account for the fatal event, can be found on cadaveric inspection.

“We see children succumb after having had profuse and continuous diarrhoea, or presented very serious nervous symptoms, while the intestines and the encephalo-rachidian system appear in a normal condition, or are too slightly diseased to explain the violence of the symptoms.” (p. 18.)

As parallel with these cases, we may refer to others still more frequently met with, in which the disease runs through its course and terminates happily; and yet we shall have been quite at a loss to say which organ had suffered material lesion, or which particular group or series of symptoms were to be regarded as necessarily marking the true nature of the malady. On the other hand, we occasionally meet with, in children, such extreme and peculiar local alterations of structure and disease of organs, giving rise, during life, to great functional and local disturbances, and, finally, to death, which yet, during their whole course, were not resolved into their exact nature; nor has the local lesion (*a* lesion being distinctly present) been deemed capable of reference to some general or diathetic malady; of which, nevertheless, such lesion has evidently been the result, as proved by examination after death. These changes of organs are sometimes found to be such as we should otherwise have considered as the consequence alone of continuous diathetic mischief in the far-advanced adult. But a few weeks back, we examined the body of a girl, eleven years of age, in whom the aortic valves were loaded with such an amount of semi-ossific, semi-calcareous deposit, that we never saw surpassed by the most gouty or rheumatic aged patient; and we have witnessed in a child, three years old, the entrance of the larynx constricted to a very narrow opening, surrounded by large warty excrescences or cauliform vegetation, and which appeared to have existed for some time, judging from the phreno-glottism and laryngeal symptoms which had been long the source of great inconvenience to the little patient. In the former case, though careful inquiry was made into the probable antecedent and diathetic disorders, the child did not appear to have betrayed any morbid symptoms; until the assumed effects of the cardiac mischief began to evince themselves, no rheumatic, urinary, or inflammatory affection had been present. The first symptom that attracted notice, was the occurrence of epileptiform convulsions, due, apparently, to the disturbance of the cerebral circulation, which was, in its turn, owing to the disorder of the heart itself, uncomplained of or unknown until such convulsions had ensued.

The tendency in the child to the diffusion of morbid acts at one and the same time, or to the supervention of local disorder upon disorder, gives rise to a numerous series of its secondary diseases; but by which is nevertheless evinced the general constitutional or diathetic base at the bottom of so many of them.

“The tuberculous diathesis is a type of such; everybody now knows, that in children general tuberculosis is far more commonly met with than the local forms. The same may be affirmed of the catarrhal diathesis. The local inflammations, the result

of the latter, rarely continue isolated; a child attacked by coryza, is soon affected with bronchitis, then with pneumonia, and enteritis may even succeed to this." (p. 25.)

Analogous relations to some general cause are, in all probability, indicated in those cases where convulsions alternate with essential paralysis; and spasm of the glottis coincides with contraction of the extremities. As opposed to such instances where important symptoms have been present, and yet no adequate lesion appears to have existed, may be adduced those in which certain usually well-marked symptoms are wanting, where some lesions nevertheless exist.

"Thus we witness eruptive fevers without prodromata, pneumonia without cough, extreme lesions of the intestines without pain or with but slight diarrhoea, and important acute or chronic cerebral alterations, whose existence had never been suspected. Facts like these appear to contradict an opinion we before expressed, as to the facility with which reaction ensues in infancy. . . . But so long as the normal physiologic tendencies remain undisturbed, the relations between the lesions of organs and the local symptoms are far more exact than between such lesions and the general or reactional symptoms." (p. 20.)

As on the subject of the want of accordancy between the reaction and the lesion we have already touched, when reviewing the work of M. Bouchut, we need not dwell further upon it here. The necessity of recognising the general constitutional or diathetic basis of the local manifestation, when the treatment has to be considered, is thus alluded to by the authors:

"Those practitioners who perceive in the greater number of the diseases of childhood only local morbid conditions, and who disregard the general state, expose themselves to many reverses. Even in primary diseases, a period arrives when the latter holds sway over the former; and when the local affection should be entirely disregarded, whilst the general condition claims all the attention of the medical attendant. We may, without fear of being contradicted by experience, apply the following proposition to the greater number of the diseases of children—if taking for your only guide the material lesion, you always attack the malady with the same remedy, you may lessen it in one instance and exasperate it in another." (p. 62.)

Simply to say that a local inflammation exists, or even to indicate the organ or tissue affected, are but very slight helps in the present day towards the establishment of a valid therapeutic. In truth, we may in one sense affirm that we are yet ignorant of the disease the patient is affected with. Very many, if not most,

"Local inflammatory lesions are but the consequences, the expression, the external form of a pre-existing general condition inappreciable to sense. . . . Each time the existence, seat, and species of inflammation is established, there yet remains its nature to be determined. . . . The local inflammation, then, is not a complication of the scrofulous, variolous, or inflammatory states, it is the direct expression of them. Further, these inflammations, different in nature, are different also in their characters and course, even when their seat is identical." (p. 82.)

When discussing the subject of pleurisy (vol. i. p. 580), and after alluding to the very different material lesions met with in various forms of the malady, and the variable symptoms accompanying them, the authors inquire if such differences be not sufficient to establish the existence of diseases of quite opposite characters, having, indeed, nothing in

common but their localization in the pleura, and their "deceptive name of inflammation?" (581.) This opinion may be considered as receiving support from the consideration, that if, on the one hand, pleuritis may accompany idiopathic inflammation of the pulmonary parenchyma, appear to participate in its nature, and be cured *pari passu* with it; on the other, it becomes developed under the influences of the rheumatic diathesis, in connexion also with scarlatina, and with albuminuria; and moreover, in certain instances, appears to owe its origin to some obscurely recognised but grave modification of the economy, whose chief result seems to be, effusion of pus within the cavity of the pleura. In the present state of science, however, MM. Rilliet and Barthez do not feel justified in describing pleuritis otherwise than as a simple inflammation of the serous membrane, seeing that neither the number nor the characters of the different varieties of the affection are yet satisfactorily determined, and

"Because, in fine (and this is of extreme importance), in the same child the inflammatory affection may unite itself to the rheumatic or any other diathesis, in order to give rise to a pleurisy which shall participate more or less of one or other of the causes which gave it birth. In the same way, we have observed the catarrhal condition become united with the inflammatory, giving rise to diseases intermediate between inflammatory pneumonia and bronchio-pulmonary catarrh. Moreover, if it is at present impossible to lay down the characters of simple pleurisies, how much less can we describe those of pleurisies which are complex?" (vol. i. p. 582.)

A very marked feature of the present teaching of our authors is the important sway which the catarrhal condition is regarded in playing, as the general or constitutional cause of very many important local maladies. It seems to be viewed almost as much in the light of a distinct entity as the rheumatic or the gouty, syphilitic, and tuberculous diathetic conditions.

"Three principal causes overrule its development. One is to be sought in the individual constitution; another in the epidemic state; and the third in anti-hygienic influences, particularly those connected with deficient alimentation." (p. 82.)

At pp. 82, 3, and 4, the reader will find the nature of catarrh discussed rather in detail, but it is still more fully entered upon when the catarrhal affections of the bronchio-pulmonary structures (473) and of the alimentary canal (718) come under review. MM. Rilliet and Barthez conclude, that a fundamental difference exists between the catarrhal diseases of the respiratory and digestive organs, and the simple inflammatory ones of the same structures. Their anatomic lesions, their seat, and also their symptoms, are held to be sufficiently indicative of this. But the catarrhal maladies of the different systems belong to the same "pathologic family" (p. 717), having at their bottom, and as their general cause, the same constitutional disorder; the symptomatic differences which exist, whether of a local or general character, depending rather upon the nature of the organ in which the malady has become localized, than upon a difference of cause.

"There exists a modification of the whole economy, which holds under its dependency the changes of the fluids and the lesions of the solids of the body. . . . As to what is the nature of this general morbid condition, we are as yet ignorant. It escapes us, and will, without doubt, always elude our investigations, as does everything bearing relation to the essence of life. Nevertheless, we distinguish it from all other affections by its effects, and accept the name of catarrh, which has

been bestowed upon it on account of its most frequent manifestation—viz., mucous hypersecretion; this hypersecretion being, as we have just said, the result of the elimination, by means of the follicular apparatus, of morbid matter contained in the blood.” (p. 718.)

It must be borne in mind that the authors do not deny the frequent alliance of the catarrhal and inflammatory elements, and this in all possible degrees (474); nor do they, whilst insisting on the essential influence of a general cause in the production of the former, deny the operation of “local topical ones” (p. 473). But “it is certain that this local cause is often nothing but the occasion of the localization of a catarrh otherwise imminent.” Under the head of “albuminous nephritis,” we marked for extract the following:

“We find again, then, in relation to Bright’s disease, a new application of the humoral theory we propounded when speaking of catarrh. Here it is no longer the mucous membranes, but the kidneys, by which the elimination of morbid matters ensues. The difference of organs does not interfere with the justness of the theory.” (vol. ii. p. 56.)

With regard to the pyrexial condition or fever accompanying, to a greater or less degree, the local manifestation of the catarrhal malady, and which is believed by the writers to include many of the forms of the mucous, remittent, gastric, pituitous, and other fevers of systematic authors, we condense the following opinions from what is scattered through the work:—In the endeavours made to eliminate the morbid matter from the blood, certain circumstances may solicit the localization of such endeavours at the bronchio-pulmonary organs (p. 408), at the gastro-intestinal surfaces (p. 655), at the kidneys (vol. ii. p. 56), or at the skin (p. 83). If such elimination ensue easily and quickly, the fever is seen to be apparently subservient to the local lesion or disturbance, and is apt to be considered as symptomatic of it (p. 728). But there are reasons for believing that the fever is not constantly and necessarily merely *reactional*, but that it must be often regarded (like the local mischief) as a direct symptom of the general modification of the economy, denominated catarrh (p. 475). If, however, such elimination is unsolicited by secondary local circumstances, if no special organ become attacked, then the pyrexial movement is witnessed, *per se*, as a general functional febrile disturbance, and betrays its relations to, and its direct significance of, the general constitutional diathetic disorder (p. 729).

It is but justice to an eminent writer in this country—viz., Dr. Copland—to draw the reader’s attention to the article “Catarrh,” in the first volume of the ‘Dictionary,’ where the germs of some of the later views on the subject before us are undoubtedly to be found.

We must now pass from catarrh to diphtherite. This “is a modification of the economy, producing a local inflammation, and impressing it with a stamp peculiar to itself.” (p. 81.)

“When the inflammation is propagated to the larynx, the prognosis is that of croup. When false membranes are developed on particular portions of the teguments, if the diseased surfaces become covered with thick deposits and furnish abundant suppuration—if the inflammation becomes serpiginous—if erysipelas increases its extent—the prognosis becomes infinitely grave, and death may follow in the midst of profound adynamia. . . . In other cases the mucous surfaces which are in contact with the atmosphere (anus vulva) become covered with

diphtheritic false membranes, which, by their progressive extension, determine the death of the patient." (p. 258.)

The diphtheritic may become combined with the catarrhal condition, and by the alliance give rise to certain modifications of the ordinary character of the former. M. Pidoux* has gone to the extent of basing a new affection on these changes, and which he calls *plastic catarrh*. Upon the subject of diphtherite, we may refer to some remarks in our eighth volume, page 69, as not inappropriate to be here reconsidered.

We need scarcely do more than recal to the recollection of the reader how largely the rheumatic and gouty diathetic conditions have of late years been appealed to, as regards the genesis of numerous forms of local disorder in the adult frame, to make it appear not unlikely that the former state at least might be called upon to resolve certain topical manifestations occurring in earlier life. And so it will be found: for independent of the typical articular disorder, certain forms of essential paralysis, of inflammation of serous membranes, chorea, and even eclampsia, have all been regarded as local effects of the rheumatismal disorder. Long ago, Stöll and Bouteille, and more recently, Bright, noticed the coincidental occurrence of rheumatism and chorea. Still later writers have repeated the observation, and sought to establish a causal connexion between the two. But it is more particularly in the writing of Seè† and Botrel‡ that the doctrine of most intimate relationship is taught. The point meets with close consideration from our authors, but we can only spare room for the following *résumé*:—

"The proofs of it must be sought in the affinities of coincidence, of causation, of symptoms or cadaveric changes, and of treatment." (p. 586.)

Inquiry so conducted leads to the opinion that such proofs of relationship are wanting, or at any rate

"We will say that choreo-rheumatic coincidence is the only proof that can be brought forward to establish a connexion, as regards *nature*, between the two maladies; that the frequency of this coincidence appears to have been exaggerated; and if we refuse not to admit that chorea may be one of the manifestations of rheumatism, we yet await other proofs before giving in our entire adhesion to this opinion." (vol. ii. p. 587.)

When noticing the 'Treatise' of M. Bouchut,§ we expressed our belief that *some* varieties of "essential paralysis" would be found to be rather of a myogenic than nervous character, and coincided partly as regarded the rheumatic doctrine so strongly insisted upon by this writer. With respect to muscular contractions, the "external tonic convulsion" and "spasmodic retractions" of some authors, we may observe that many pathologists, especially Corvisart, Delpech, Seè, and De la Berge, have dwelt very strongly on their connexion with the rheumatic diathesis. We confess that we have not been able to arrive at a very clear interpretation of MM. Rilliet and Barthez' views on this matter. At first, we considered them as regarding the malady "as of the same nature as

* Journal de Médecine, 1843, p. 154.

† De la Chorée, Mémoires de l'Acad. Nat. de Médecine, tom xv.

‡ De la Chorée considérée comme Affection Rhumatismale. Paris, 1850.

§ British and Foreign Medico-Chirurgical Review, vol. xii. p. 111.

the convulsions of young children," next as a neurosis of a rheumatic character, and lastly, as

"Not refusing to admit two species of contraction—the one purely rheumatic, the other connected with a functional perturbation of the nervous system. The first is frequent in the adult, the second in the child, occupying especially the extremities. As to the similitude established between the contractions and intermittent fever, it appears to us superfluous; the rheumatic element amply suffices to render account of the intermissions." (vol. ii. p. 497.)

We admit that we were but little prepared for the following views in connexion with sclerema:—

"In reflecting on the nature of this rare and curious malady, we are strongly inclined to regard it as rheumatismal. Our first case supports this opinion, for the complications which were successively developed had their anatomic seat in the mucous membranes, and we know that the rheumatic diathesis specially affects these membranes. The child forming the subject of the second example had been subjected to the influence of atmospheric conditions which engender this malady: she inhabited a damp lodging. . . .

"If the opinion we thus venture on be confirmed by other observers, sclerema will be another malady to be removed from the group of local affections, and associated with a diathesis." (vol. ii. p. 114.)

In the course of some preliminary observations on the "hæmorrhages," occur the following remarks worth quotation:

"This would undoubtedly be the place to inquire if there exists an hæmorrhagic affection or diathesis analogous to the serous diathesis, to the inflammatory and catarrhal conditions, all general maladies whose existence we have admitted; but the elements for resolving this question are wanting." (vol. ii. p. 238.)

The modifications of the whole economy which may give rise to the *neuroses* are, according to MM. Rilliet and Barthez, of three kinds: the nervous condition, properly so called, the rheumatic, and the cachectic. Badly defined as the first is as an alteration of the general health, yet its existence is widely admitted. Indeed, it is rather by exclusion than by a direct method of reasoning that its specification is arrived at. Very many of the *neuroses* have not, therefore, the same origin; and, as regards children, it is important to distinguish those nervous affections which are symptomatic from those which may be regarded as idiopathic in their character. The latter are the disorders particularly included under our present head by the authors, who define them as *apyretic maladies*, characterized by a disturbance of the functions of the nervous system, which latter does not present any appreciable material lesion (vol. ii. p. 448). M. Racle has strongly advocated the doctrine,* that the affections having their seat in the encephalon are frequently associated with some general or diathetic disorder. Thus the greater number of meningitic diseases, certain hæmorrhages, and some forms of hydrocephalus, are dependent upon tuberculization. Simple primary meningitis, however, is often associated with rheumatism, some cerebral hæmorrhages with purpura, and certain hydrocephalic affections with an albuminous condition of the urine. The opinions of MM. Rilliet and Barthez substantiate, in the main, these views; but they admit that, during the first months of life, we frequently observe very serious cerebral symptoms, sometimes of a convulsive, at

* *Récherches sur les Affections du Cerveau dans les Maladies Générales.*

others of an ataxic, character, rapidly terminating in death, and revealing no sensible lesion of the cerebro-rachidian axis or rest of the nervous system, no alteration of any other organ under the most careful investigation, nor the existence of any other malady, through whose medium such cerebral symptoms have become developed (p. 93).

The propriety of maintaining the distinctions between primary and secondary diseases is emphatically dwelt upon by the authors; and the important influences which acute secondary affections have in producing the *cachectic condition*, and *vice versa* the peculiar character impressed on such affections when occurring in cachexia, are severally treated of. This general cachectic condition is often the result of a vicious hygiene, and is thus what may be termed primitive in its nature, and the certain origin of a numerous series of morbid symptoms and manifestations of local disorders. The younger the child, the more readily is the condition of cachexia by such means induced, as well as being the rapid and essential consequence, not only of secondary diseases, but sometimes even of primary ones, too. In such cases,

“The organs have lost all power and resistance. Vital activity is nearly abolished, the child does not develop, it vegetates; and like the aged, it inclines towards the grave by excess of organic and vital debility. The energy of the power of growth is destroyed, or at least temporarily suspended; for the elongation of the body, which we were speaking of not far back, when alluding to febrile disorders, cannot here be verified. Further, if this condition persist, and if by a happy exception the child recovers, years pass, in which it very slowly develops, and retains the figure and bodily appearance of a much younger child, a condition contrasting remarkably with the aged aspect of its countenance.” (p. 28.)

Independent of certain local and traumatic *gangrenes*, the children, more particularly of the lowest classes, and of some densely-crowded hospitals on the Continent, are well-known to evince the death of parts and organs under conditions which leave no doubt as to the intimate alliance of such gangrene with some general constitutional or diathetic affection. As to the nature of the latter, however, we are ignorant. That it belongs to the category of the cachectic diathesis is undoubted, but there seems to be something superadded to mere debility or impoverishment of the blood, whether by anti-hygienic or pathologic causes. It is difficult to believe that there is not another element in play along with cachexia, besides the “scorbutic” of MM. Bouley and Caillault, the “defibrination” of M. Boudet, and the “augmentation of alkalies” of M. Becquerel. We have reason for thinking that gangrene of the lung, of the mouth, and of the pharynx, are associated with some absolute general and extreme vital deterioration not as yet definitely signalized. One remarkable fact may be here stated—viz., that out of one hundred cases of gangrene, forty were associated with measles (Tourdes Du Noma, &c.) According to our authors:

“The general gangrenes become developed under two influences:—

“1. That cachectic or scorbutic state, almost special to children, resulting on the one hand from the anti-hygienic conditions under which they live, and on the other from the prolongation or the succession of diseases they suffer.

“2. A special condition, determined by certain maladies, and particularly by measles.” (vol. ii. p. 343.)

The concluding portion of the second volume of the ‘Treatise’ is

occupied with the consideration of the "General Acute Specific Maladies," (p. 599). Of these, parotitis, pertussis, and typhoid fever, are now discussed; while variola, vaccinia, scarlatina, and measles, are retained for the third and concluding volume. The latter, however, it is presumed, will be chiefly taken up with the important subject of *tuberculosis*.

It is unnecessary here to dwell upon the proofs of the existence of some general modification of the whole economy, some special change of the dynamic or constituent states of the circulating fluid, which form the basis of the external manifestations of the febrile specific maladies; and as the subject of tuberculization, whether in its diathetic or local forms, is as yet unreached by our authors, we need not dilate upon it now. In conclusion, we would observe that whilst the 'Treatise' strikes us as having somewhat a less prolix character than before, there is yet much diffuseness, and even repetition of details in it. We doubt, too, whether, for particular purposes, the more compact though superficial volume of M. Bouchut will be displaced by it; though there can be no question that it will continue to occupy its position in the judgment of the most worthy as the chief authority, the first systematic work on the increasingly important subject of *pædiatrics*.

W. Hughes Willshire.

REVIEW V.

1. *A Manual of Pathological Anatomy.* By C. HANDFIELD JONES, M.B., and EDWARD H. SIEVEKING, M.D.—*London*, 1854.
2. *Handbuch der Speciellen Pathologischen Anatomie.* Von Dr. AUGUST FÖRSTER.—*Leipzig*, 1854.
Manual of Special Pathological Anatomy.
3. *Atlas der Mikroskopischen Pathologischen Anatomie.* Gezeichnet und Bearbeitet von Dr. AUGUST FÖRSTER.—*Leipzig*, 1854.
Atlas of Microscopical Pathological Anatomy. Observed and Drawn by Dr. AUGUSTUS FÖRSTER.

EVERY work treating of the science of medicine ought to be historical, original, suggestive. The amount of information given under each of these headings must vary according to the extent, character, and tendency of the subject. In a dictionary or cyclopædia of medicine, for example, we should find a full historical account, with more or less original matter; in a monograph, the original is expected to predominate; while the suggestive appears to find its most appropriate place in pamphlet medical literature.

The class for whom a work is intended forms another very important consideration in book-writing, a consideration which, in general literature, is never lost sight of by either author or publisher. Hence, our opinion cannot be truly valuable unless we are familiar with the educational or social position of the individuals among whom a work is to be circulated, and upon whom its influence is to be exercised. These are our reasons for believing that in literature, as in art, each work should be examined from the point of view indicated by its author.

Simple as are these considerations, they have, we believe, been too frequently overlooked, or have not suggested themselves to the majority of reviewers of scientific books—an observation that is particularly applicable to our Continental brethren. They should remember that the points from which we started are far apart. Many of the branches of science which they have been for many years investigating and teaching, are in this country examined by but a few, and are considered as accomplishments rather than requisites in a medical education.

Before we contrast the investigators, authors, and teachers, of medical science in Great Britain and Ireland, with their fellow-labourers on the Continent of Europe, it ought to be recollected that, in this country, the pioneers of medicine have to work individually, and without assistance. On the Continent, the moment a few master-minds have perceived that medicine is about to take a new course, which they consider likely to lead to discovery, they have but to touch the well-oiled medico-educational machine, when it instantly turns in that direction, and the new science finds a professor whose salary is sufficient to enable him to devote his time and attention to the advancement of his particular department.

How different is this position from that of the men who live in our great cities, where toil from morning until night obtains merely a sufficiency of the necessities, not to mention the civilization-created luxuries of life, without which their position in society cannot be maintained! The saddest part of this statement consists in its simple truth. Let those who doubt it go to each village and town and churchyard in these islands—there they will find our witnesses—men who have sunk under the struggle for life, and yet have left behind them the signs of good done in evil days—men who still hold out in the daily conflict between necessity upon the one hand, and the feelings of educated men upon the other—men who, while their services cannot be dispensed with, are harder worked and worse paid than the mechanic who stuffed the parliamentary seats of those right honourable gentlemen that “take advantage of a poor but truly honourable profession.” Before we calculate how much love for science exists throughout the practitioners of this country, it must be borne in mind that we are all obliged to be practitioners: by practice we must live, and from it are snatched the few hours we can devote to study and research. These considerations justify us in claiming the highest praise for those who are endeavouring to form a taste and a school for pathological and rational medicine in this country; and we receive the ‘Manual of Pathological Anatomy’ as a proof that this science is advancing with a force too great to be long resisted by the curricula of examining boards.

“The absence of any original work in the English language which embraces the entire subject,” is the reason given in the preface for the authors’ having undertaken such a work. This not only renders such a book absolutely necessary, but also appears to us to have considerably increased the labour and difficulty of its composition. In the table of contents, the name of each author is attached to the division of the subject he has described; and as they “are each individually responsible for the chapters they have treated,” we shall examine the work in the order they have adopted.

"Some general observations must, however, be premised," which consist in succinct explanations of the terms—"Morbid anatomy, pathology, exciting and predisposing causes, idiosyncrasies, diathesis, degeneration," &c. With respect to the long-debated question, whether diseases had their principal seat in the fluids or in the solids of the body, Dr. Jones observes, that—"At the present day we marvel how men could have adopted exclusively one view or the other, and refused to each their share in the production of morbid phenomena." This is the result of modern pathological investigations, to which belongs the merit of having swept away a heap of errors of the worst kind, inasmuch as they were the source of the most destructive bed-side practice.

At this, the very outset of our review, we must object to all vagueness of description in pathological science—therefore we cannot allow the following sentences to pass unnoticed: "That, on the one hand, there is a sound and healthy systemic life—there often comes in its stead, either generally or locally, an unsound and unhealthy life, which leads to perverted morbid action, or to actual decay." (p. 5.) This is given as "A great practical truth, which daily experience presses on our attention." Now, we refuse altogether to discuss this proposition, as the terms "healthy life," "unhealthy life," "perverted morbid action," are too indefinite to admit of argument. Criticism of this kind may, perhaps, seem little more than mere word-catching; but if our ideas are well defined, it is an easy matter to find words in which to express them. In a word, it is better to avoid all generalizations and explanations, than employ words that are neither precise nor self-explicative. Professor Engel, of Prague, has directed particular attention to the want of accuracy of expression in medical reasoning, and we hope to see these and similar inadvertencies avoided in succeeding editions of the 'Manual.'

The second chapter contains a highly practical and pretty complete expression of the researches of most of the authorities upon the normal and abnormal conditions of the blood. The contrast drawn between the descriptions of the textural characters of fibrine, as described by Rokitansky and Paget, is readily understood and instructive. The organic constituents of serum, extractive oily matter, and the saline ingredients in the blood, together with the parts that water, urea, bile, lactic and oxalic acids, play in disease, are fully described. Dr. Jones observes, that it seems very doubtful that such a condition as partial anæmia can exist, "if we recognise a deficiency of red corpuscles as an essential feature of this state." In general hyperæmia, on the other hand, Rokitansky is of opinion that there exists a qualitative as well as quantitative change in the blood, resulting from imperfect oxidation; while local hyperæmia is accompanied by but a quantitative change. This distinction does not appear to us to be well founded, as it is more than probable that the elimination of certain ingredients from the blood must be subject to a great variety of circumstances; and that the same amount of these changes cannot occur, in the liver, for example, when in the normal, in the anæmic, and in the hyperæmic states. If blood, then, passes through an organ without undergoing the usual changes, it contains abnormal ingredients, and is therefore qualitatively changed. We are consequently inclined to believe that hyperæmia, whether general or local, is accompanied by a qualitative as well as quantitative change in the blood.

Adopting Dr. Williams's arrangement, our author considers local hyperæmia under—*first*, hyperæmia with diminished motion of the blood in the part; *secondly*, active hyperæmia or determination of blood—meaning thereby a superabundance of blood resulting from increase in the velocity with which the blood is supplying a part; and *thirdly*, inflammation, or that variety of the hyperæmia in which the movement of the blood is partly increased, partly diminished. Regarding the practical and scientific value of this arrangement, Dr. Jones observes:

“It is not certain, especially as respects determination of blood and inflammation, that it is correct, or rather, that it may not prove a source of error by not being founded on that which is the essential circumstance in these two conditions. This, at present, is not possible, from the imperfection of our knowledge; but it may be well to bear in mind the above caution.” (p. 71.)

Hæmorrhage, flux, and dropsy, are given as the results of hyperæmia. Respecting the former we read, that

“In every case where blood is effused in any quantity, the walls of the vessels must have given way; and perhaps this is the case in every instance where a blood-globule escapes from its channel, though it is not, to our minds, absolutely certain that there is no such thing as the hæmorrhage by exhalation of the older writers.” (pp. 88, 89.)

It appears self-evident that there are but two ways in which a fluid, having physical characters such as blood, can escape from the vessels—*first*, by a solution of continuity in the tube; *secondly*, by filtration through the tissues of which the tube is composed. The causes of the first of these states are well known; the second, the filtering of blood-corpuscles through the walls of vessels, cannot, we believe, occur until two changes have been effected, both resulting from the abnormal state of the blood which causes hæmorrhagic diseases—*first*, the walls of the vessels having been badly nourished, are consequently lax; *secondly*, the fibrinous glaze which, in the normal condition, is being constantly laid down on the innermost surface of vessels of all sizes, out of the blood circulating in the calibre of the tube, is no longer well formed—then the walls of the bloodvessels are in a condition similar to that of unglazed paper, through which the corpuscles can readily pass.

We offer this explanation of spontaneous hæmorrhages, believing it to be of a physical, and perhaps even demonstrable character.

Relative to the injurious effects of hæmorrhages it may, we believe, be laid down as a rule, that they are to be estimated by the position, rather than by the amount of blood escaped.

Dr. Jones agrees with Mr. Paget and Wharton Jones in rejecting Dr. Williams's opinion, “that an essential part of inflammation is the production of numerous white globules in the inflamed vessels; and that the obstruction of these vessels is mainly due to the adhesive properties of these globules;” while he differs from Mr. Paget, and agrees with Rokitansky and Williams, “as to the increased production of white corpuscles in the inflamed vessels.” Were we obliged to give an opinion upon the subject, we would agree altogether with Mr. Paget, and refer the increased number of colourless corpuscles seen in the vessels of an inflamed part, to an accumulation, rather than a local production, of white corpuscles. We are well aware that the elements of these corpuscles

are constantly present in the blood; and the increased stimulus given to development by the local inflammation may exercise an influence upon these elements, and cause their rapid development. Yet, however, from actual observation, we are inclined to refer their increase to accumulation.

"We saw reason to believe that the tissues, in virtue of their nutrition power, exercised an influence on the movement of the blood; that in active hyperæmia their attractive force was increased; and we would now add, that it is through the failure of this nutrition power that we believe stagnation takes place. . . . The nutrition power of the tissues is chiefly concerned in the production of the flow of active hyperæmia and the stasis of inflammation." (p. 109.)

Under "Inflammatory Exudations," and "the changes that take place in the tissues affected by them," the author, following Rokitansky's description, differs from him concerning the albuminous exudations, which Rokitansky considers to be distinct from the fibrinous; a separation Dr. Jones considers to be scarcely warrantable.

The differences between pus and mucus are given thus:

"Liquor puris is albuminous, liquor mucis not so; pus will mix with water, and mucus will not; pus is dissolved, in some measure, by acetic acid, while mucus is coagulated; and mucus generally contains traces of epithelium, while pus does not. It is manifest, from what has been stated, that the difference between mucus and pus consists essentially in the different nature of the fluids, not in that of their corpuscles." (pp. 127, 128.)

Under "Leucocythemia," we find Dr. Bennett's is the only name mentioned. It must be borne in mind that Professor Virchow has not only thoroughly investigated this condition, but was also the first that recognised the true nature of the corpuscles, which, from Dr. Bennett's first case, were believed to be pus cells.

The complete adoption of the term "crasis" must not pass unnoticed. "Crases" have been described by Rokitansky as so many "alterations of the natural composition or *mixture* of the blood;" and we find the section on tubercle headed, "The Tuberculous Crasis—Tubercle." We would much prefer the word "state," as the term "crasis" seems to convey the expression of a substantive change in the blood, to which disease can be referred, and by which its phenomena may be explained. The doctrine of "crasis" was very useful in its time. Many morbid changes received a more or less complete explanation by reference to this theory; but we have no positive knowledge of the changes in, or conditions of, the blood, to which the term has been applied. In fine, the phrase, "crasis of the blood" belongs to that period in the history of pathological anatomy that has been rendered an epoch by the publication of Rokitansky's work; but it is in reality merely a speculative one, and has not been generally received by pathologists.

"Textural Changes," including hypertrophy, atrophy, induration, and softening, together with the fatty, fibrous, and calcareous degenerations, are next described.

The "degenerations" are defined as "changes of an essentially chronic character, latent in their origin, and obscure in their progress, until they have produced such deteriorations of structure as give rise to prominent secondary phenomena." (p. 154.)

Fibrous degeneration has been lately fully described by one of the

authors of the 'Manual' in this journal. It is stated to be somewhat allied to induration, but to be distinguished from it by there being scarcely any exudation in the fibrous degeneration; or exudation takes place slowly, and at once passes into fibre. In induration, on the other hand, "a notable quantity of blastema is effused, which compresses and atrophies the adjacent texture." (p. 154.)

Under "New Formations," the author follows,

"In the main, the arrangement which Rokitansky has adopted: sets forth their distinctive features, as far as possible; and recognises the frequent insufficiency of any structural or chemical peculiarities, to explain or even diagnose the essentially different natures of different specimens we may meet with."

Conformably with the course we have adopted in a former review,* this part of the subject shall be noticed in an article devoted to the consideration of "tumours;" nor will this delay detract from the completeness of our present analysis, as Dr. Jones's articles in this Journal, 'On New Formations,' have, we feel assured, convinced our readers that he is quite familiar with these growths; while, before closing the "general" part of the 'Manual,' it is only necessary to add, that those who desire to ascertain the leading characters of the vegetable and animal organization that exist in and upon the human body, will find the chapter on "Parasites" highly interesting and instructive.

The succeeding 572 pages, treating of "special pathological anatomy," do not admit of so detailed an analysis as that just given of the division, "General Pathological Anatomy." We shall, however, notice its most salient parts, and refer at intervals to Förster's 'Manual.'

"The Nervous System," described by Dr. Sieveking, opens with some general observations regarding the physiology of the series of organs about to be examined. The author proceeds to describe the pathological anatomy of, *first*, the brain and its membranes; *secondly*, the spinal cord and its membranes; *thirdly*, the cerebro-spinal nerves; *lastly*, the sympathetic system. The lesions of the dura-mater, of the arachnoid, and pia mater, with the changes which occur in its vessels; the lesions described under the names of tubercular meningitis and hydrocephalus, with that interesting class of tumours found in the choroï-plexus, to which Virchow has given the name of "corpora amylacea," and Dr. Jones of "concentric corpuscles," form the subjects with which this division is introduced to our notice.

"It is highly desirable that anatomists should settle its (the arachnoid's) normal relations, in order that the deviations occurring in morbid processes may receive a correct and proper estimation."

Prof. Kölliker has evidently represented the true state of the case when he observes,†

"Henle has shown that the arachnoid is not a serous sac, as is the pleura, or pericardium. Its inner surface, with its epithelium, is everywhere in close contact with the dura mater, so that a *cavum arachnoideæ* does not in fact exist."

Quoting from the same authority, Dr. Sieveking observes:

"Those who state that the arachnoid lines the ventricles of the brain, and the processes of the pia mater contained in them, suppose a thing that is impossible—viz., that the arachnoid passes through the pia mater, and invests the surface of the plexuses, which is actually an internal one."

* The Pathologico-Anatomical School of Vienna. No. xxviii., 1854.

† *Mikroskopische Anatomie*, von Dr. A. Kölliker, Band ii. Hälfte 1, § 1.

That the students of our schools are disposed to take a very great deal as granted, and also that the teachers of anatomy are well inclined to consider as proved all that relates to ordinary dissecting-room anatomy, must be evident to those who are acquainted with the present system of anatomical instruction. If our normal anatomy be wrong, our pathological descriptions must be very far from truth. If we had as little faith and as much industry as our German colleagues, it would have been unnecessary for us to quote a second time from our ever-working friend, Dr. Kölliker, who, with Virchow, Scherer, and Scanzoni, has made the little town of Würzburg equal, if not superior, as a scientific school, to any in Germany. Dr. Kölliker writes:*

“With respect to the *pia mater*, very many authors assume that the *plexus chorioidei ventriculi tertius et quartus* are connected with each other through the *aqueductus Sylvii*; further, that the *ventriculus quartus* is not completely closed, and that the *calamus scriptorius* communicates by a foramen with the subarachnoidal space. Both of these assertions are incorrect, as is that which affirms that a particular serous membrane lines the ventricles.”

In the succeeding chapter “On the Brain,” Dr. Sieveking discusses the question, whether the amount of blood contained in the cranium of the adult can vary? Physiologically and pathologically important as it is that this question should be satisfactorily and fully answered, we do not at present feel in a position to enter fully into the discussion it has excited. We shall therefore state the negative opinions in the words of Dr. Hughes Bennett:†

“But if we imagine that venesection will enable us to diminish the amount of blood in the cerebral vessels, the theory (held by the Edinburgh school) points out that this is impossible, and that the effects of bleeding are explained by the influence produced on the heart, the altered pressure on the brain, exercised by its diminished contractions, and the change of circulation within the cranium thereby occasioned.”

This sentence contains the admission, *first*, that the diminished action of the heart can *alter the pressure on the brain*—as the consequence of more or less blood being sent to that organ—the brain then can be thereby *compressed, or that pressure can be diminished*;—and *secondly*, that a change of circulation can be thereby occasioned.

Dr. Bennett further states, “there are many circumstances, however, which occasion local congestions on the brain, in which case another portion of its substance must contain less blood.” To this assertion the following experiment, made by Berlin under Donder’s direction, affords the best reply:

“A portion of the skull of a rabbit was removed, the corresponding piece of dura mater cut out, an accurately fitting portion of a watch-glass let into the opening in the calvarium, and the junction made air-tight with gum. When, by compressing the nose and mouth, respiration was intercepted, within ten seconds the increased redness of pia mater could be seen with the naked eye. This condition was made still more evident by the use of the microscope; and each time some minutes elapsed before the congestion again diminished. A dependent position of the head also increased the hyperæmia. Rapid abstraction of blood very distinctly diminished the diameter of the vessels.”‡

* Mikroskopische Anatomie, loc. cit.

† Lectures on Clinical Medicine, p. 143.

‡ Die Pathologie und Therapie der Gehirn-krankheiten, von Dr. Leubuscher, zweite Abth. S. 185. Berlin, 1854. The experiment appeared originally in *Nederlandsch Lancet*, p. 461, 1850.

Until we have experimentally investigated this matter, we refrain from giving an opinion respecting it, though we feel disposed to agree with Dr. Sieveking, "that a variation does take place, and sufficient to account for many of the phenomena of nutrition and disease;" we shall, however, avoid "confounding blood with fluid, and brain with cranium," an error into which, in Dr. Bennett's opinion, Dr. Burrows has fallen.

Congestion of the brain, hæmorrhage on its surface, into its substance and ventricles, with white and inflammatory softening, œdema, induration, hypertrophy, and atrophy, is the order adopted by Dr. Sieveking. Förster describes the anomalies in the size, position, &c., of the brain, the results of wounds, the new formations which occur in it, inflammation, softening, and induration; the different forms of abscesses; hyperæmia, anæmia, hæmorrhage, œdema, and, lastly, the parasites found in the brain, as the echinococcus and cysticercus cellulosa. Förster mentions three kinds of softening, red, white, and yellow; in the latter, "the clear yellow colour is caused by a diffusion and alteration of the colouring matter of the blood." Rokitansky was the first who drew particular attention to this variety of softening; he observes—

"Blood-corpuscles and the pigment already alluded to have as little to do with the production of the yellow colour as purulent matter, for the fluid contains far too small a quantity of blood-corpuscles, as well as of amorphous pigment, to account for it. My own impression is, that yellow softening is founded in a chemico-pathological process."*

Rokitansky alludes to Fremy's researches, which we cannot notice further than that he considered one of the constituents of the brain to be oleo-phosphoric acid, free, and in combination with soda; this organic acid is, at ordinary temperatures, when brought into contact with water or with decomposing animal matter, converted into phosphoric acid and olein; the olein is rapidly converted into oleic acid, which forms a soap with the ammonia; such, at least, is Fremy's view of the process of yellow softening. According to Lehmann,† the oleo-phosphoric acid of Fremy has been proved to be cerebrie acid, and the result of its decomposition, as Gobley discovered, is glycerin-phosphoric acid ($C_3H_7O_6$, $2HO + PO_5$), not free phosphoric acid. In confirmation of which, Lehmann states that it occurred to him once to observe a well-marked acid reaction, such as had been already noticed by Rokitansky, in a case of very extensive yellow softening of the brain, a reaction that was positively ascertained to be caused by glycerin-phosphoric acid.

"The absence of pus cells would not be an absolute proof that *suppuration* had not occurred; for they are not always present‡ in undeniable abscesses, where molecular granules and pyoid bodies may be the sole objects detected by the microscope."

This statement may lead to much confusion, and therefore requires comment. The absence of pus cells would, if we rightly understand the subject, not be absolute proof that *inflammation* had not occurred; the granular exudation-corpuscles being the form of cell under which the product of inflammation of the brain's substance shows itself. If *suppu-*

* Rokitansky's *Pathological Anatomy*, Syd. Ed., vol. iii. p. 423.

† *Lehrbuch der Physiologischen Chemie*, zweite Auflage, Band iii. § 99. 1853.

‡ Lebert: *Physiologie Pathologique*, vol. ii. p. 303, in a note.

ration occurs—by which must be meant the formation of pus, and not simply softening of the brain—pus cells must be, and are, found in all such cases. How pus cells could be absent from “undeniable abscesses” is beyond our comprehension; as we understand an abscess to be an accumulation of pus, the microscopic character of which fluid is the pus cell. There is a mistake with respect to the authority given for this statement, as at volume second, page 303, Lebert describes “cancer of the breast,” not abscess in the brain. We have moreover read this author’s description of inflammation of the brain, without being able to find any statement similar to that just given; on the contrary, pus cells are mentioned as having been found in a case* of “cerebral inflammation, with red softening, around an encysted abscess;” this being the only case that has been given of inflammation of the brain accompanied by the formation of purulent matter.

The morbid growths described as found in the brain are tubercle, cancer, cholesteatoma fibroids, and cysts. Dr. Sieveking, treating of cancer, observes—

“There are no symptoms peculiar to the disease, beyond the effects resulting from pressure; and even they do not appear to be in any way commensurate with the size of the deposit.” (p. 261.)

This remark may be extended to all the adventitious growths in the brain, as their position and character, rather than size, regulate the production of symptoms.

Förster treats of the pathological alterations which occur in the spinal cord and its membranes, and uses headings similar to those given above, under which he has described the lesions of the brain and its membranes. After a brief description of the most important of these conditions, Dr. Sieveking “regrets that our knowledge of the morbid changes occurring in it (the spinal cord) are in no way commensurate with the importance and dignity of the organ.” The mechanical difficulties attendant upon examination of the spinal cord are in a great measure removed by the use of the double-blade saw-edged rachitome of M. Charrière, of Paris. After having repeatedly used this instrument, we can testify to its utility. With it the entire spinal cord may be laid open by an assistant in about ten minutes. Finally, those who are interested in the lesions of this organ will consult with advantage the different articles published by Dr. Türk, of Vienna,* who, as physician to the department of the “Allgemeine Krankenhaus,” for diseases of the nervous system, has ample opportunities for such study.

Förster considers the lesions of the organs of circulation in the following order: Congenital anomalies of formation, alterations of size and position, wounds and ruptures, new formations, inflammation, metastatic abscesses, hyperæmia, anæmia, hæmorrhage, pneumatosis, parasites, and cadaveric changes. With reference to the production of hypertrophy of the heart,

* Lebert’s *Physiologie Pathologique*, tom. i. p. 120.

† Ueber kompression und ursprung des schnerven.

Ueber den Zustand der Sensibilität nach theilweiser Trennung des Rückenmarkes.

Ueber ein bisher unbekanntes Verhalten des Rückenmarkes bei Hemiplegie.

These and other articles by Dr. L. Türk are published in the *Zeitschrift der k. k. Gesellschaft der Aerzte in Wien*, 1850–51; and in the *Sitzungsberichte der Mathem.-Naturw. Classe der kais Akademie der Wissenschaften*, Band ix. § 229.

Dr. Sieveking, by whom the division "Pathological Anatomy of the Organs of Circulation" has been written, expresses his own opinion by adopting the terms used by Dr. Watson—thus: "There is something to hinder the free and sufficient play of the organ—an adhering pericardium, it may be, or mal-position of the heart." We turned to the section on "Atrophy of the Heart," expecting to find adherent pericardium mentioned among the causes of the latter affection; but this cause has been lost sight of by our English author, while Förster gives "the formation of thick fibrous masses after pericarditis" as one of the causes of this atrophy. Practitioners and students ought to understand fully the effects of so frequent a disease as pericarditis; and as it is commonly taught that hypertrophy of the heart is the result of adherent pericardium, we turn to Rokitansky's work, translated by the Sydenham Society; there we find—

"These conditions are occasioned by the injurious influence exerted by the pericarditic process on the heart, in consequence of which the muscular substance of that organ is paralyzed, and a flabby condition induced, which admits of the texture being easily torn, and which speedily yields to (passive) *dilatation* of the heart. (p. 137.)

"1st. The mechanical obstructions which give rise, according to circumstances, either to preponderance of *dilatation* or preponderance of *hypertrophy*, are—(a) Mechanical obstructions in the ostia of the heart; (b) Mechanical obstructions in the arterial trunks; (c) Similar (mechanical) obstructions in the capillaries. . . .

"2nd. Diseases of the texture of the heart (which produce hypertrophy of the heart) are, &c. . . .

"3rd. Excessive innervation of the heart," &c. (pp. 162, 165, 166.)

Passing to "Atrophy of the Heart," we read—"Atrophy of the heart is, moreover, also the result of pericarditis." (p. 170.) And if further confirmation is necessary, we find Dr. Stokes, in his late work "On the Diseases of the Heart and Aorta," writes thus:

"Without denying that a general adhesion may induce hypertrophy and dilatation, experience leads me to doubt that such an effect necessarily, or even commonly, follows the condition indicated. I have often found the heart in a perfectly natural condition, with the exception of an obliterated pericardium." (p. 11.)

We know of no opinion more valuable than that of Professor Smith, whose opportunities for such investigations are extensive, while his reputation as a pathological anatomist is not limited to these islands or to the Continent.

"It has been stated to me," writes Dr. Stokes, "by Professor Smith, that he has found general adhesion of the pericardium coinciding with atrophy or with hypertrophy of the heart, in a nearly equal frequency. And it is a remarkable fact, recorded by the same observer, that he has always found ossification of the pericardium, which we may hold as the extreme of the obliterating process, attended with atrophy of the heart." (p. 12.)

Drs. Barlow,* Chevers,† Walshe,‡ and W. T. Gairdner,§ may be referred to; their observations, as quoted by Dr. Stokes, all tend to establish the rule originally stated by Rokitansky, that *atrophy* must be considered as one of the most frequent consequences of adhesion of the pericardium; and that, in the great majority of cases in which, as a consequence of adhesion,

* Medical Gazette, 1847.

† Guy's Hospital Reports, vol. ix.

‡ Practical Treatise on Diseases of the Lungs and Heart, p. 452. 1851.

§ Monthly Journal of Medical Science, vol. xii. p. 103.

hypertrophy occurs, its production is a secondary consequence; the order of phenomena being—pericarditis; adhesion, with consequent paralysis, and often fatty degeneration of certain portions of the muscular structure of the heart; dilatations which, to use Rokitansky's words,* “are maintained by their own secondary conditions, which mechanically augment them, and gradually superinduce hypertrophy.”

Dr. Sieveking is of opinion—

“There is no doubt that, in a majority of cases, the ætheromic deposit is a secondary fatty degeneration of fibrine. Ætheroma appears, in the first instance, as a series of fibrinous layers, subjacent to the inner coat of the artery, formed by a process analogous to secretion from the blood.” (pp. 349, 350.)

A careful examination of ætheromatous deposits has led us to agree with Rokitansky, and to refer the production of ætheroma—in many cases, at least—to the deposit of oily matter from the blood circulating in the calibre of the tube, on the innermost surface of the vessel; at the same time, we do not deny that fibrinous deposit may occur in the elastic tissue of an artery, and that fatty degeneration may arise in such a deposit, for we have too much confidence in Dr. Sieveking's accuracy as a microscopical observer to doubt the absolute correctness of his observation; though we cannot admit that, “in a majority of cases,” ætheroma is produced in the manner he describes.

Passing to the chapters “On Aneurism,” it appears to us to be a curious circumstance that our friend, Professor Förster, quotes at great length from Dr. Crisp's work ‘On Diseases of the Bloodvessels;’ while Dr. Sieveking's statistics are taken from the researches of M. Bizot and Mr. Hodgson. We trust that the labours of a fellow-countryman will not, in future, be lost sight of by English pathologists; it should be remembered that Dr. Crisp's work contains an account of 551 cases of aneurism, selected from British medical journals, and of 364 preparations of aneurism in the Loudon museums; while Hodgson's cases number 63 only, and M. Bizot's but 189.

Adopting the general description of authors on aneurism, Dr. Sieveking observes, “it is unknown in childhood.” Dr. Crisp has recorded one case, in which the patient was aged nine years, while five are noted by him as having occurred between the ages of ten and twenty.

The occurrence of entozoa in the blood of man and animals has attracted considerable attention, and in addition to the references given in the works before us, we would direct attention to the researches† of M.M. Gruby and O. Delafond. Finally, a short chapter “On the Lymphatic System” closes the division, “Organs of Circulation.” From a careful perusal of “The Pathological Anatomy of the Organs of Respiration,” we are in a position to state that this division contains a well-arranged description of the results of the researches of a host of investigators. We need not, however, analyze these subjects, as they are familiar to our readers; but we desire to direct particular attention to the views of Dr. W. T. Gairdner‡ as to the production of emphysema.

* Sydenham edition, vol. iv. p. 165.

† Troisième Mémoire sur le ver filaire qui vit dans le sang du chien domestique: Comptes Rendus des Séances de l'Académie des Sciences, tom. xxxiv., Séance du 5 Janvier, 1852.

‡ Monthly Journal of Medical Science, vols. xi. and xii.; also, British and Foreign Medico-Chirurgical Review, April, 1853, p. 452.

Dr. Jones commences the description of the "Pathological Anatomy of the Alimentary Canal" with an account of the morbid changes that occur in the mouth, fauces, and parts contained in them. The affections to which the teeth are liable are described "from the excellent work of Mr. Tomes on the subject." The "abnormal conditions of the stomach" have been the subject of most interesting original observations, upon which, however, we refrain commenting, as we have seen a notice that Dr. Jones is preparing a work, entitled 'Pathological and Clinical Observations on Diseases of the Stomach.' The diagram of intus-susception will make this change of position readily understood. The results of prolapsus recti are in general much less serious than those of intus-susception; one case, however, in an adult male, came under our immediate observation, in which the prolapsus was as large as the patient's head: he died on the fourth day, before suppuration set in.

Congestion, inflammation, phlebitis, cirrhosis, and fatty degeneration of the liver, the various tumours that occur therein, together with the most important alterations in the biliary passages and in the bile, are well described. The pancreas is, we believe, the least frequently examined of all the viscera of the abdomen, yet its state in diabetes and other diseases is worthy of observation. Dr. Jones treats very briefly of atrophy, inflammation, fatty degeneration, cancer; and to these Förster adds tubercle of the pancreas. So little is known of the pathological changes to which the spleen is liable, that we take this opportunity of mentioning, that in a spleen weighing thirty-one drachms, taken from the body of a child, aged two years, who died of epistaxis, we observed long rhomboidal crystals of ammonio-phosphate of magnesia lying in great numbers in the spleen-pulp. A similar appearance has been noticed by Dr. Jones, and is mentioned at page 581. Further, the occurrence of leucocythemia, that we have observed in two cases of enlargement of the thyroid,* will, if further confirmed than they have already been by Dr. Neale,† be interesting, as affording a connexion between the condition of the blood in splenic and thyroid enlargement.

"The Morbid Anatomy of the Urinary Apparatus" is another division of which Dr. Jones is the author. He considers congenital anomalies, hyperæmia, renal hæmorrhage, anæmia, nephritis, and degenerative disease of the kidney, or desquamative and non-desquamative nephritis, or sub-acute inflammation of the kidney, under each of which names morbus Brightii has been described. The remarks on this latter affection are founded upon the observation of Frerichs, Johnson, and the author, and illustrated with nine woodcuts. This chapter concludes with the anomalous conditions of the urinary passages, bladder, urethra, and urine. The abnormal conditions of the testes and vasa deferentia are described and illustrated after Mr. Curling's article "Testis," in the 'Cyclopædia of Anatomy and Physiology;' these, with the lesions of the prostate and penis, complete the chapter on "The Male Generative Organs."

Dr. Sieveking treats of the "Pathological Anatomy of the Female Organs of Generation," which he divides into four chapters, on the ex-

* Excess of the White Corpuscles of the Blood, occurring in cases of Goitre: Quarterly Journal of Microscopical Science, April, 1853, p. 176.

† Medical Times and Gazette, vol. viii. p. 430, 1854.

ternal and internal organs of generation, the morbid conditions following and preceding parturition, the ovaries and mammae.

We wish to direct the attention of those interested in such subjects to Dr. Heschl's researches (translated by Dr. Robert M'Donnell), which have led him to believe that* "the proper substance of the uterus undergoes so complete a transformation to molecular fat, that not one single fibre of the uterus, existing previous to childbirth, remains behind."

The fifty concluding pages are occupied by the pathological anatomy of the joints by Dr. Jones, and of the bones by Dr. Sieveking. We have already noticed the other divisions of this work at such length, that these important subjects cannot be analyzed.

Förster opens each division of his 'Manual' with a short description of the normal structure of the parts, the abnormal conditions of which he is about to examine; and at the end of each subdivision reference is given to the principal authorities upon the subject. His book is a pretty complete compendium, without containing any original matter. Förster's 'Atlas of Microscopic Pathological Anatomy' consists of eighteen tables, having from twelve to four figures in each; together with eighty-seven pages of letterpress, containing a table of contents arranged, *first*, according to the class of pathological process; *secondly*, according to the organs in which the change occurs; *thirdly*, explanations of each table; and *lastly*, a short description of the figures is given at the foot of each table. These plates are not so valuable as those of Funke's 'Atlas of Physiological Chemistry,' because the latter are drawn as they were observed; while Förster's plates are beautiful pictures and very fine specimens of engraving, but they are too artistic, too nicely filled in, and far too well defined, to be facsimiles of what is seen by the aid of the microscope. It is, however, well suited for showing students, after they have seen an object as it is, what they might see could they make perfect sections; and it must be considered to be the best and most complete atlas of the histo-pathological anatomy of the tissues that has yet appeared.

Drs. Jones and Sieveking's 'Manual' is a more readable and far more original work than its German cousin. In the second edition we hope we shall find a chapter on the method in which autopsies should be conducted: an addition that would be, we presume, most acceptable to students and practitioners. The wood-cuts are, with very few exceptions, all well brought out, and are highly creditable to Mr. Bagg, who, to use the words of the preface, "has achieved as much as could be done by wood-engraving."

In conclusion, those who have followed our review thus far, or perused the original, have, we presume, formed a very favourable estimate of the English 'Manual' as a whole. By studying it, the student will not only learn the most important principles of the science of pathological anatomy, and be thereby materially assisted in appreciating the effects and symptoms of disease, but will be aided in taking a comprehensive view of the phenomena of diseased action. The form of this 'Manual,' as of Mr. Churchill's other publications of the same series, together with its con-

* Researches on the Conduct of the Human Uterus after Delivery. By Dr. R. Heschl. Translated from the German. Dublin, 1853.

densed yet comprehensive information, will doubtless render this work most acceptable to those engaged in the everyday practice of the profession.

Yet a word before we close : the great majority of medical men admit, that pathological anatomy is the base upon which rests rational diagnosis and practice. The teachers of that science have for years past spent both time and labour in their endeavours to create a taste for its study. How long, then, will our universities, colleges, and public boards, allow their students to judge whether it is, or is not, necessary to study the marks left by disease? When will pathological anatomy enter into the curriculum of medical education?

REVIEW VI.

Chloroform, its Properties and Safety in Childbirth. By EDWARD WILLIAM MURPHY, A.M., M.D., Professor of Midwifery, University College, Obstetric Physician to University College Hospital, &c. &c.—
London, 1855. pp. 72.

THE increase in the number of deaths during the performance of surgical operations under the influence of chloroform, has naturally excited a good deal of anxiety in the minds of professional persons and of the public, and it has given a colour at least, if not force, to the arguments and assertions of those who have throughout opposed the use of anæsthetics. The mistrust thus excited is doubtless strengthened by the fact, which cannot be denied, that we have arrived at no very satisfactory or positive conclusions as to the cause of death in such cases; whether it has been the result of the mode of administration, as Professor Syme seems to think, or from the presence of some incompatible disease, or some peculiarity of constitution which renders the system intolerant of chloroform—we cannot say, but certain it is that we are not yet in a position to say in what cases it ought to be proscribed. Nay more, it is not certain that, in several of the fatal cases, chloroform had any share in producing the unfortunate result; for many cases are on record of patients who died suddenly at the commencement of an operation, without any adequate cause, long before chloroform was discovered. In these cases it requires more than ordinary skill to guard against the old fallacy of *post hoc ergo propter hoc*. The truth is, that the whole subject requires careful revision; and the object of those who seek for the truth should be, by a careful collation and stringent analysis of facts, and especially of the unsuccessful cases, to ascertain the diseases with which chloroform is incompatible, the states of the system generally in which its administration is injurious or questionable, and to what extent its employment is without risk. If we knew these points, chloroform would rank precisely as any other remedial agent, but until then we shall be liable to a repetition of unexpected fatal accidents.

These considerations apply to its exhibition in obstetric as well as surgical operations, although as yet they have not been forced upon our attention by similar unfortunate circumstances. Yet we cannot deny, *à priori*, that there may be obstetric cases in which so powerful an agent

will be injurious, and therefore it becomes our duty to guard against them by thorough investigation.

Many, however, who do not object to the employment of anæsthetics in obstetric operations, decline to use them beyond these limits, and demand why they should be employed in ordinary labour? To which the advocates of chloroform would probably reply by another question, Why should we not employ them if we find them beneficial? The investigation of these two queries will go far to exhaust the arguments *pro* and *con*, and we propose to lay the matter pretty fully, but as briefly as we can, before our readers.

In this inquiry we shall derive most valuable assistance from the little volume just published by Dr. Murphy, which we have placed at the head of this article. Its tone is so calm and moderate, its facts so clear, its directions as to the mode and extent in which chloroform should be exhibited are so precise, that we think it will quiet the fears which recent occurrences have excited as to the danger to be anticipated from its employment in midwifery, as well as afford adequate guidance to those who are willing to use it provided they incur no danger by so doing.

“Why should you employ anæsthetics in ordinary labour not requiring an operation?” say the opponents, “since by so doing—1, You interfere with a *natural* process; 2, You contravene a special arrangement of Providence; 3, You induce a state of drunkenness, or something equivalent to it; 4, You may suspend uterine action, so as, 5, to involve the necessity of an operation; 6, You may occasion hæmorrhage after delivery, or convulsions, or insanity; or 7, You may cause death.”

These are all the objections we remember to have seen against the employment of chloroform in midwifery, and we have endeavoured to state them fully and fairly. Let us now calmly inquire into their validity, merely premising that they assume the exhibition of chloroform in full doses—in other words, the production of complete anæsthesia.

1. It is undoubtedly true, that in giving chloroform in labour we interfere with the natural process, so far as pain is concerned; but so we do when we give laudanum in a painful disease, for pain is as much the natural accompaniment of certain diseases as of labour, although of course labour is not a disease. But this argument comes with a very bad grace from those who would not scruple to induce premature labour, which is a much more positive and decided interference with a natural and healthy process.

2. That the excess of pain, agony, and struggle of labour is the providential punishment of sin, we do not question; but so are labour (“in the sweat of thy brow,” &c.) and sickness and death; and if it be wrong to take measures for the relief of the one *because* it was part of a Divine sentence, so, surely, it must be equally wrong to alleviate or postpone the other; yet we do not find that those who conscientiously oppose the former have any scruples about the latter. Exactly the same objection was made to vaccination, to steam-boats, and even (in the conscientious tenderness of Mause Headrigg) to winnowing machines. We trust, however, that the very able answers to such objections which have appeared, aided by a little common sense, have had the effect intended, as we have not heard much of late on this subject.

3. A very influential writer on obstetrics, Dr. Ramsbotham, observes, that "those who come forward as the chief advocates for anæsthetics under labour, have entirely denied, or maintained an undisturbed silence respecting its intoxicating properties;" and again:

"But if the case were put fairly and honourably before them (the candidates for chloroform)—if they were informed that they might probably be made *dead drunk*, but must certainly be reduced to that state which the law designates 'drunk and incapable,' how many, it may be asked, of our high-born dames, how many women possessing common feeling, how many indeed, removed above the very lowest orders of society, would be found to avail themselves of the immunity of suffering which anæsthetics hold out, at such a price, at such a sacrifice of moral obligation?"

Very few, we are sure, unless they believed, as we do, that there is not an atom of foundation for Dr. Ramsbotham's statement; and on this subject we confidently appeal to the numerous surgeons and accoucheurs who have witnessed its effects. In answer to Dr. Ramsbotham's assertion "that this state is not sleep, but drunkenness," Dr. Murphy replies:

"I must be equally emphatic, and repeat that the anæsthesia of chloroform has not the least resemblance to drunkenness; they have not a symptom in common. Alcohol mixes intimately with the blood; chloroform does not. The one is highly stimulating; the other not at all so. Alcohol has no anæsthetic power, unless taken in very large quantities, when the imbibor, after a stage of most boisterous excitement, arrives at the condition termed '*dead drunk*,'—the anæsthesia of alcohol. Chloroform manifests this power without the least excitement; it produces anæsthesia, and takes away pain, *without disturbing the intellect in the least degree*. The patient is perfectly herself, and, in the words of Dr. Ramsbotham, can '*expatiate on the relief afforded her*.' If chloroform cause sopor, the sleep is perfectly tranquil, the only evidence of excitement (if such should happen) being the occasional mutterings or ramblings in the transition from wakefulness to sleep. The effects of alcoholic potations do not pass away for hours afterwards, because time is required to separate it from the blood. Chloroform is not so dissolved, and therefore rapidly evaporates, leaving the patient, in much less than an hour, as much herself as before she inhaled the vapour." (p. 64.)

In fact, whether we note the effect of small or of full doses, whether we observe the initiatory stage, the full effect, or the recovery, the difference from drunkenness is most remarkable, the scientific resemblance, nothing; the subjects are no more legally "drunk and incapable" than they are "drunk and disorderly." That the subjects of a full dose of chloroform or alcohol are insensible, is true; but we might as well assume that travelling by railroad is the same as travelling by coach, because the destination attained by each is the same.

4. In a certain small proportion of cases, chloroform does interfere with uterine action, rendering the pains less powerful, and perhaps less frequent. In Dr. Denham's report* of 56 cases in which chloroform was used in the Rotunda Lying-in Hospital, of which 15 were natural labours, he mentions that in 4 it suspended more or less the natural action of the uterus, which *returned in full force when the chloroform was withdrawn*. This, we think, is a larger proportion than is generally observed, but it does not matter, for common sense tells us that in such cases we

* Dublin Journal, vol. viii. p. 116, new series.

must cease giving chloroform, just as we give up the use of calomel if it cause diarrhoea; it could be no argument against the general use of chloroform, unless this effect were equally general and permanent.

Dr. Murphy thus states his experience:

"The action of the uterus under chloroform is not generally interrupted. The uterine contractions are governed by the reflex or excito-motor and the ganglionic nervous systems. The latter is never influenced, and will always maintain them. The former requires the full dose to disturb its power; a moderate dose (that which I have recommended) has no effect at all on the reflex nerves; nay, it may rather irritate than control their power. The uterine contractions are sometimes increased under the influence of chloroform, and labour makes a rapid progress. It is true this may arise from the removal of the great disturber of uterine action, mental anxiety and dread of pain; but it may also be explained by excitation of the excito-motor nerves, rousing up the uterus to increased action. If, however, the action of the uterus be suspended because these nerves are getting under the influence of chloroform, the effect is only temporary, because the ganglionic system restores the contractions; while the very fact that the reflex system is thus affected, renders the passages much more yielding and dilatable than before." (p. 37.)

5. But this suspension of uterine action, it is said, may not only be complete, but permanent; and Dr. Robert Lee has related five cases in which it became necessary to deliver with the forceps, in consequence of the cessation of pains, and in which chloroform had been given, although we are not told to what extent, in each case. Now, it will be recollected that in the cases we have quoted from Dr. Denham, the pains immediately returned on the omission of the chloroform; this is the experience, too, of Drs. Simpson, Beatty, Murphy, and M'Clintock, and certainly our own. We have never seen any exception to this rule, but we *have* seen cases of powerless labour without chloroform, in which the pains gradually ceased, and the forceps became necessary, and so, we doubt not, has Dr. Lee. To establish his point, he should give us presumptive proof that the cases would have terminated naturally but for chloroform, or some evidence more special of the direct evil effects of the vapour. Upon the data before us we must confess that we must decline agreeing with his conclusions, although we cannot but say that if the practitioners who had charge of the cases thought that the chloroform diminished the pains, they acted very injudiciously in not instantly suspending it.

Dr. Lee relates two other cases in which craniotomy was necessary, as he considers, in consequence of the suspension of the pains by chloroform. In case 1, where insensibility was not produced, it does not appear that the pains *were* suspended; in the other they were, but whether by chloroform or not is not clear. Dr. Beatty's remarks upon these cases are so apposite, that we shall take the liberty of quoting them:

"We all know that uterine contractions are often suspended naturally for hours in the middle of a labour, when no chloroform has been used, but that alone would never lead one to resort to craniotomy; there must be something else in the case besides mere want of action in the uterus to warrant such a proceeding; and so it must have been in these cases cited by Dr. Lee. Very likely the uterine action was interfered with by a precipitate employment of the drug; but it is also likely that the cases were such as would have required craniotomy equally if chloroform had never been used; for I cannot for a moment imagine that a physician of Dr. Lee's experience would resort to such an operation on the simple grounds of an

arrest of uterine action. In the absence of details of these cases, we may safely put them down as *post hoc ergo propter hoc* cases, and class them among the absurd exaggerations (to use no more severe term) with which partisans so often attempt to mislead their readers.”*

Let us also just remark, that the arrest of pains took place during the first stage, a time when their spontaneous diminution in pregnancy is not uncommon and is of no consequence, if other circumstances are favourable; and during which stage, in the absence of complications, an operation is rarely, if ever, justifiable.

6. Dr. Lee relates seven cases in which “insanity and great disturbance of the functions of the brain followed its use;” or, in obstetric language, some degree of puerperal mania occurred; and if this never happened except when chloroform had been used, they would exactly prove the point, but as all the best writers on obstetrics, before and since chloroform, have described such an affection of childbed, and as all of us are unfortunately familiar with it in practice, it is difficult to see how such a case as the following proves anything but that puerperal mania may occur after chloroform, as well as without it. “Case 10. In the month of June last, chloroform was cautiously administered to a lady in her first confinement, twelve hours after its commencement. The pains were soon wholly suspended, and it became necessary to deliver with the forceps. Eight days after, violent cerebral disturbance ensued, and she has continued, till a very recent period, insane.” Now, considering that in this case there are two very serious deviations from the all but universal action of chloroform, we think that it rests with Dr. Lee to prove that the necessity of the forceps and the occurrence of insanity were due to chloroform; might we not as well attribute the insanity to Dr. Lee’s use of the forceps, and on exactly the same process of reasoning?

“I have administered,” says Dr. Murphy, “chloroform in upwards of a hundred instances, a number sufficiently large to form an accurate opinion on such a point. I can truly say that in not a single case was there the slightest approach to mental aberration. Could this happen if insanity were one of its effects?” And again, “I have not as yet met with an instance of mania *after* the administration of chloroform, although it is quite possible I may do so, when the usual causes of this disease come into operation to produce it. I once attended a lady (the wife of a medical friend) to whom had I given chloroform I should have been very much blamed. She suffered rather severely. I proposed the inhalation of this vapour; my friend however objected, as he feared, from some peculiarity in her constitution, that it would not agree with her. She went through her labour, and after a severe trial was safely delivered. On the tenth day symptoms of mental aberration manifested themselves, which continued some time before they disappeared.” (p. 67.)

The experience of most of those who have used anæsthetics, Drs. Simpson, Channing, Beatty, Denham, &c., quite supports Dr. Murphy’s statement, and would almost, if not quite, justify the conclusion that puerperal mania is rather less common when chloroform is used. Although a single fact is not of much value, we may mention that in a case under our own care, in which mania and insomnia occurred without chloroform, the only rest or relief the patient obtained was after the full exhibition of the vapour.

* Dublin Journal, vol. xvii. p. 362.

Again, we are told by the same author that "dangerous or fatal peritonitis or phlebitis ensued after the exhibition of chloroform in cases 7, 8, 11, and 13." But here there is the same absence of any attempt to show the connexion between the chloroform and its supposed consequence. Take, for example, the following case:

"CASE 11.—About the same time, chloroform was exhibited to another patient, a lady in her first labour. The contractile powers of the uterus were soon wholly suspended, and the delivery was completed by the forceps. Fatal peritonitis, with peculiar nervous symptoms, soon supervened."

Now, unless we are prepared to take Dr. Lee's word for it, we do not see on what ground we are to attribute the peritonitis to the chloroform; if on the doctrine of sequences, then the operation with the forceps comes after the chloroform, and has consequently a clearer right to be considered its cause.

It is proverbially difficult to prove a negative, and, therefore, without denying the *possibility* of grave accidents from chloroform given in labour, we shall be quite contented, if our readers are satisfied, from the foregoing considerations, that these facts upon which Dr. Lee founds his opinions are "not proven" to be liable to the interpretation put upon them.

But with regard to another effect attributed by him to chloroform, we have more positive evidence in answer. Case 14 is given as one of epilepsy or convulsions caused by chloroform; and we remember the time when it was thought that in certain cases chloroform might produce such an effect, but the result of further experience has been the discovery that, in many cases, the inhalation of chloroform or ether is one of the most powerful means we possess for controlling and curing convulsions. For example, Dr. Channing, of Boston, U.S., gave ether in 10 cases; in 6, the patients recovered;* Mr. Turner,† Mr. Norris,‡ and Dr. Keith§ each gave it in a case with perfect success, and an equally favourable example occurred at Gosport.|| Mr. Bolton tried it successfully after bleeding and opium had failed.¶ Dr. Shekleton, the late master of the Dublin Lying-in Hospital, administered it in 9 cases; in 5 the convulsions were completely arrested, and in 4 they were lessened in intensity and frequency. We have ourselves given it in convulsions during gestation, with immediate benefit. Many more similar cases might be adduced, but these are surely enough to oppose to the single case related by Dr. Lee, and more than sufficient to exonerate the chloroform from having caused the convulsion, unless we admit (on the principle of *similia similibus curantur*), that it may be at once the cause and cure of the disease.

Lastly, a fear was expressed by some practitioners that, as chloroform sometimes interferes with uterine action, it might give rise to hæmorrhage by arresting contraction after delivery; but such has not been found to be the case. We are not aware that any instance of the kind is on record, and we have the testimony of Dr. Simpson and others that it has been safely administered in cases of placenta prævia and other forms of alarming hæmorrhage.

7. We do not know of any case of labour, supposed to have terminated

* On Etherization in Midwifery, pp. 307, 330.

† Edinburgh Monthly Journal, May, 1849.

‡ Medical Times, March 23, 1850.

† Lancet, Jan. 12, 1850.

§ Ibid, Aug. 1850.

¶ Lancet, Jan. 29, 1852.

fatally from chloroform, related on good authority, except the one given by Dr. Ramsbotham, the substance of which we shall quote, together with Dr. Murphy's comments, with which we entirely agree.

"It was the lady's fourth child; she gave birth to the first, after a very tedious and painful labour, in consequence of a considerable narrowing in the conjugate diameter of the pelvic cavity, which rendered the use of the long forceps necessary. This was before the application of anæsthetic agents to the practice of obstetrics was adopted in England. Her medical attendant (not Dr. R.) at her urgent request placed her under the influence of chloroform during her second labour, and her recovery was speedy and perfect."

It was exhibited in her third confinement, but inefficiently, and in her fourth labour,

"Parturient pains came on about noon. Chloroform was given at 7½ P.M., when the os uteri had acquired the diameter of an orange, and the pains had become frequent and strong. Its effects were most delightful and tranquillizing. After refreshing sleep, she rose and bore some moderately strong pains without a return to chloroform. It was then resumed, and repeated in frequent drachm and half-drachm doses, but only when she entreated to have some of the delightful chloroform, from about ten to a quarter to twelve, soon after which the child was born. She instantly expressed much gratitude, and expatiated on the relief afforded, though even then she felt 'wrung' by the severity of her labour. The uterus contracted well, and the patient appeared comfortable. At the end of an hour and a half, however, distressing dyspnœa came on; this was soon followed by convulsions and almost immediate death.*

"Such," observes Dr. Murphy, "is the account of this deplorable death from chloroform. Let it be compared with those which have occurred in surgical practice, and a judgment formed of the accuracy of this conclusion. The lady referred to never lost her consciousness. Chloroform never caused her the slightest inconvenience during its inhalation, her respiration was perfectly undisturbed. After delivery she '*instantly expatiated on the relief afforded her, was quite collected, quite comfortable.*' But in an hour and a half afterwards, when all the chloroform inhaled (and she had evidently taken very little) had quite time to evaporate and disappear, she is seized with a distressing dyspnœa! an effect the very contrary to what chloroform is known to produce; *convulsions* and death follow. We would ask, how could the absent vapour cause a distressing dyspnœa, when its presence did not disturb the respiration in the least degree? If chloroform cause distressing dyspnœa, why not do so during its inhalation? or if such be its effect *after* inhalation, how is it that we cannot find a single instance of death so produced in the thousands who have been under its influence for surgical operations? A moment's reflection is sufficient to show the injustice of attributing this fatality to chloroform, simply because no other explanation can be given." (p. 61.)

To us this case resembles those cases of idiopathic asphyxia which occur sometimes after delivery, and of which we had intended to quote one or two as illustrations, had not this notice already occupied so much space.

In addition to the foregoing serious objections to the use of chloroform, a minor class has sometimes been adduced, consisting of certain symptoms which occasionally follow its employment. For example, headache, vomiting, and temporary incoherence, do occasionally but rarely occur: the former speedily pass away, and the latter is removed by a little less or more of the vapour; we have never observed that the language was either violent or coarse; in most cases it was unintelligible, from the patient uttering only parts of words. As to erotic ideas, said to be excited by

chloroform, it is difficult to speak positively; at any rate, it cannot be a common effect, as few with whom we have spoken have witnessed it.

We have thus endeavoured to lay before our readers all the objections brought against the use of chloroform in midwifery, and we have endeavoured to do so fairly, neither extenuating nor undervaluing the arguments of its opponents, nor denying the possibility of accidents. But we must here remark a very important point: all, or almost all, the objections are directed against the use of chloroform in full doses—i.e., given so as to produce insensibility or sleep; and although we think the facts adduced fail to establish the case even against its full exhibition, we have ourselves an objection to carrying it to this extent in ordinary labour, for this very obvious reason—*that it is quite unnecessary*. Quite sufficient relief may be afforded without interfering with the mental condition of the patient—such relief as will enable her to bear her labour firmly and patiently, and secure her from all the evil consequences of prolonged pain. This, we say, may be done without placing the patient within the *possibility* of danger; and to this, therefore, none of the objections hitherto made apply. Dr. Murphy's testimony is conclusive upon this point:

"The obvious conclusion," he remarks, "from these experiments is, that the risk from chloroform may be altogether avoided, and yet the patient receive a considerable amount of relief. In the practice of midwifery, the pains of labour can be assuaged and rendered tolerable without inducing sleep; and in the practice of surgery, it appears to me that many minor operations may be performed with equal safety." (p. 52.)

On the other side of the question, the advocates of chloroform contend for its exhibition, because—1. You relieve suffering by it; 2. You diminish the nervous irritation produced by long-continued pain; 3. You lessen, or avoid altogether, the shock to the nervous system, and so far the object may be obtained without producing sleep or unconsciousness; 4. In case of an operation, the patient is not merely saved from suffering, but placed in a more favourable condition for its skilful performance, inasmuch as the operator has not to guard against her struggles and resistance where chloroform is fully administered.

1. That pain is an evil can hardly be denied, but that a certain amount of it can be borne with perfect impunity is equally true: we daily witness labours terminated within a few hours without any perceptible effects upon the constitution, or any retardation of the recovery. In such cases, we are far from thinking chloroform necessary, but if the patient request to be spared a portion of this amount, and we believe that we can do so with perfect safety, why should we refuse? Our own practice has been, never to propose chloroform unless we saw that the amount of suffering was likely to do mischief, or in cases where an operation has been necessary; but we have not felt at liberty to refuse it even in less severe cases, when demanded, and we believed that its administration was safe.

2. But in many cases the suffering is very severe, either from the exquisite sensibility of the patient, or from the greater resistance; and, no doubt, the recovery may be retarded by it. From this injury we possess the means of saving our patient, in the moderate exhibition of chloroform.

"The advantages of chloroform in obstetric practice consist not only in its power of controlling the intensity of suffering to which the parturient woman is

too often unnecessarily exposed, but in promoting a more favourable recovery. Since the publication of Mr. Travers' work 'On Constitutional Irritation,' the profession acknowledge the danger that sometimes results from intense pain. Patients have died from the shock of an operation. It is denied, however, that the pains of labour, be they ever so intense, produce any shock to the constitution: I believe this to be utterly untrue. I know nothing that predisposes more to troublesome consequences than long-continued and severe pain, especially with delicate women. Their recovery is always slow; and while in this depressed state, if a morbid poison be within reach, they are sure to absorb it." (p. 44.)

3. But the shock to the nervous system may not only retard the patient's recovery, but may implicate her in immediate peril. We have more than once witnessed cases of labour terminated by the natural powers, and yet which left the patient in such a state from the "nervous shock," that it was doubtful for some time whether she would ever rally; and in one such case, death took place apparently from no other cause. Again, in cases not so severe, but in which an operation may be necessary, this addition to the shock may leave the patient with a very doubtful chance of recovery. From these dangers we may, in most cases, preserve our patient by a timely and moderate exhibition of chloroform, without incurring any risk of injury.

4. Lastly. The situation of a patient upon whom an obstetric operation has to be performed is very different from that of a surgical patient, and renders the benefit of chloroform still more striking. The latter has a choice offered him, and if he submit to the operation he does so willingly, and with an effort at least to bear it bravely. Besides, there are always assistants at hand who will exercise both control and even a little coercion, if necessary, after the operation has been commenced. On the contrary, an obstetric operation is a comparatively sudden necessity, without a choice whether the patient will submit or not. She *must* consent to its performance to save her own life, or her child's, or both, and this urgency, together with the effect of her previous sufferings, seems to diminish her power of self-control. Then, if during the operation she resist or struggle, there is no power of restraining her, nor would it be easy, or perhaps safe, to do so, even if there were plenty of assistants at hand. Yet, notwithstanding the outcries and struggles of the patient, an important operation, internal, involving organs essential to life, and easily injured, in the neighbourhood of some of the great viscera of the body, has to be performed coolly, discriminatingly, and deliberately, under most trying circumstances of fatigue of body and mind, and possibly with many other disadvantages. How difficult this is, even in favourable circumstances, all obstetricians know full well, and those in contact with the poor know how almost impossible it occasionally is. But under the effects of a full dose of chloroform all this distress is spared; the patient, unconscious of suffering, lies sleeping calmly, perhaps smiling, while the operator, relieved in mind and body by the absence of cries and struggles, is at liberty to concentrate his entire efforts to the successful completion of the formidable operation he has undertaken; and when all is finished, and the effect of the chloroform dissipates, he finds his patient awake, calm, easy, and grateful for the relief he has afforded and the suffering she has been spared. That this is no exaggerated statement will be testified by every one who has fairly tried the experiment.

Until we have better evidence than has hitherto been adduced, that the full use of chloroform in such cases is attended with danger, we confess that we should feel ourselves blameworthy if we refused to employ it. Judiciously given, it is a most valuable addition to our means for rendering an operation successful, both by relieving the patient's suffering and rendering her a better subject for the skill of the operator. To those who think that they derive any benefit or guidance from the outcry of the patient, we hardly know what to say without giving offence. The true guide for an operator ought to be his perfect knowledge of the organs upon or among which he is going to operate; and if this be insufficient, without the addition of warning from the cry of his patient, it would perhaps be better that he should not operate at all.

We have occupied so much space in these investigations, that we shall only lay before our readers Dr. Murphy's rules for the administration of chloroform, premising that we ourselves prefer a white pocket handkerchief, folded in a conical shape, to any inhaler we have hitherto tried, and that we think the dose rather to be measured by the sensibility of the patient, and the effects produced, than by minims or drachms.

"Rule 1. Let the chloroform be pure. If rubbed on the hands, the smell should be fragrant, not pungent, like sulphuric ether. If inspired from the inhaler, there is a sense of warmth in the mouth, a fruity flavour, no pungency; if the strength of the vapour be sufficient, it will excite a slight cough: but if impure, the cough is irritating. Let the sponge of the inhaler be placed in warm water, and then wrung perfectly dry. About thirty minims may be poured upon it, which is sufficient in the first instance.

"2. When labour has commenced, do not interfere so long as the patient bears her pains well; if she be not teased with short, very severe, and inefficient pains, chloroform need not be given. If, on the contrary, the severity of the first stage be such, the anguish of the patient so great, that pain is evidently a cause of protraction, chloroform may be given with great benefit.

"3. Always commence with a small dose, about thirty minims; if it agree with the patient no inconvenience is caused, but she will generally complain that it is doing no good; the quantity may then be increased, until on inhalation the exhibitor finds that she cannot take a full inspiration without cough.

"4. In the second stage of labour, chloroform may be given when the head is approaching the perineum, or before then if the pains become intolerable. This may be known not merely by their greater intensity while the uterus is in action, but also by the restlessness of the patient in the intervals. She is watchful, dispirited, still crying, but in a more subdued tone, from pain and a feeling of soreness.

"5. When the head arrives at the perineum, chloroform may be given in a fuller dose, if it have not already accumulated. The perineum yields more easily under its influence, and the severity of the pains is controlled without any loss of force. This rule applies especially to cases in which powerful forcing pains are acting against the perineum at the hazard of its laceration.

"6. When operations are necessary, if they are not severe—as, for instance, some forceps operations—chloroform may be given in the same manner as in natural labour, but always after the instrument is applied.

"If severe, it may be given as in surgical operations, but not to the same extent. Hence an assistant is necessary who is conversant with the properties of this anæsthetic. It is obvious that the same person cannot operate and give simultaneously the full soporific dose of this agent.

"7. The inhaler should be applied to the mouth just before the pain commences, two or three full inspirations taken, and the moment the action of the uterus

ceases it should be withdrawn. The inhaler should never be applied in the interval between the pains, and if used in the middle of a pain the cries of the patient blow away the vapour, and no relief is given.

"8. When inhalation has been continued in this interrupted manner for some time, if any alteration be observed in the countenance or manner of the patient—if the face is flushed, or bloated, or tinged with a slight lividity—if she ramble or become hysterical, let the inhaler be withdrawn, and the face of the patient fanned. Wait until the pains return to their original severity before renewing the inhalation, when it is probable that these symptoms will not return.

"9. In some instances, the patient is very intolerant of her pains, and if given chloroform to relieve them, she becomes hysterical, crying, perhaps, louder than before it was inhaled. In these cases, it is better to induce sopor, which may easily be done, without stertor. For this purpose, a sponge and folded handkerchief applied to the nostrils is preferable to the inhaler. Whenever sopor is brought on, the closest attention should be given to the countenance—observe the irritability of the eyelids; to the respiration—notice its frequency, and especially stertor; to the pulse—mark its strength. The handkerchief should always be held at a distance at first, and be gradually brought nearer, but the sponge should never be applied quite close to the nostrils.

"10. There should be the freest circulation of air in the apartment; and if, after delivery, there should be any feeling of faintness or nausea, ammonia in effervescence will relieve it." (p. 69.)

We strongly recommend Dr. Murphy's little work to the profession, which we think owes him obligation for having brought clearly before it the advantages of chloroform in small doses for the relief of the suffering of labour.

Fleetwood Churchill.

REVIEW VII.

1. *The Decline of Life in Health and Disease; being an Attempt to Estimate the Causes of Longevity.* By B. VAN OVEN, M.D.—London, 1853. pp. 300.
2. *De la Longévité Humaine, et de la Quantité de Vie sur le Globe.* Par P. FLOURENS.
On Human Longevity and the Amount of Life on the Globe. By M. P. FLOURENS.—Paris, 1855. Small 8vo, pp. 240.

M. FLOURENS treats of certain topics which have reference to the theory of life. In every age, the author observes, this has been an object of study, while it is only now that we have begun to study it in its largest aspects. The questions of the quantity of life, and of the first appearance of life upon the globe; of the permanency of species, and of lost or extinct species—are, M. Flourens remarks, entirely new. Along with these, the author discusses other more ancient questions, but which he thinks he has invested with some degree of freshness; these are—human longevity, the formation of life, and old age. M. Flourens' complete 'Theory of Life' has yet to appear.

In the first part of his work—that on 'Human Longevity'—M. Flourens brings forward the example of Louis Cornaro, who, by dint of extreme moderation and sobriety, prolonged his life to the length of nearly a century. The age of one hundred years is regarded by M. Flourens, on

the following physiological grounds, to be the period of human longevity. Taking the period of increase or growth of the human frame—that, namely, up to the age of twenty—by which time the epiphyses of the long bones are found to be firmly united to their shafts, and multiplying this age by five, the period of one hundred years is obtained. It must not, however, be forgotten that M. Quetelet and others have fixed upon twenty-five years as the period of increase; and that very different multipliers and multiplicands have also been given by other physiologists. M. Flourens supports and illustrates his theory by an examination of the various stages of existence or periods of growth, maturity, and decline in man and in the lower animals.

“The duration of life,” observes Buffon, “is not regulated by climate, food, manners, or external condition, but by fixed laws of the animal economy.” This opinion is based upon correct observation. We observe that each species has its distinctive characters; however nearly they may resemble in some points, there are features belonging to each sufficiently different to establish their distinctness.

Each species, remarks M. Flourens, has its determinate duration of gestation and of growth, as well as its determinate size, figure, &c.; wherefore, then, he asks, should there not be a fixed law of duration of life in each species? Buffon, accordingly, thought he saw the operation of this law in the multiplication by seven of the period of growth, which he fixed at the age of fourteen. This gives about the same duration of life as is deduced from the more philosophical and exact observations of M. Flourens.

There are countless instances of longevity on record in which persons have been said to have lived beyond a hundred years. We admit that the truth of many of these is placed beyond doubt or dispute; therefore, without incurring the charge of scepticism, we may observe that these instances are met with before accurate registration was enforced. Future generations will, however, have in their possession the means of verifying the fact in every alleged instance of longevity.

The comparative observations of M. Flourens lend confirmation to his law as regards the age of man. Thus the camel has attained its growth by the age of eight years; the horse, at five; the ox, at four; the dog, at two; the cat, by eighteen months; the rabbit, at twelve months; the guinea-pig, at seven months; &c., &c. Multiplying these figures by five, we have the usual length of life in these animals, e. g., of the camel, forty years; of the horse, twenty-five years; of the ox, from fifteen to twenty years; of the lion, about twenty years; of the dog, from ten to twelve years; of the cat, from nine to ten years; of the rabbit, about eight years; of the guinea-pig, from six to seven years; &c., &c. The theory is thus found in accordance with facts. In most other animals the data for the calculation are yet wanting.

M. Flourens points out a relation between the principal phenomena of animal life; thus, the duration of life is measured by the period of growth; the period of growth has also given a relation to that of gestation; the larger the animal, the longer the period of gestation; the latter, in the rabbit occupies thirty days, in women nine months, in the elephant nearly two years.

Of the duration of life in the elephant, observes the author, we are ignorant. If, however, the author's theory be applicable in this instance,

the age of an elephant may be as great as is commonly received; thus, 9 months (the period of human gestation) $\times 25 = 20$ years; (the period of growth) $\times 5 = 100$ years (the extreme duration of life in man). Applying the same calculation in the case of the elephant, e. g., "nearly two years," (say 22 months) $\times 25 =$ nearly 46 years (the period of growth of the elephant) $\times 5 = 230$ years, we have the duration of life in the elephant. Observations are wanting for the verification of this calculation, but the result does not differ widely from recorded instances of elephantine longevity.

M. Flourens cites from various authors numerous instances of extraordinary longevity in men and lower animals. Dr. Van Oven has given tables comprising the names, condition, country, date of death, and ages of fifteen hundred and nineteen persons who have attained to ages between a hundred and a hundred and ten years; of three hundred and thirty-one who died between the last-named age and one hundred and twenty years; of ninety-nine who reached the age of a hundred and thirty; of thirty-seven who lived to be a hundred and forty years old; of eleven who reached a hundred and fifty; and of seventeen who exceeded the last-named age. Besides these, Dr. Van Oven has collected notices of fifty living persons at ages varying from one hundred to one hundred and eighty years! Nor are these all that Dr. Van Oven has brought together. Four hundred and ninety "additional instances" of longevity, two thousand one hundred and seventy-nine instances of ages above one hundred in Russia, seven hundred and fifty ditto in Sweden, and the quotation from the Registrar General's Reports of seven hundred and eight deaths above one hundred years of age in England and Wales, in the five years 1838 to 1844. In all, seven thousand who have lived from a hundred to a hundred and eighty-five years.

Dr. Van Oven does not vouch for the genuineness and authenticity of all these records; but they may be received, in the words of the author, "as more than enough to justify a fair presumption that human life might endure much longer than it usually does, and to encourage the exertions of those who desire to promote healthful longevity."

Dr. Van Oven's work has a chapter upon the Causes of Longevity, which he finds in the original constitution of the individual, his habits, avocation, mode of life, immunity from disease, equanimity of temper, and freedom from great and frequent excitement. The attaining that (to some persons) very desirable point, an old age, is in consequence resolved, in the work of Dr. Van Oven, into the regulation and care of all these conditions. The same author, moreover, devotes a dozen chapters to "Diseases of Age," and to "The Decline of Life in Disease;" having introduced the study of longevity by several chapters "On the Process of the Organization of Man, from Birth to Maturity."

M. Flourens having, as above described, expounded and illustrated his theory of the duration of life, proceeds to discuss certain subjects which he regards as collateral to the question of human longevity and his theory of life. The topics to which we refer, are the permanency and extinction of species, spontaneous generation, fossil remains, the deluge, &c. M. Flourens has treated all these topics in an interesting and lucid manner, but he has not therein presented us with any facts that are not to be met with in standard works on zoology, geology, and natural history in general.

REVIEW VIII.

1. *Die Hämodynamik nach Versuchen.* A. W. VOLKMANN. (Cap. xii. *Von den Kräften welche das Blut Bewegen*).—*Liepzig*, 1850.
On the Forces which Move the Blood. By A. W. VOLKMANN.
2. *Le Système Capillaire Sanguin.* Thèse par L. A. SEGOND.—*Paris*, 1853.
On the Sanguineous Capillary System. By L. A. SEGOND.
3. *Principles of Comparative Physiology.* By W. B. CARPENTER, M.D., F.R.S., F.G.S. (Chap. v.)—*London*, 1854.

It is now more than a century since some few physiologists were led to suspect, from the observation of certain phenomena which they could not otherwise explain, that during the passage of the blood through the capillary vessels it was subjected to a force, independent of the action of the heart, which assisted the circulation. Various conjectures were then formed of the nature of this power, and its source was generally supposed to be either in the blood itself, or in the walls of the capillaries.

But the doctrine of a force resident in the capillaries, supplemental to the heart's action, in circulating the blood, was, by the great majority of physiologists, rejected. They saw no evidence of its existence, and, moreover, they calculated that the powers already known, and perhaps understood, were amply sufficient for the purpose, and hence they negatived the idea of a supplementary one. This question has since been repeatedly discussed, and many excellent names may be appealed to in support of either view. The rapid advances which physiology has made of late years, have greatly tended to dispel the mystery in which this subject was formerly enveloped; but the question still continues to receive opposing answers, and in the most recent authors and most eminent physiologists of the present day, advocates of either doctrine may be found. Moreover, those who admit that the blood in its passage through the capillaries is subjected to a force which influences its circulation, are by no means agreed as to the extent of its operation. Here the greatest diversity of opinion prevails, from that of Bichat,* who imagined that the influence of the heart did not extend beyond the capillaries, to that of Dr. Allen Thompson and others, who believe that the power which operates on the blood in the capillaries is only capable of modifying its distribution—not contributing to its progressive motion.

Many arguments which were formerly advanced in relation to this subject have been subsequently either altogether rejected or considerably modified, while new ones have been supplied as physiology in its progress has swept away former errors, or revealed new truths. The question is an important one, and it will be interesting to inquire into the nature and extent of the evidence concerning it which we at present possess.

The facts which relate to this inquiry are both numerous and various.

* See also Guthrie, who says, "The heart exerts a comparatively trifling degree of influence over the circulation." *On the Diseases and Injuries of Arteries, &c.*, by G. J. Guthrie, p. 226. 1830.

These we shall attempt to arrange into more general expressions, as arguments either for or against the doctrine.

In support of the doctrine:—

1. The circulation of the nutritive fluid is naturally maintained, in various instances, independently of a heart, by the operation of other causes.

2. After the heart has been removed, or after the great vessels at their origin have been tied, the circulation is continued for some time through the capillaries.

3. The empty state of the arteries, so generally seen after death, is not altogether due to the contraction of their walls, but is partly dependent on the continuance of the capillary circulation.

4. In its healthy and natural circulation the blood is subjected to continual changes during its passage through the capillaries, both in its velocity and direction, which cannot be attributed to any influence derived from the heart or vessels.

5. The amount of blood circulating in certain parts or organs is liable to considerable variation, independently of any general change.

6. The quantity of blood flowing through the various arteries is regulated by causes operating in the parts which they supply.

7. The circulation of blood through any part or organ is in a great measure dependent on the normal changes which it undergoes in the capillaries.

Against the doctrine:—

1. It is alleged that some of the preceding statements will not stand the test of a rigid investigation, and that the phenomena actually observed may be otherwise explained.

2. A force not exceeding the natural action of the heart has been proved by experiment to be sufficient to propel the blood through the entire system.

3. When the circulation of a limb is confined to the branches of a single artery and corresponding vein, the flow of blood through the latter is directly controlled by pressure on the former.

These several statements will be now separately considered.

1. In plants, the sap circulates independently of any contractile or propelling organ; and it has been proved by well-known experiments (see those of Hales and others) that in the higher plants, at least, there are two distinct forces in operation—one at the roots (*vis à tergo*), and the other at the leaves (*vis à fronte*)—and that these forces operate so long as the vital changes continue, and are dependent on them.

But the best illustration is to be found in the circulation of “the elaborated sap or latex, which from its containing the elements for the nutrition, and for the various secretions of the plant, may be likened to the arterial blood of animals.” (Todd and Bowman.) This nutritive fluid circulates through a distinct system of anastomosing vessels, the laticiferous, closely resembling the capillaries of animals, in the leaves and bark. The fluid moves in various directions, even in contiguous vessels, with a velocity by no means uniform. The circulation is most vigorous in rapidly-growing parts. Its rate seems to depend upon the activity of the nutritive process. It is often observed to circulate in a

direction opposed to the force of gravitation, as towards the stem in a dependent branch.

In some of the lower animals a distinct circulation may be observed, and yet no special organs for the propulsion of the blood can be detected; and the heart in its simplest condition, as the dorsal vessel of insects, is manifestly incompetent, if unassisted, to maintain the circulation.

In the higher animals, the lymph and chyle are propelled onwards in their vessels with considerable force—a force which can be in no way dependent on the action of the heart.

In the early embryo of the higher animals a distinct movement of blood has been observed in the vascular area before any pulsating organ could be detected. Moreover, it has been affirmed by very excellent observers, that the first motion of the blood is *towards*, not *from*, the centre.* These points, however, have not yet been determined beyond all doubt.

In reply, it may be argued that the above facts, although true in themselves, yet have no direct bearing upon the question at issue. Because, in plants and in the lower animals, and even in the earlier conditions of the higher, causes independent of the action of a heart may act to circulate the blood, it does not follow that in the adult of the higher animals such causes shall still continue in operation. In short, it is denied that this kind of evidence is admissible.

In answer to these objections it may be replied, that although the above facts are not brought forward to prove the doctrine, but merely to show that other causes than the action of a heart may circulate the blood, yet it does not follow that because these causes, which operate independently of a heart in plants and the lower animals, are subordinate to the action of a powerful heart in the higher, that therefore they no longer exist. Such an idea is opposed to one of the most interesting facts displayed by the study of the organic kingdoms. It is well known that in tracing any particular structure or function, from the lowest to the highest living beings, through the regular series of organized forms, or even through the successive stages of development of the higher, we are led to perceive a great principle of progression from a more general to a more special character. For example, the function of respiration. In the simplest forms of animal structure (Polypes, Medusæ, &c.) no special organ exists for this purpose: in them, as in plants, the aëration of the fluid is accomplished at the surface of the body, simply by exposure to the surrounding medium, through the membrane which forms their integument. As we ascend the series we find that the external membrane is extended inwards into cavities among the viscera, or outwards from the surface of the body; and that in these organs, more particularly, the necessary changes are effected in the blood. Thus the rudiments of lungs or branchiæ appear. These organs, which at first present themselves as simple prolongations of the external membrane, gradually assume more

* Von Baer: Ueber Entwicklungsgeschichte der Thiere, &c. With regard to the development of bloodvessels in effused lymph, Mr. Paget says: "Although direct observations are wanting, I think we may conclude that all the vessels of inflammatory lymph are formed by outgrowth from adjacent vessels, as in the process of repair, and that through these vessels, not by its own development, it derives its supply of blood."—Lectures on Surgical Pathology 1853, vol. i. pp. 367—8.

complex forms, until, when we arrive at the higher animals, we find special complicated organs adapted for the aëration of the blood. But as it is thus seen that "a special function arises out of one more general, and this by a gradual change,"* the existence of another no less interesting fact is also to be observed. It appears that even in the highest animals this limitation of function to a special organ seldom proceeds so far as to completely exclude or obliterate, however it may supersede or obscure, its more general character. "The function of respiration is not confined to the lungs, even in animals which possess them in their most developed form. The blood which circulates through the capillaries of the skin is aërated by communication with the atmosphere, wherever there is no impediment offered by the density of the tegumentary covering." And this is true even of the human body. This law has been thus stated, "In cases where the different functions are highly specialized, the general structure retains, more or less, the primitive community of function which originally characterized it."

Now the circulation affords another example, equally evident, of the same general laws. Although in the higher animals we have this function principally discharged by a special central organ, it by no means follows that the more diffused force, which is alone employed in the lower animals, is here annulled, although subordinate to, and obscured by, a far more effective power. On the contrary, we should reasonably expect to find distinct traces of it still existing. At all events, it will be admitted that these facts clearly prove that the movement of the blood can be accomplished by other causes than the action of a heart.

2. The circulation of the blood in the capillaries, after the influence of the heart's action has been removed, has been repeatedly witnessed. Dr. Wilson Philip relates the following experiment:

"A ligature was thrown round the vessels attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind legs before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid."†

This experiment has been repeatedly confirmed.

But in the hands of other observers this experiment has not been attended with such decisive results. Thus, Dr. M. Hall relates the following:

"A ligature was applied round the aorta of a frog: the circulation in the web, which was previously very vigorous, was almost immediately arrested, first in the capillaries, then in the veins. In the arteries there was a singular oscillatory motion of the blood for ten or fifteen minutes. The globules of the blood proceeded slowly onward for some seconds; there was then, all at once, a rapid retrograde movement of the blood, apparently through the same space. This oscillation was repeated: the globules of the blood were again moved alternately in progressive and retrograde directions as before."‡

A similar description has also been given by other observers. Haller

* On Unity of Function in Organized Beings, by W. B. Carpenter: Edinburgh New Philosophical Journal, 1837. See also Von Baer: *Über Entwicklungsgeschichte der Thiere*. Königsberg, 1828—37.

† An Experimental Inquiry into the Laws of the Vital Functions, by A. P. W. Philip, p. 91. 1826.

‡ An Essay on the Circulation of the Blood, by M. Hall, 1831, p. 78.

appears to have performed many of these experiments, but not with a very uniform result. In several, the phenomena observed after excision of the heart, or section of the great vessels, varied. In many, the blood continued to move in its natural direction; in some instances it became oscillatory, and in others retrograde. In many of these experiments he states that after the heart had been removed, or a ligature had been applied to the aorta, or after its two great branches had been divided, the motion of the blood in the arteries continued, even for a considerable period; and he says, "It hath seemed to me that the motion is better preserved in the veins than in the arteries."* Indeed, he observed that after division of the vessels near the heart, the blood moved onward longer in the veins than in the arteries; it continued to move in the veins after the arteries were emptied.†

It appears to us that the majority of these experiments, which were attended with very decided results, ought to outweigh in value the remainder: for when such experiments are frequently repeated, it is not surprising that the results should sometimes vary, for they are amenable to the influence of many modifying causes; and it is not always easy, even with the utmost care, to avoid all sources of error.

But the opponents of the doctrine assume the less decisive experiments to be the correct ones, and explain the production of the oscillatory motion by the operation of extraneous influences, by the elasticity or tonic contraction of the vessels, by the movements of the animal, or other disturbing causes. When these experiments have been repeated with the variation of removing the heart instead of tying the vessels, the continuance of the capillary circulation has been attributed to the escape of the blood from the divided vessels, and their consequent contraction. Under these circumstances, we repeated the experiments. The following are the notes made at the time:

The medulla oblongata of a frog having been divided, the chest was laid open, and the heart and great vessels were exposed. The web was then placed under the field of the microscope, and the circulation observed. It was apparently in all respects natural. The whole of the heart, with a portion of the vessels, was then cut out with scissors by a friend, while we watched the circulation. Its rapidity was immediately and strikingly diminished, but the blood continued flowing on in the same direction for some few seconds, then the current in the smaller arteries oscillated, moving slowly backwards and forwards, its motion in the veins being very slow, but uniformly progressive. The capillaries seemed gradually to empty themselves into the veins.

Another frog was prepared in the same manner, and a ligature was passed beneath the aorta; while the circulation, which was natural, was observed, the ligature was tightened. The circulation almost immediately became much slower, but continued for more than a minute to move progressively onward with considerable speed, appearing to be in all respects natural, but slow. In between two and three minutes it gradually stopped.

Frogs are readily placed under the influence of chloroform, by causing

* A Dissertation on the Motion of the Blood, &c., by Albert Haller, 1757, translated.

† Deux Mémoires sur le Mouvement du Sang, &c., 1756, expt. 195.

them to breathe its vapour, and they become completely passive and motionless.

Having placed a frog thoroughly under the influence of chloroform, and prepared it, the circulation, which was natural but accelerated, we observed in the web. The blood was unusually florid. The whole of the heart, with the base of the great vessels, was then removed. The rapidity of the circulation was immediately and strikingly diminished; and in a second or two the blood began to oscillate in the arteries, but it continued at the same time to flow progressively, although very slowly, onward in the veins, the capillaries gradually emptying themselves into these vessels. When all motion had ceased, the capillaries contained scarcely any blood, the arteries none, but the veins were much engorged.

A frog was prepared in an exactly similar manner to the last, but the ventricle was removed below the origin of the aorta, so as to leave its valves uninjured. The circulation almost immediately became slower, but continued to flow onward with considerable speed. It was in all respects natural, and no trace of any oscillatory movement was visible. It was quite evident that the rapidity of the circulation was greater in the veins than in the arteries. The capillaries gradually emptied themselves into the veins. Indeed, after the circulation had quite ceased in the arteries (which happened in about two minutes), there was still some movement onward in the veins. During the whole time the frog was perfectly passive and motionless, and the movements of respiration had ceased.

These experiments were frequently and carefully repeated, with a generally uniform result; the chief variation being, that when the whole of the heart, with the origin of the vessels, was removed, the current in the arteries in one or two instances became *immediately* oscillatory, occasionally flowing backwards with some rapidity. But the current in the veins was always progressively, although slowly, onward. These experiments were witnessed on various occasions by several friends, who confirmed their accuracy.

The various results arrived at by different observers may, perhaps, be explained by a consideration of the following circumstances:

It has been remarked that great care is required in the performance of these experiments, in order to guard against the influence of all disturbing causes. Some contradictions may be explained by imperfections in the method of conducting them. Thus Dr. Black states—"The vessels of the web were observed in full circulation immediately before the animal was fastened for the experiment," &c.* Any one accustomed to prepare the frog's web for the microscope must be aware how easily the circulation is interrupted or disturbed.

It is to be observed that there is a striking difference in the effect upon the capillary circulation between excision of the heart, &c., and ligature of the aorta. In our first experiments, we were somewhat puzzled by obtaining two very different results from excision of the heart: sometimes the effect was similar to that produced by ligature of the vessels: at other times, the motion of the blood in the arteries immediately became oscil-

* A Short Inquiry into the Capillary Circulation of the Blood, &c., by J. Black, M.D., 1825, p. 60.

latory. This apparent contradiction we found to depend upon the place where the section was made. Whenever the division was made *below* the valves of the aorta, so as to leave them uninjured, the direction of the circulation was unaltered; but when, on the contrary, so much of the heart was removed as to include the aortic valves, the oscillations before described were immediately observed, for the blood then flowed freely backward through the open orifice. Many of the contradictory statements may be thus reconciled.

With regard to the oscillations which have been observed when a ligature has been employed, we would remark, that great care is required in exposing the heart, and more especially in passing a ligature round the aorta. It is no very easy operation to pass a ligature around the commencement of this vessel without injuring the auricles of the heart. Their delicate walls are very easily torn, and this accident may readily elude observation. No blood should escape if the operation be successfully performed; any hæmorrhage will indicate almost certainly that this accident has happened. Under these circumstances, so much blood is lost as to interfere more or less with the result of the experiment. The natural condition of the circulation is destroyed. The heart ceases to act with due effect upon the blood, for the ventricle does not receive its proper supply.

When these sources of error were guarded against, the results of the experiments appear to be tolerably uniform, whether the ventricle of the heart only be removed below the valves of the aorta, or whether the aorta itself be tied, so that regurgitation of the blood be prevented.

Although it is admitted that the mere fact of something like a circulation still continuing in the smaller vessels after the removal of the heart's influence, cannot be advanced as an unobjectionable argument in support of the doctrine—as it is impossible to say how much is due to the contraction or elasticity of the arteries—yet these experiments, when carefully examined, seem to demonstrate in a striking manner a power supplemental to the heart in moving the blood; and the seat of that power is no less evidently shown by the fact that, when the blood is oscillating in the arteries, its motion is regularly progressive in the veins; or when moving directly onward in the arteries, its rapidity may be observed for a time to be greater in the veins, although the latter vessels are the larger—so contrary to the rule under the influence of the heart's action. The empty state of the capillaries after all movement has ceased, when the heart has been removed or the aorta tied, contrasts strikingly with their engorged condition under circumstances which will be hereafter mentioned. These facts are far more important in connexion with the question than the mere length of time during which the movement of the blood continues after the removal of the heart's influence. It is obvious that the circulation in the capillaries must depend on their supply, and this, from the nature of the experiments, cannot last long.

All extraneous sources of error were guarded against with all possible care. A frog fully under the influence of chloroform is completely passive in all respects. Even the movements of respiration had ceased in most of the experiments.*

* The existence of those pulsating sacs situated upon the primary divisions of the aorta of the frog and toad, described by Dr. M. Hall (*op. cit.* p. 82), has not been overlooked; but it is obvious that they cannot influence the results of the experiments.

Similar observations have been repeated by Dr. Wilson Philip and others upon the mesentery of warm-blooded animals, but the severe shock occasioned by the operation on them interferes greatly with the result; while the exposure and disturbance of the mesentery, which must inevitably occur, greatly invalidates the results obtained.

"I have long ceased," Dr. M. Hall says, "to place the slightest reliance upon the circulation, as seen in the mesentery, in physiological experiments of any delicacy. The circulation of the web, on the other hand, is unequivocal under judicious management, the arrangement of the toes and of the membrane remaining accurately the same."

The following experiment was devised by Dr. G. Calvert Holland, as "much less exceptionable in its character than any with which we are acquainted, demonstrating the power of the capillaries to carry on the circulation:"

"A placenta was procured, twenty minutes after separation from the uterus, and placed, with the exception of the cord, in a bladder, which was immersed in water at the temperature of 100° Fahr. The free extremity of the cord at the same moment was elevated to an angle of 30°, resting on the edge of a glass, and at the distance of a foot from the placenta. At the commencement of the experiment, no blood escaped from the vein, but in two minutes from the immersion it began to flow, and continued for twenty minutes, and at this time it was found that the glass had received above one ounce."*

Relying, therefore, on the evidence afforded by the experiments which have been related, we cannot subscribe to the following statement:

"Whenever the action of the heart ceases or is impeded, the whole circulation ceases; and that, when an obstruction prevents the action of the heart from reaching the blood in any of the bloodvessels, the flow of blood ceases almost instantaneously in all the branches proceeding from the obstructed vessel."†

3. "After most kinds of natural death, the arterial system is found, subsequently to the lapse of a few hours, almost or completely emptied of blood." Indeed, the contrast between the empty state of the arteries and the engorged condition of the veins after death, is familiar to every one. This state has generally been attributed to the tonic contraction of the arteries which occurs after death.‡ But it will appear, upon consideration, that this explanation will not wholly account for the fact. "The emptying is commonly more complete than could be thus accounted for, . . . since their calibre is not found to have diminished in a proportional degree." Speaking of this assigned cause, Dr. Wilson Philip observes: "This may reduce, but it cannot wholly expel, their contents." This post-mortem contraction of the arterial tubes must be in proportion to the amount of muscular tissue contained in their walls. Now it is well known that, as a general rule, the relative proportion of muscular tissue in the walls of the arteries increases as these vessels diminish in size—the reverse of the arrangement which exists with regard to the elastic tissue; so that the walls of the larger arterial trunks, at a little distance from the

* The Forces by which the Blood is Circulated in the Capillary Vessels, by G. Calvert Holland, M.D.: Edinburgh Medical and Surgical Journal, July, 1842, p. 58.

† Cyclopædia of Anatomy and Physiology: Article, Circulation, by Allen Thompson, M.D., vol. i. p. 655.

‡ See Die Hämodynamik, nach Versuchen, von Dr. A. W. Volkmann, 1850, chap. xii.

heart, are composed almost entirely of elastic, with comparatively but a very small quantity of muscular, tissue intermingled; while in the smaller arteries, towards their termination, the muscular tissue is far more abundantly, and the elastic tissue very sparingly, found.* Now, if no other force were in operation upon the blood in the arteries than the muscular contraction of their walls, how are we to explain their "almost or completely emptied" state after death? It is obvious that if it were simply due to this cause, that the blood would be forced backwards into the larger trunks, as well as forwards into the capillaries, by the more complete contraction of the smaller vessels; for no contraction of the larger arterial trunks could, at any time, occur to produce a degree of constriction at all approaching to an obliteration of their cavity. Indeed, the following experiment of Mr. Hunter is an excellent illustration of this point:

"I found in the uterus of a cow, which had been separated from the animal above twenty-four hours, that after it had been injected, and allowed to stand another day, the larger vessels had become much more turgid than when I first injected them, and that the smaller arteries had contracted so as to force the injection back into the larger. This contraction was so obvious, that it could not but be observed at the time, which was forty-eight hours after the separation from the body of the animal."

He continues:

"This shows, too, the muscular power of the smaller arteries to be superior to that of the larger, and that it is probably continued longer after the separation from the body."†

Some very striking instances have been recorded by Dr. Bennett Dowler,‡ who has observed—

"That in the bodies of individuals who have died from yellow fever, the external veins frequently became so distended with blood *within a few minutes* after the cessation of the heart's action, that, when they are opened, the blood flows in a good stream, being sometimes projected to the distance of a foot or more, especially when pressure was applied above the puncture, as in ordinary bloodletting."

Dr. Carpenter, from whom the above is quoted, remarks:

"It is not conceivable that the slowly-acting tonicity of the arteries should have produced such a result as this: which can scarcely, therefore, be attributed to anything else than the continuance of the capillary circulation by forces generated within itself."§

In order to decide this question more completely, the following experiment was performed: Immediately after a woman was delivered, and while the umbilical cord *was still pulsating*, a portion, about six inches in length, was isolated, by means of two ligatures, the one nearest to the placenta being first applied. This portion was, directly afterwards, cut out just within the ligatures, and carefully laid aside. Some blood escaped at the instant of division. The tied extremity of the remaining portion of the cord attached to the placenta was then cut off, and some blood

* See Hunter. Works.

† Hunter's Works, edited by J. F. Palmer, vol. iii. p. 158.

‡ Researches, Critical and Experimental, on the Capillary Circulation, Jan. 1849.

§ Principles of Human Physiology, p. 496.

escaped. In a few minutes the placenta was removed, great care being taken not to handle the portion of the cord attached. This part of the cord was then removed from the placenta, and carefully laid aside. The two portions of the cord were examined twelve hours afterwards. Little or no difference was observed in the amount of blood contained in the veins of the two portions, but the arteries of the portion from the centre contained distinctly much more blood than those vessels of the portion which had remained in connexion with the placenta.

This experiment was repeated many times, with a very uniform result. Mr. Bletchley obtained for us portions of cords treated as above described. Either Mr. Holden or Mr. Coote, who were requested to ascertain the relative amount of blood in the vessels of the two portions, or we ourselves, examined these, and always found a marked difference in the quantity of blood contained in the arteries.

The cords were allowed to remain undisturbed for various periods before they were examined—from six hours to fifty or sixty—in order that the arteries might have ample time for their contraction; Hunter stating, as the result of his experiments—“That the vessels of the cord have the power of contraction above two days after separation from the body.”

4. In a careful microscopic examination of the circulation, in its healthy and natural condition, many irregularities may be observed in the speed and direction of the capillary currents, which it seems impossible to ascribe to any influence derived from a “*vis à tergo*.” Such variations are often distinctly seen in vessels which are supplied by the same trunk. A striking contrast is often presented by the velocity of the blood in two neighbouring channels. “Sometimes the red particles flow rapidly from one current into a second, as if by attraction.” Sometimes the direction of the current is completely reversed, which change is often preceded by a temporary stagnation. Such changes occur more frequently when the heart’s action is enfeebled. It has been, moreover, observed, that the velocity of the blood in the capillaries is often greater than “in the trunk, whence several arise, which could not happen in vessels the joint area of which increases as they subdivide, if the only cause of motion were an impulse *à tergo*.”* Attempts have been made to explain these changes and variations in the capillary currents, by asserting—

“That variations of pressure, and position, and motions of the animal, are always the causes of these changes. All these variations in the capillary currents are, then, just as in currents of water or irrigated land, merely the results of mechanical causes.”†

These explanations will not be deemed satisfactory by any one who has been in the habit of watching these variations in the capillary currents. They are seen when the animal is perfectly passive, and when no pressure or other mechanical causes can be conceived to exist. Such causes certainly often interfere with and influence the capillary circulation, but it is quite possible, with care, to guard against all such sources of error, and these changes occur independently of any extraneous cause which can be conceived. Moreover, an attentive examination of the circulation will

* Outlines of Physiology and Pathology, by W. P. Allison, M.D., 1836. Supplement, p. 22. Haller, Mémoires, &c., p. 56; and expt. 62, 68, 72, 98, 262, &c.

† Müller’s Physiology, by Dr. Baly, vol. i. p. 221.

impress the mind with a conviction that the phenomena observed must depend upon causes operating in the part, and are alike inexplicable either by supposing them to result from the action of the heart or vessels, or from the operation of extraneous causes. The following observation throws much light upon this question. It is especially valuable because made by Mr. Wharton Jones, and recorded without reference to the present inquiry :

*“Effect of Section of an Artery of the Web of the Frog on the Flow of Blood in the Part.—*When an artery is cut across, it immediately becomes constricted, even to obliteration of its calibre, upwards in the direction of its trunk, and downwards in the direction of its ultimate ramifications. The flow of blood is thus arrested, and the immediate consequence is, an ex-sanguine state of the heart, to which the ramifications of the artery lead. But this state of matters is not of long duration. In the course of a minute or so, relaxation of the wall of the artery and dilatation take place, both above and below the wound. In the upper part of the artery, the flow of blood is re-established as far down as the first considerable branch proceeding from it above the place of section. By this branch the stream of blood passes off. In general, none of the blood, except a stray corpuscle now and then, enters the artery further, although it has become dilated down to the place of section, where, however, the cut end of the vessel continues closed by constriction. Into the part of the artery below the section, blood, of course, no longer enters directly. It enters, however, in a retrograde direction, and very slowly, by one set of branches, and passes out in a direct course, but still very slowly, by another set of branches. The blood which enters the artery below the section, in a retrograde direction, regurgitates from the capillaries and veins to which the branches lead by which the blood enters; and if the cut artery has a direct anastomosis below the section with another artery, blood also regurgitates by that anastomosis.”*

Now, in this experiment, why is the flow of blood re-established in the upper part of the artery *only* so far down as the first considerable branch proceeding from it above the place of section? Why is it that—“In general, none of the blood, except a stray corpuscle now and then, enters the artery further, although it has become dilated down to the place of section?” Can this be explained by any “*vis à tergo*,” or by any mechanical force? Is it due to any other cause than some power in operation at the capillaries to which the vessel leads, which draws the whole of the blood passing through the vessel in that direction?

5. It is well known that there are certain organs which are destined, under different circumstances, to undergo considerable changes in their development and activity. The best examples are—the uterus after conception, the mammary glands during pregnancy and lactation, and the testicles of various animals at certain periods. The increased development of such organs is attended by an increased determination of blood to their tissues, without any change either in the heart's action or in the general circulation. Other instances, in which the flow of blood to particular parts or organs is augmented, may be found wherever there is any increase in the activity of the process of nutrition, secretion, &c. Such temporary changes are of frequent occurrence; but the most obvious example is to be seen in the change which an organ undergoes, when, from disease, its fellow becomes incapable of duly performing its functions.

* On the State of the Blood and the Bloodvessels in Inflammation, &c., by T. Wharton Jones, F.R.S.: Guy's Hospital Reports, vol. vii. part 1, pp. 23, 24. See also Haller, *Mémoires*, &c., expt. 54; and *Experimenta circa statum Sanguinis et Vasorum in Inflammatione*, auct. G. Kaltenbrunner, 1826, pp. 4, 5, 20, 21.

This often occurs in the kidney. The structure of one of these organs becomes damaged, and the function correspondingly impaired. Thus additional work is thrown upon the remaining healthy one,—its tissue is considerably increased,—it becomes hypertrophied, and the flow of blood to it is proportionally augmented. Moreover, it is to be observed that under these circumstances not only is the quantity of blood passing to the part or organ augmented, but its circulation is more rapid.* Now, upon what cause does this increase in the quantity of blood, and in the rapidity of its flow, depend? Obviously not upon any change in the force of the heart's action, for this would affect the system generally. Indeed, there is no relation between the activity of the heart and the circulation in certain parts. "The distribution of the blood is not in the least influenced by the heart."† The cause of these local determinations of blood has been sought for in the arteries.‡ It has been supposed by some to depend on an increased action of these vessels. But the only active power possessed by arteries is contraction, due to their muscular tissue. This could only act to constrict their calibre, and so diminish the quantity of blood passing through them. Those obscure notions about a vermicular contraction need no reply. But attempts have been made by high authorities (Magendie, Mayo) to explain these local determinations of blood by a relaxation of the arteries leading to the part, and a consequent diminished pressure on the blood. Such dilatation may possibly accompany such changes, but they cannot be regarded as the cause. If this were so, various irritations which excite local determinations of blood "must act as *sedatives* on the contractile power of the arteries; which is not only the reverse of their action on more strictly irritable textures, but is the reverse of the effect which they have been often observed, in experiments, to produce and keep up, for a considerable time, on individual living arteries, to which they have been applied."|| Moreover, local determinations of blood are often the "consequence of a strictly local cause;"§ and it cannot be conceived how this can act to produce relaxation of an artery. When, for instance, the capillary circulation is examined under the microscope, it is observed that its rapidity is increased by moderate stimuli and retarded by sedatives;¶ and these experiments afford an additional proof that the cause operates at the capillaries, and not elsewhere.

Again, relaxation of an artery could not explain the increase in the rapidity of the flow of blood through the vessel; indeed, this cause alone would tend to retard it. Moreover, if this local determination be kept up for any length of time, as in the uterus or mamma, the arteries leading to such parts undergo themselves an increase of development—they become enlarged, thickened in their walls, and tortuous. The subsidence of this increased activity in a part or organ is attended with a corresponding

* See Wharton Jones, op. cit.

† Volkmann, op. cit., p. 341.

‡ Because variations in the capillary circulation are of course accompanied by corresponding variations in the flow of blood through the arteries supplying them, it has been assumed that the arteries are the "regulators of the capillary circulation." Are not the cause and effect here reversed?—See section vi.

§ Allison, op. cit., supplement, p. 20.

¶ Carpenter: Principles of Human Physiology, p. 497.

¶ Wilson Philip, op. cit., 285—6. Also, an Inquiry into the Nature of Sleep and Death, 1834, p. 72.

diminution in the amount of blood passing to it. It seems equally impossible to explain this by supposing the contraction of its arteries to be the cause. "It is difficult to understand how the heart is not able to maintain this increased flow of blood after the periods of activity in the functions of nutrition and secretion have ceased, if we look for an explanation of this in any property possessed by the coats of the blood-vessels."*

In the consideration of local determinations of blood, mere evanescent changes are generally included—such as the act of blushing, &c.—also the changes which ensue in erectile organs or tissues. Now neither of these states has been brought forward in the previous remarks, because they appear to depend on very different causes, or at all events they are open to certain objections which will not apply to the other cases. For instance, the evanescent phenomena of blushing seem to be immediately dependent on some "nervous influence," which is at present involved in considerable obscurity; while the phenomena of erection appear to be clearly due, in a great measure, to some special and peculiar structure of the parts in which they occur.

If, then, the cause of the changes which have been considered can be found neither in the heart nor arteries, it must be sought for in the part itself. "If more blood is carried to one organ than to another—i. e., more than the normal width, length, form, and arrangement of its vessels justifies, we have to deal with a case in which we must seek for the explanation in new moving forces."† The nature of these forces will be hereafter inquired into, but they must be conceived, from the previous considerations, to operate upon the blood while in the capillary vessels, and so to affect it as to increase its local determination and the rapidity of its motion.

6. The proofs upon which this statement rests are to be found in the arguments advanced in the previous section, and also in those contained in sections 4 and 7.

For it is proved that the determination of blood to any organ is the result and not the cause of the local action. This statement is well illustrated by the different changes which occur in the arteries of a limb, a portion of which has been amputated, and where a ligature has been placed on the main trunk for the cure of aneurism.

"When an artery is tied after amputation, the end of the vessel, although exposed to the influence of the heart and arteries, does not become enlarged, nor are the branches given off by it above the ligature dilated. In the dissection of several stumps I have invariably found a considerable extent of the main artery, as well as the branches arising from it, remarkably contracted."‡

Contrast this account with the following:

"The dilatation of the vessels, by which a collateral circulation is carried on, takes place principally in the minute ramifications. The trunks of the branches from which these ramifications originate are very inconsiderably enlarged, even when a collateral circulation has been for a long time established. In several preparations which I have examined at different periods after the artery had been

* *Physiological, Pathological, and Anatomical Researches*, by John Reid, M.D., 1818, p. 47.

† *Volkman*, *op. cit.*, p. 341.

‡ *On the Diseases of Arteries and Veins*, by Joseph Hodgson, 1815, p. 262—4. See also Wharton Jones' *expt.* before quoted, pp. 32, 33.

tied, the mouths of the branches above the place of obstruction in the main artery did not appear larger than in their natural state, and in a few instances only a slight dilatation was perceptible." (p. 247.)

Whence this difference? Why, when a portion of a limb is removed, are the vessels considerably diminished in size, while when the whole limb remains the collateral branches become enormously enlarged? It is especially interesting to mark the vessels in which this dilatation occurs—to observe that it "takes place principally in the minute ramifications," because it amply refutes all notion that the enlargement of the vessels is the result of mere distension "in consequence of the obstruction," or that it can be due to any *vis à tergo*. The influence of the cause is manifestly *retrograde* along the vessels.*

Again, how can we explain the difference between the vigorous flow of blood in the vessels of a healthy, well-nourished limb, and the languid circulation in one that has been long kept at perfect rest—as in a state of paralysis, or of some chronic disease of a joint—if not by admitting that the quantity of blood passing through the arteries is regulated by causes operating in the part?

When the placenta is detached from the uterus, with the foetus, at birth, the pulsation of the cord ceases first at the placenta, and afterwards at the umbilicus. After such pulsation has ceased, section of the cord is followed by the escape of comparatively very little blood; in many instances, by none whatever.† Whence this change? Why does the blood thus desert the umbilical cord? The heart and other forces operating from behind are not less active. Indeed, the contrary is the fact. The case is a remarkable one, and seems susceptible of but one explanation.

Admitting, then, these facts, it is clear that the local causes which can regulate the quantity of blood passing through the arteries must act by influencing the circulation in the parts supplied by them. The nature of this influence will be subsequently considered.

7. Perhaps the best illustration of this statement is afforded by the changes which ensue when the process of respiration is impeded or arrested. There are few subjects about which more controversy has arisen, or which have more fully engaged the attention of a greater number of eminent physiologists, than the pathology of asphyxia. Since the time of Haller, doctrine after doctrine has been advanced and refuted. But at length it appears to have been clearly proved, that the first of the morbid changes is an obstruction to the passage of the blood through the capillaries of the lungs, and that this obstruction is due to the cessation of the chemical changes between the air and the blood—to the cessation of the changes which normally occur in the blood during its passage through the capillaries of the lungs. It cannot be necessary now to retrace the various facts and arguments upon which this conclusion is founded. The task has been already repeatedly performed, and various objections which have been occasionally advanced have been satisfactorily answered. As to the relation the morbid changes which follow this

* Alison, op. cit., p. 30. Reid, op. cit., pp. 47, 48.

† On the Circulation of the Blood in Acardiac Foetuses, &c., by John Houston, M.D.: Dublin Journal, 1843, vol. xxiv. pp. 348—9. Graves's Clinical Medicine, 1843, pp. 482—3.

primary one bear to each other, our knowledge is less clear and certain, but with these we are not, in the present instance, concerned: it is simply with the fact, that the circulation of blood through the capillaries of the lungs is obstructed in proportion as the changes which it naturally undergoes in those organs are prevented.

Some objections which have been advanced against this statement by Mr. Erichsen* require notice. The first of these objections is—

“That provided the heart’s action be maintained, black blood may be made to circulate through a lung (in which the chemical changes have entirely ceased) for a much longer period than in ordinary cases of asphyxia, the same force that keeps up the circulation of red blood in one lung sufficing for that of black blood in the other.” (p. 24.)

And this statement is founded upon two experiments, which are detailed.† In these experiments—

“A tube was adapted to the trachea of a young spaniel. The animal was immediately pithed; artificial respiration was then set up, so as to maintain the heart’s action, and the chest was laid open. A ligature was next passed under the right bronchus, close by the bifurcation of the trachea, and tied tightly, so as to prevent the entrance of any air into the right lung. This was done in about three minutes from the time that the animal was pithed. The inflation, which was only carried on by the left lung, was now continued—the right lung remaining in a mid state between collapse and distension, and quite motionless. The heart was beating tumultuously—about 120 per minute.”

In the first experiment—

“9th minute after ligature of bronchus: heart beating forcibly, but rather irregularly, from 60 to 70. A ligature was now passed under one of the pulmonary veins of the right lung (the obstructed one), and tied as near as possible to its entrance into the auricle. The vessel was then punctured on the distal side of the ligature, and a jet of semi-venous blood escaped. The blood was not quite black, owing to regurgitation having taken place from the left auricle, as the pulmonary vein was not ligatured until after inflation had been established for several minutes. . . . 11th minute: a quantity of venous blood trickles out of the puncture in the pulmonary vein.”

Up to the 17th minute, black blood continued to ooze from the puncture, slowly, but very distinctly. In the second experiment—

“At the 10th minute after the bronchus had been tied, the heart was beating 60 to 64, strongly and regularly; artificial respiration was kept up with the left lung alone, the right one being quite collapsed. . . . 14th minute: A ligature having been passed under a pulmonary vein of each lung, these vessels were punctured on their distal side. A quantity of black blood flowed from the pulmonary vein of the right lung, whilst from the corresponding vessel of the left lung florid arterial blood escaped in a small jet. . . . 17th minute: From the pulmonary vein of the right side dark blood is still flowing, rather slowly, but very distinctly; whilst from that of the left lung a larger quantity of arterial blood is evidently escaping. . . . 19th minute: Flow of blood still continues, but very slowly. . . . The blood that had accumulated in either side of the chest was carefully collected and measured, when it was found that that which had flowed from the right lung amounted to 2½ drachms, while that from the left amounted to 3½ drachms; and in another experiment of the same kind the quantities that escaped were respectively 2½ and 4 drachms.”

* An Experimental Inquiry into the Pathology and Treatment of Asphyxia, by John E. Erichsen: Edinburgh Medical and Surgical Journal, Jan. 1845.

† Experiments 10 and 11.

What evidence do these experiments furnish that "the circulation of black blood may continue through the lung for a considerable time beyond the period at which the circulation ordinarily ceases in asphyxia?" In twenty-four minutes the quantity "which had flowed" amounted to $2\frac{1}{4}$ drachms. That so small a quantity as $3\frac{1}{2}$ drachms only flowed from the left lung may, we think, be easily accounted for by the nature of the experiment. Artificial respiration was set up, and *the chest was laid open*. We can scarcely imagine the functions of either lung to be continued, to any great extent, under these circumstances. But it must also be remembered—and this fact is generally overlooked in all such experiments—that "when the access of air to the lungs is suddenly and completely checked, the circulation through them continues for some little time," because

"A considerable quantity of air is contained in the air cells of the lungs; and that it is not until this has been so far deprived of its oxygen, and loaded with carbonic acid, as to be unfit to effect any change in the blood, that we should expect the movement to be entirely checked. Moreover, the alteration in the character of the whole mass of the circulating fluid is effected gradually, as might be inferred from the small proportions transmitted by the heart at each contraction; so that, if a small stream be drawn from the carotid artery of an animal undergoing asphyxia, it will be seen to become progressively darker from the commencement of the suspension of the respiratory movements to the cessation of the heart's action."*

This important fact did not escape the attention of Bichat, who proves it thus:—If the access of air to the lungs be suddenly checked immediately after an inspiration, the change of colour in the blood occurs more slowly than if the supply of air be arrested immediately after an expiration, and the change is especially rapid after a forced expiration; or if, by means of a syringe adapted to the tube in the trachea, the air be drawn out of the lungs, the colour of the blood passes from red to black very suddenly—twenty or thirty seconds suffice for the change. Whereas, if air be forced into the lungs, beyond what is taken in by the deepest inspiration, and retained there, the blood remains of its natural colour for a much longer time—it does not become darkened for more than a minute—it does not flow out completely black till the end of three; and this varies according to the state and the quantity of air that is thrown in.†

Again, it must be very difficult to tell when the blood has become "perfectly venous," and has passed through the capillaries of the lungs independently of any change. Certainly, mere inspection is very unsatisfactory.‡ That it continues to circulate so long as any change occurs, but only in a degree proportionate to the extent of the change, is a very conclusive proof of the truth of the doctrine.

Mr. Erichsen appeals, also, to "the fact of the circulation continuing actively in lungs that are compressed by effusion into the pleural sac," as evidence "that the heart's action is of itself sufficient, when vigorous, to keep the circulation through a lung in which the chemical changes have

* Library of Medicine, vol. iii.: Article, Asphyxia, by Dr. Carpenter, p. 225.

† *Récherches Physiologiques sur la Vie et la Mort*, par Xavier Bichat, 1805, pp. 241—2.

‡ This is strikingly illustrated by the contradictory statements which have been advanced as the result of actual observation.—See Bichat, *loc. cit.*; Kay, *op. cit.*, p. 188, &c. &c.

ceased." (p. 24.) Conclusions drawn from such data must be acknowledged to be, in the highest degree, uncertain. How can we estimate, in any way, the amount of blood passing through the compressed lung, and when can we say that the chemical changes have entirely ceased?

The other objection is the theory,

"That the obstruction which has been proved to take place in the pulmonary and systemic circulation, is due to the venous blood exciting the contractility of the minute divisions of the arteries and pulmonary veins, by acting upon their special sensibility." (p. 31.)

The arguments which Dr. Reid has opposed to this theory appear to be conclusive; for he shows that the obstruction cannot be caused by the venous blood acting as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, because incompatible with some well-established facts. The contractility possessed by the vessels manifests itself by slow contraction, followed by equally slow relaxation.

"When contraction has been induced in the coats of an artery by an excitant, it is well known that relaxation does *not follow suddenly*, even on the withdrawal of an excitant, but it occurs slowly and gradually. If, then, the ultimate ramifications of the pulmonary veins are stimulated to contraction by the venous blood, relaxation ought *not to follow instantly* upon the withdrawal of this excitant, and the entrance of pure atmospheric air into the lungs ought not to be instantly succeeded by the free passage of blood from the right to the left side of the heart. The following experiment, mentioned by Bichat, and which I have frequently repeated, appears to me to be an *experimentum crucis* upon the point under discussion. If a tube with a stop-cock upon it be tied into the trachea of an animal, and the stop-cock turned to exclude fresh air from the lungs until the circulation of dark blood along the arteries has become much enfeebled, as ascertained by exposing a large artery and making an opening into it, *instantly* on the admission of fresh air into the lungs the blood springs from the cut artery of a bright red colour, and with greatly increased force. I have observed the same thing repeatedly when the hemodynamometer was fixed in the femoral artery; no sooner was the stop-cock opened, and fresh air permitted to enter the lungs, than the mercury suddenly sprung up several inches in the ascending portion of the tube of the instrument."*

Thus, then, the passage of the blood through the pulmonary artery is impeded by any cause which interferes with the normal changes which the blood undergoes in the capillaries of the lungs.

But the above is not the only illustration which can be drawn from the morbid changes which occur in asphyxia. Dr. Reid has shown most conclusively, that if the blood becomes venous in the arteries, it will not circulate freely through the systemic capillaries; that it is obstructed in its passage through these vessels, causing increased pressure on the walls of the arteries. These experiments appear unexceptionable. Again, Dr. Kay relates an experiment which shows that it is very much more difficult to force venous than arterial blood through the capillaries of the lung. The same fact is proved by the experiments of Mr. Wharton Jones, who caused the blood to stagnate in the capillaries of a frog, by directing upon them a stream of carbonic acid gas.† Also the fact before

* Dr. John Reid, *op cit.*, pp. 45—6.

† *British and Foreign Medical Review*, vol. xiv p 600.

mentioned, that the circulation in the placenta, and therefore the flow of blood to it, ceases when it is detached from the uterus, may be here again referred to.

Dr. Allen Thompson has said,

“We are not acquainted with any facts or experiments which show that the systemic capillary circulation is immediately dependent upon the change of the arterial into venous blood. On the contrary, such an opinion is opposed by the facts—that a free circulation of imperfectly arterialized blood takes place in the foetus before birth, as well as in many children after birth, affected with malformation of the heart or greater vessels; and that a completely venous blood circulates through the system in hybernating animals, when in the state of deepest torpidity.”*

Now the facts here referred to are very important, because they are very closely connected with the question. If it can be proved that the blood continues to circulate through the capillaries independently of *any* change, then assuredly an incontrovertible argument is advanced against the doctrine under consideration. But, on the other hand, if it can be shown that a change, however slight, does occur, and that the activity of the circulation is in direct proportion to the extent of this change, then, on the contrary, we have an irresistible argument in support of the doctrine.

To say “that a free circulation of imperfectly arterialized blood takes place in the foetus,” is hardly stating the whole of the case. We know that in the foetus different parts receive blood of a different character, and we have also good evidence for believing that the activity of the circulation varies accordingly; and moreover, that in the foetus the blood, as a whole, undergoes a most decided change; and that upon this change the life of the foetus directly depends, there can be no doubt. “In many children after birth, affected with malformation of the heart or greater vessels,” we certainly witness the circulation of imperfectly arterialized blood, as in cases of cyanosis. But is it “free”? On the contrary, a languid circulation is most characteristic of the disease, and its torpidity strikingly corresponds with the deficiency in the change of the blood. Lastly, what is the condition of the respiration and circulation in hybernating animals? Dr. Marshall Hall, who has most closely investigated this subject,† in his account of the extent to which the respiration is suspended, says, in different places:—“The respiration is very nearly suspended in hybernation.” (p. 769.) “I think it right to remark, that after the apparent total cessation of respiration . . . there is probably still a slight diaphragmatic breathing.” (p. 769.) Then again he says, “although the respiration be suspended,” &c. (p. 772.) “The respiration is nearly, if not totally, suspended.” (p. 772.) “In the midst of a suspended respiration,” &c. (p. 772.) That the changes which the blood naturally undergoes in respiration are, in animals in a state of hybernation, nearly suspended, is proved not only by the analysis of a confined portion of air in which they have kept for a considerable time, but also by the fact that they can bear the total deprivation of oxygen, as by immersion in water, for a considerable period. But that some change is effected in the blood seems equally proved by the fact, that they cannot wholly

* Op. cit. p. 677.

† Cyclopædia of Anatomy and Physiology, vol. ii.: Article, Hybernation.

dispense with the presence of oxygen. If no change occurred in the blood while passing through the capillaries of the lungs, animals, while hybernating, ought to be entirely independent of the presence of atmospheric air, for any length of time, while hybernation lasts; but this, we know, is not the case. Indeed, it cannot be doubted, however deficient the respiratory movements may be, that the blood undergoes some change in its passage through the capillaries of the lungs.

Now, what is the condition of the circulation in an animal in a state of hybernation? The same authority says, "The circulation is reduced to an extreme degree of slowness, according to a law well known, but hitherto, we believe, unexplained: according to which the respiration and the circulation are always proportionate to each other." (p. 771.) What clearer evidence could be furnished in support of any doctrine than this—that the activity of the circulation varies with the extent of change which the blood undergoes in the capillaries? And does not this doctrine furnish a satisfactory explanation of the law "hitherto unexplained?" A law generally observed under the various conditions of health, and often still more strikingly illustrated in disease. Indeed, if this doctrine be true, it is apparent that, within certain limits, the respiration and the circulation would be proportionate to each other.

During hybernation the condition of the animal is reduced below that of the reptile; but the analogy is obvious. In reptiles we are more familiar with the fact, that a darker blood circulates more languidly throughout the system. They may with truth be said to live more slowly; yet we find in them, also, the same laws in force, modified, but not annulled. Neither here, nor elsewhere, does the blood flow through the capillaries independently of change.

We conclude here our discussion of the several arguments in favour of a special local force in the capillary circulation; and in the next number we shall consider the opposite arguments, and complete our review of the subject.

(To be continued.)

William S. Savory.

REVIEW IX.

On the Formation and Extension of Cancer-Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation.

By J. L. C. SCHROEDER VAN DER KOLK. ('Nederlandsch Lancet,' Sept. 1853.)

THE paper by the eminent Professor of Utrecht, which we have quoted above, appears to us to be of such interest, that we have thought it advisable to give a very full abstract of it, and as often as possible to use the author's own words.

"Among the various diseases to which the human frame is liable," says the author, "none have ever been looked on as more formidable than the so-called malignant tumours, which exhibit themselves in their several stages as scirrhus, carcinoma, and medullary fungus; not only on account of the insufficiency of art to contend successfully against their development and extension, but especially by reason of the great uncertainty which exists, that the removal of these diseases by

operation shall be permanently successful, since in most instances they break out again, after a shorter or longer interval, with renewed violence, and usually bring the unhappy sufferer to a painful and torturing end.

“Although these diseases have of late years been much and amply treated of by various writers, by whom their structure and development have been more accurately investigated under the microscope, I think every contribution, however trifling, to a more accurate knowledge of the origin and dissemination of maladies which so obstinately resist all remedies, and defy the efforts of the healing arts, sufficiently important to excite general attention and closer investigation.”

Having made these prefatory remarks, the author alludes to some observations on the origin, development, and dissemination of these diseases, communicated by him in the year 1847 to the sectional meetings of the Provincial Society of Utrecht,* where he showed, that around a carcinomatous tumour, in tissues which, to the naked eye, appear to be perfectly sound, the disease becomes developed under the form of little cells, gradually increasing in size, and collecting in larger and more numerous groups in proportion as we approach the swelling; and, in some cases, in advanced stages of the affection, extending tolerably far into the surrounding parts.

“Without going,” continues the author, “into any detailed considerations, I shall briefly communicate my observations on the subject, in the first place, in reference to the spread of epithelial, and subsequently to that of ordinary, cancer.

“If we divide an extirpated underlip, more or less affected with epithelial cancer, in such a manner as to obtain an anterior and posterior portion, we shall be able, with tolerable accuracy, to observe the extension of the disease, so far as we can follow it with the naked eye. Generally speaking, the disease has a greater tendency to spread along the edge of the lip than downwards in the direction of the chin. If we now, with a sharp knife (I generally use a razor for the purpose), take a very thin layer from the morbid portion of the section, we shall see that the disease is formed by an assemblage of epithelial cells. These become smaller as we approach the boundaries of the affected part. If we now accurately examine the neighbouring parts, which still appear sound, under the microscope, we shall find therein (in quantity varying in proportion as we remove further from the affected parts, and go deeper into the apparently sound tissue) little cells, nuclei, and, finally, granular matter and fat-corpuscles, scattered among the healthy tissue, so that we can never observe the progress and development of these cells more accurately than in these very tumours; in fact, we can often most clearly see, in the same microscopic field, the entire progress of the transition of nuclei, around which a cell has as yet scarcely formed, to the fully-developed epithelial-cell; and with a strong power, we may observe a soft more or less granular matter being deposited around the nuclei, and cells forming precisely in the manner represented by Schwann.”

Investigating more closely their diffusion in the neighbouring parts and muscular fibres, the author found the nuclei and cells to extend principally along the areolar tissue between the fibres, so that in some parts of this tissue, little groups of cells had collected, while on the fibres themselves, several nuclei and little cells lay scattered. In other places, granular matter and nuclei, in the stage of first formation, alone were to be seen in the tissue, while scattered among them were fat-corpuscles, easily recognisable by their darker contours.

* Aanteekeningen in de Sectie-vergaderingen van het Prov. Utr. Genootschap, 1847, pp. 27, *et seq.*; also translated into Swedish, by Dr. Wahlgren.

"In the muscular fibres themselves, it was difficult to trace any alteration; even when they were tolerably abundantly covered with nuclei and little cells, the transverse striæ could still, if sufficient light was employed, be perceived. In some places these, however, disappeared, and seemed to be replaced by longitudinal striæ, presenting a more fibrous aspect; in individual cases, I even succeeded in recognising a dissolution and degeneration of the muscular fibre itself, the latter appearing to be resolved into transverse divisions or cells, intermingled with numerous fat-corpuscles; in fact, the fibre seemed to have undergone a fatty metamorphosis."

The author could not, with any certainty, perceive degeneration in the nerves in these cases of cancer of the lip: these organs appeared long to resist the disease. In several places he even found bundles of nerves entirely surrounded with epithelial cells, while within the neurilemma no trace of any degeneration was visible; it was only in isolated spots that he now and then observed nuclei and little cells between the nervous fibrillæ, but in much smaller number than between the muscular fibres; in a few instances he saw solitary fibrillæ, covered with epithelial cells, passing out through the connecting tissue. In other cases of carcinoma, however, he frequently met degeneration of the nerves, as shall be mentioned hereafter.

An unusual number of epithelial cells and nuclei was also present in the follicles and crypts of the beard. Omitting, however, a more detailed description of epithelial cancer and the changes it gives rise to, the author passes to the consideration of the more immediate subject of his paper—namely, the extension of the degeneration when the disease has been once established.

"It is quite unknown," he observes, "what may be the cause of the extensive cell-formation by which new cells appear constantly to form in the parenchymatous fluid pervading the parts which are still sound, from little nuclei and fat corpuscles, gradually assuming the form of epithelial cells. It may, however, be looked upon as certain that, between these cells and the interstitial fluid, a considerable interchange of material takes place, so that the fluid present in the tumour between the cells acquires other elements than exist in the parenchymatous fluid of the neighbouring sound parts. Both fluids, however, meet at the boundaries of the tumour, and must intermix; and thus, in my opinion, can we best explain how nuclei and cells form in the surrounding tissue as well as in the tumour itself, where the number of these cells increases so much.

"As, however, the parenchymatous fluid penetrates all parts, principally in the course of the areolar tissue, it is evident why the new formation of nuclei and cells takes place between the muscular fibres and other structures chiefly along this membrane, and thence extends into the adjacent parts. These parts, however, for a tolerably long time, offer resistance to the injurious influence and pressure of the cells constantly accumulating in the interstices, so that we cannot discover the least change of tissue with the naked eye, although the microscope shows us that a greater or less formation of cells has already penetrated between the healthy structures."

The author first discovered the importance of accurately tracing this cell-formation in an extirpated lip sent to him for microscopic examination. In this case, although the tumour, which consisted entirely of epithelial cells, appeared to the naked eye to be surrounded to the extent of a line and a half or two lines with healthy structures, he observed, on microscopic examination of a very thin layer of the edge of the removed portion, a number of granules, nuclei, and already-developed epithelial

cells, from which he inferred that the edges of the wound of the portion of the lip left behind should contain the seeds of the disease, that the latter had been incompletely removed, and that a relapse was inevitable; an opinion which was verified within three months—notwithstanding that the wound had healed rapidly and favourably—by the return of the disease, which, after causing fearful destruction, terminated in the death of the patient.

When we examine the nature of these cells, we shall see that it is not likely that, when once formed, they will be removed by the inflammation consequent on the operation, especially as the wound made in extirpating cancer of the lips generally heals by the first intention. The latter fact almost excludes the possibility of the destruction by suppuration of the cells and nuclei which have been left behind—

“And it will, therefore, depend on the degree of development of these cells, and on the greater or less amount of subsequent irritation and access of fluids, and on the interchange of material thus brought about, whether, after the healing of the wound, these cells shall again begin to be developed; forming, on a greater scale and with renewed violence, a carcinomatous ulcer, which, generally speaking, is no longer amenable to treatment. It is, however, not less important to note that the parenchymatous fluid, which, in the cancer of the lip and the parts adjacent, is already full of cells and nuclei, and is tainted through the interchange of material, not only gradually penetrates and extends still further into the surrounding parts, and thus gives rise to a constantly spreading and serious ulceration, but is also taken up by the lymphatics, whence arise similar lesions in other and sometimes distant situations. Although this observation applies much more strongly to true cancer, we are not without examples of it in the epithelial disease. Thus, my friend Professor Donders mentioned to me that, in a case of cancer of the lip, he had found the glands in the neck, next the larynx, swollen, and quite filled with epithelial cells. Lebert, Bennett, and Hannover have remarked a similar extension of epithelial cells to neighbouring glands. In epithelial cancer of the bladder, Schrant found cavities in the humerus and vertebræ filled with epithelial cells; a cavity also occurred in the os frontis. Virchow describes similar cases. These observations are, indeed, very important, as demonstrating the absorption of the parenchymatous fluid, and the tendency to the formation of epithelial cells, thence derived. Whether the latter might be explained solely from the chemically-altered constituents of the infected and absorbed parenchymatous fluid, or whether we must assume that some of these minute nuclei or little cells have forced their way into the lymphatics, may appear doubtful, as even blood-corpuscles, which are larger, occasionally occur in these vessels, having entered in another manner. It, however, seems to me probable that we must refer this cell-formation in the lymphatics to an alteration in the parenchymatous fluid, since, as we shall hereafter see, the cancer-cells may even form within a structureless membrane (sarcolemma) surrounding the muscular fibres, and through which no nuclei can penetrate. In a case of epithelial cancer of the tongue, I found not only that the epithelial cells had penetrated deeply between the muscular fibres, to the root of that organ, but that a carcinomatous tumour had at the same time formed in the neck, passing into open cancer, under which the patient sank, and in which I found ordinary cancer-cells, but no epithelial cells; whence it would appear that in epithelial cancer, after the absorption of the parenchymatous fluid, a general infection and secondary cancer-cells of a different kind may arise, which sufficiently demonstrates the analogy of the two affections.* The diffusion of cells which occurs in epi-

* Some writers, as Lebert (*Physiol. Patholog.*, § 2, p. 615) and Hannover, deny that epithelial cancer can degenerate into true cancer, or that the one disease may give rise to the other. Hannover considers that even where true cancer supervenes in epithelioma, only a combination of the two diseases had existed, as epithelial-cells cannot pass into cancer-cells. (*Das Epithe-*

thelioma, in the parts adjacent to the swelling, takes place in no less a degree in ordinary cancer and medullary fungus, which is only the acute form of the same disease.

"In ordinary cancer, as well as in epithelioma, the cell-formation around the tumour follows the direction of the parenchymatous fluid which is contained in the areolar tissue, and in this way it penetrates the textures of the surrounding organs. I perceived this very plainly, among other instances, in the case of a woman nearly eighty years of age, who had a medullary fungus in the calf of the leg, by which the gastrocnemius was in great part destroyed. In many places the muscular fibres could no longer be recognised, in others the texture was still plainly visible. On microscopic investigation, it appeared that in some of the parts most affected the primitive muscular bundles had entirely lost their structure; a fibrous form was indeed still to be perceived, but it exhibited itself only as a mass of cells and nuclei, more or less linked together longitudinally, and in many situations enclosed in the unaltered sarcolemma; in other parts the primitive bundles were still entire, exhibiting even their transverse striæ; but they were very transparent, charged, to a greater or less extent, with fat corpuscles, and covered with some cells under the sarcolemma.

"It shortly attracted my attention, what a very important alteration the primitive bundles had in many places undergone,—the fibres being changed into fibre-cells of various lengths, which, now lying closely packed on one another, and again in diminished number, were enclosed in the loose enveloping membrane, the sarcolemma."

The author, in an admirable figure, copied from a drawing of his own, gives a representation of the successive stages of the destruction of the muscular fibres contained in their loose sarcolemma; in a note he suggests that

"The long membranous filaments which Schrant found in a medullary tumour under the glutæus muscle, and which were more numerous in the vicinity of the muscle, and even exhibited transverse striæ, from which the tube acquired a surprising resemblance to sarcolemma,* were not new formations, but similarly degenerated muscular fibres, where, after the disappearance of the fibres, the sarcolemma alone remained as a thin membrane. Perhaps also some of the tubular fibres and pipe-like structures, which Rokitansky describes as new forms, are of a like nature."

In the text he remarks, that from the description he has given in explanation of the plate,

"It appears that, by transudation, or imbibition, the morbid parenchymatous fluid is taken up by the sarcolemma, and now the same cell-formation sets in within the sheath as had previously taken place in the surrounding areolar tissue. The parenchymatous fluid must thus, by interchange with the contents of the cancer-cells, undergo such a change, that it everywhere acquires the tendency to produce similar nuclei and cells. When these cells are once formed within this thin sheath, and are in contact with the primitive fibre, normal nutrition can no longer proceed; the muscular fibre becomes altered, and divides, under the form of longitudinally extended cells, into several parts, not unlike the fibres of invo-

lloma, Leipzig, 1852, p. 21.) Bennett, on the contrary, says that epithelial-cells, especially new ones, may exhibit all the characteristics of true cells. (On Cancerous and Canceroid Growths, p. 149.) It is, however, another question whether this parenchymatous fluid of epithelioma, when taken up by the lymphatics, and perhaps by bloodvessels, may not be so much altered as to give rise in other situations—for example, in glands—to the development of true cancer-cells, although, in other cases, epithelial-cells form in the glands. It seems to me that the occurrence of true cancer after epithelioma of the lips can scarcely be otherwise explained; Schrant, too, thinks that the epithelial form may produce true cancer. (See Schrant, Prysverhandeling over de goed- en kwaadaardige gezwellen, blz. 396, 349, *et seq.*)

* Op. cit., p. 352, *et seq.*

luntary muscles, and finally becomes entirely broken up and replaced by a constantly increasing quantity of cells. The sarcolemma appears for a very long time to resist destruction. In some places I could still plainly observe it where the contents already appeared to consist solely of cells, and no longer exhibited a fibrous structure.

"Not less was the effect of these cancer-cells upon the nerves. This I particularly had an opportunity of observing in the carcinomatous tumour of the leg of the old woman, the state of whose muscular fibres I have already described, where the fibular nerve was greatly affected, and surrounded with medullary fungus. The microscopic condition of the nervous bundles and fibres was very variable. In the parts most affected no primitive fibrillæ could be perceived, and the nerve appeared, from atrophy of the tube, to consist merely of connecting tissue; in other places the change could easily be observed."

Some fibrillæ were very broad, especially in particular parts, and were almost entirely filled with cells and fat globules; others were much atrophied, and in some portions devoid of cells; while others, filled and distended at one part with cells, suddenly collapsed at another. In the connecting tissue situated between the fibrillæ could be seen portions of atrophied nervous fibrillæ, the remainder of which appeared to have been changed into fibres or connecting tissue. The fibrous tissue between the primitive fibrillæ itself contained few or no cells, and probably consisted for the most part of atrophied nervous fibrillæ; the connecting tissue, however, which surrounded the nervous bundle externally, was completely studded with cancer-cells similar to those which occupied the fibrillæ themselves. From these circumstances, the author argues, that the tissue situated between the fibrillæ consisted rather of atrophied fibrillæ than of original areolar tissue, an hypothesis which would also explain the small number of fibrillæ found in a nervous bundle of such thickness. Several bundles moreover appeared to consist of thin filaments, as connecting tissue, without containing a trace of nervous fibre. Bennett, too, has noticed this disappearance of the nervous fibrillæ, and has represented them as being changed into fibres, with numerous fat globules and granular matter.

"It cannot, however," observes the author, in a note, "be ascertained with certainty whether the cells found in the nervous tube are true cancer-cells, since these tumefied nerves entirely agree with the change we meet with in a divided nerve. Since, however, the cancer, and not any mechanical pressure on the nerve, which did not exist, was, in this instance, incontestably the cause of the nervous degeneration, and at the same time similar cells had formed, as we have seen, within the sarcolemma of the muscular fibres, it appears to me extremely probable that true cancer-cells had been developed in the nervous tube, which had caused the atrophy, which latter can scarcely be explained in any other way."

The author has observed a similar extension of cancer-cells in the internal parts of the body. Thus, among other instances, he was able, in a case of cancer of the pyloric extremity of the stomach, plainly to follow the progress and diffusion of the cancer-cells: in the apparently still healthy portions of the muscular fibres of the stomach he found several small cells and nuclei interspersed. He also witnessed a like formation and propagation of cancer-cells external to the muscular coat in the areolar tissue under the peritoneum.

This formation of cancer-cells may even penetrate the walls of the bloodvessels, especially of the veins: thus, in two cases of medullary

fungus of the stomach and liver, the author found a portion of the fungus in the cavity of the adjoining vena portæ.* He could observe how, from the increased development of the cells in the wall of the vein, they had pressed into that vessel, where they were found smaller, or in other words, more recent. It is easy to understand how, under these circumstances, an infection of the blood and general spread of the disease must rapidly take place.

“It appears, however,” observes the author, “that the vena portæ is peculiarly disposed to this fungous growth; I have not as yet met with it in other veins, while complete filling up of the vena portæ in the liver, even to the smaller branches, with a vast fungous mass, is not so very rare, as I can prove from many remarkable examples.

“From all that has been hitherto adduced, it is evident that when once epithelial (epithelioma) or any other species of cancer has formed, and has arrived at a certain degree of development or maturity, the extension of the disease occurs by the constant new formation of cells in the parenchymatous fluid which penetrates the adjacent parts, granulations first taking place, from which nuclei become developed, and subsequently appear to be circumscribed with cells. Sometimes the number of nuclei in an already formed cell increases; these nuclei proceed to cells, and thus a parent cell arises, which is finally resolved, setting free the enclosed cells. This latter mode appears, however, to occur only in [true] cancer and medullary fungus, and not in epithelioma. We therefore assume that the parenchymatous fluid acquires the property to form similar cancer-cells, from the interchange of constituents which must necessarily take place between the cancer-cells already existing and this fluid; while the latter, gradually reaching the surrounding parts, becomes the cause of the local extension of the disease. The adjacent parts frequently pass into cancer, but cancer-cells become developed between and in them, whereby the earlier tissues are dissolved and disappear.

“It has been supposed that the destruction or disappearance of the surrounding parts is principally owing to the pressure of the enlarging cancerous tumour. This, however, would seem not to be so exclusively the case; the morbidly altered parenchymatous fluid penetrates the adjacent textures, so that not only does a multitudinous new formation of cells take place in the areolar tissue between the other organic constituents, but the latter taking up this fluid, a similar cell-formation ensues in the tissue of the parts themselves. Thus, we saw cancer-cells form within the structureless membrane or sarcolemma covering the muscular fibres, and in like manner they seem to be developed in the cavity of the nervous tube. The formation of a quantity of fat at the same time accompanies this change, by which the healthy constituents seem to be gradually broken up; the parts are therefore destroyed, not so much in consequence of pressure, as of impeded nutrition, due to the morbid condition of the parenchymatous fluid, the development of fat, and the solution of parts which everywhere attends the latter. This diffusion of morbid parenchymatous fluid does not, however, take place solely in the immediate neighbourhood of the cancerous tumour, but the same fluid is taken up by the lymphatics, and so conveyed to the nearest glands. This is particularly proved by the examples I have quoted, where, in epithelioma of the lips, or also of the tongue, the neighbouring lymphatic glands become swollen and filled with epithelial cells. Thus, too, I have often, in medullary fungus of the liver or stomach, seen the absorbents as thick white cords pass through the diaphragm and along the sternum, so that I could follow them into the thoracic duct, which was also swollen,

* In a note, the author calls attention to the remarkable fact, described by him so far back as 1828, that after the injection of fibrous tumour and carcinoma, only fine capillary arteries can be seen, and that no veins appear to exist in connexion with those of the general system. It would seem, he observes, that the blood in carcinoma returns to the arteries, that a new capillary net exists between the vessels of this class, such as we find between the ramifications of the vena portæ and the venous system.

while their contents quite agreed with the cells of the medullary fungus; and in this manner the morbid parenchymatous fluid was, through the medium of the lymphatics, thrown into the blood, and so conveyed through the whole body. But it is extremely probable that this absorption does not take place exclusively by means of the lymphatics, but likewise through the capillaries and veins, which are washed by the parenchymatous fluid, between which, as is well known, such an interchange of action exists.

“When this process has worked in a sufficient degree, and has spread through the body, we have the secondary formation of cancer-cells, and, as is well known, the development of cancer in other places.

“Of this, a case of cancer of the tongue, by which the entire of the left half of the organ was destroyed, appeared to me to be a remarkable example. The patient died of a sudden hamorrhage, in consequence of the ramine artery giving way during the act of eating, when the copious stream of blood rushing into the mouth, and backwards into the trachea, produced suffocation. An accurate examination of this specimen, which is still in my collection, showed that not only was the further course of this vessel destroyed and changed into a black cord, but that the lingual nerve, which was very much thickened at its entrance into the cancer, passed into a black slimy band, which, during the life of the patient, I had seen lying in the mouth. Examining the nerves on the right, or still apparently sound side, I found in some degree in the lingual nerve, but especially in the hypo-glossal, little inequalities in thickness, as if tubercles had been developed within the nerve. Having made a longitudinal division of the last-named nerve, I took a very thin section from its centre, and observed numerous cancer-cells grouped together, surrounded apparently with areolar tissue, which, however, I suspected to consist in great part of atrophied nervous fibrillæ.

“This formation of cancer-cells in the centre of the hypo-glossal nerve of the healthy side, can scarcely be explained by assuming direct imbibition of the morbid parenchymatous fluid. I think we must look upon it occurring in so distant a nerve as a secondary effect, and refer it to a general infection of the fluids. It appears that little liable as the nerves otherwise are to be affected by inflammation, and even suppuration of the neighbouring parts, this is not the case with cancer, which very easily produces disturbances in them. Of this I have several striking examples in my collection; in one case, the entire sciatic nerve is destroyed at its exit from the sacrum, so completely as to sever it from its inferior portion.

“It appears to me that the burning, stinging pains which exhibit themselves in the course of cancer, are the result of cancer-cells beginning to locate themselves in, and to destroy, the neighbouring nerves; so that these very pains, in my opinion, afford a proof that the formation of cancer is no longer a local disease, but that it has already commenced to spread in the adjacent parts, and that, consequently, the time for operating, with a reasonable hope of a favourable result, has probably already passed away. It is, indeed, to be lamented that the resolution to remove a cancer is so often taken when the first period, in which the operation might be performed with a well-founded expectation of a more permanent result, has already elapsed. This is in some measure the fault of the patient, who conceals the disease so long; but the surgeon is also partly to blame, who, dreading an unfavourable impression on the mind of the sufferer, does not think himself justified in proposing operation, until he is satisfied that the disease is cancer; but at such a period a lasting cure is, in many instances, scarcely to be expected. There have been so many unfortunate cases in which, in consequence of the postponement of an early operation, a fatal relapse has subsequently occurred, that I think I cannot sufficiently insist on the removal of every swelling or hardening, from which cancer might afterwards become developed, even though this should be very uncertain. The operation is then of little importance, and even if the tumour had been a benign one, is not thereby rendered more injurious.

“If we trace the formation of scirrhus and cancer, this becomes, in my opinion, still more evident. The scirrhus in the beginning forms as a hard tubercle. If

this be examined under the microscope, we find nuclei and very small cells enveloped in and surrounded by fibres."

As an example, the author gives a drawing of a scirrhus taken from the uterus, in a case in which fully-formed cancer existed in the breast, so that scarcely any doubt could arise as to the true nature of the scirrhus. Here are seen several groups of little cells and nuclei, surrounded and enclosed in a number of fibres, which latter make up the chief bulk of the scirrhus, and among which some few elastic fibres are visible. Through the intervention of fibres, the cells are beginning to be divided into secondary groups. The state of the breast is represented in another figure; the cells are here much larger, and the tendency to form secondary groups is more plainly seen. By simultaneous increase of the cells the whole fungus enlarges, while the quantity of connecting tissue, which appears not to be so rapidly reproduced, or is perhaps removed, is proportionally much less.

"It is known that the growth of scirrhus is very slow; no great interchange of matter is, therefore, as yet taking place; by the slow increase of the cells and consequent enlargement of the groups, the surrounding tissue becomes tense: hence the hardness of the tumour. No cells or nuclei as yet exist in the adjacent tissue external to these enveloping fibres; the disease may therefore be considered as a local one, which may still be advantageously removed.

"In cancer which has already formed, the development of cells is much greater, the cells are larger, they are not separated from the neighbouring parts by such thick layers of connecting tissue and fibres; the entire tumour is less hard—that is, more parenchymatous fluid is present, and this keeps up a stronger interchange of material with the existing cells, which are reproduced with proportionally greater rapidity. Finally, if the disease is luxuriant, it is soft, often receives the name of medullary fungus, and now consists almost entirely of cells, sometimes without a trace of fibres.

"From this well-known progress and development of cancer, which I have described merely for the sake of greater clearness, it is evident that it is only in the first period of scirrhus we can reasonably suppose that, from the still sparing interchange of material, the contents of the little cells shall not have been so freely transferred to the scanty parenchymatous fluid of the tumour as to have pervaded the adjacent parts. When, however, the growth of the tumour increases, this in itself is a proof of greater activity, of an increased interchange of constituents, and of the diffusion of the infected parenchymatous fluid. When, in addition, burning, shooting pains set in, we have, I am convinced, an absolute proof that the parenchymatous fluid has penetrated the nearest nerves, and produced in them a new cell-formation, with destruction of the nervous tube. In cancer itself, it is true, as a new formation, no nerves exist; the tumour itself is insensible, so that the introduction of a stilet, if the surrounding parts be avoided, is entirely unfelt by the patient.* But if the cancer be removed, it is not only most important to take away at the same time as much of the apparently sound parts surrounding the tumour as can be done without too great injury, but I reckon it most essentially necessary, after the operation is over, to examine as accurately as possible under the microscope the edges of the part removed, in order to ascertain whether granular matter, nuclei, or cells, exist in any part of the tissue. Should this be found to be the case, we must conclude that the disease has not been wholly and completely removed, and the wound being still open, a

* Schrant makes a similar observation, but adds, "Sometimes, however, by the increase of the tumour, nerves are included. I possess a portion of the medullary fungus of a breast penetrated by a nerve, but which, on its entrance, has become thin and transparent, and appears to have lost its contents or medullary portion."

further portion should be cut off in the situation where the cancer-cells and nuclei were seen ; or perhaps, what is more painful and less certain, we should endeavour to destroy the part with caustics, in order to prevent relapses, which always lead to a fatal result."

In a note to the foregoing, the author states that subsequently to making his communication to the Provincial Society of Utrecht, in 1847, 'On the Extension of Cancer-cells by Means of the Parenchymatous Fluid,' he saw that Dr. Bennett, in his excellent work 'On Cancerous and Canceroid Growths,' had come to the same conclusion ; that he had found cancer-cells in the muscles situated in the neighbourhood of carcinoma, and had thence predicted the return of the disease ; and that he also insisted on the necessity and possibility of instituting, at the time of operation, a microscopic examination of every suspected tissue before the edges of the wound are closed. He takes the same opportunity to recommend the application, to open and fungous cancer, of pledgets moistened with a very strong solution of iodide of potassium, which he has found to produce a gradual diminution and apparent solution of the tumour, as well as a cessation of the disagreeable smell and hæmorrhages, while the remedy seemed to have no injurious effect on the adjacent parts. In a case of cancer of the tongue, in which this organ was so much swollen as to render eating extremely difficult, he caused the patient to keep continually in his mouth a solution of from half a drachm to a drachm of the salt in an ounce of water ; the result was that the fungus became softer, and separated in a few days, the tongue returning to its normal thickness. The application did not, however, prevent relapses, and the patient subsequently died of cancer in the neck : but certainly this simple means, which caused him neither pain nor inconvenience, had prolonged his life. If the fungus be sprinkled with the salt in the form of powder, it dwindles more quickly, and passes into mortification, without injury to the other parts.

From all that he has brought forward the author draws the following conclusions :

1. Through an interchange of material, taking place between cancer-cells and intercellular fluid, the latter acquires the property of forming new nuclei and cells of a similar nature.

2. This intercellular fluid passes, along with the parenchymatous fluid pervading the sound parts, into the textures adjoining the tumour. The parenchymatous fluid thus acquires the same constituents and tendency to form similar cells, which now become developed among the healthy surrounding tissue, in the course of the areolar membrane.

3. On account of the minuteness and small number of the last-mentioned cells, their presence cannot be detected with the naked eye ; so that the surrounding parts may appear to be perfectly sound, notwithstanding that they contain the germs of the advancing formation of cancer.

4. It is, therefore, of importance, in removing cancer by operation, not only to take away at the same time a large quantity of the adjacent sound parts, but also to examine the innermost sectional edges under the microscope, in order to ascertain whether any trace of cancer-cells in process of formation is to be discovered in them.

5. The existence of burning, shooting pains in carcinoma, may be

taken as a proof that the cancer-cells have reached the neighbouring nerves, and the disease can then scarcely be looked upon as a local one, in which an operation might be permanently successful.

6. By the absorption of the infected parenchymatous fluid through the lymphatics and veins, the whole body seems to become more or less tainted, so that secondary cancer ensues in distant situations, when, as is self-evident, operation can no longer be thought of.

7. This altered parenchymatous fluid penetrates the organic tissues which are washed by it, the sarcolemma of the muscular fibres, the tubes of the nerves, &c. These membranes, too, both the sarcolemma and the walls of the nervous tubes, appear to take up the altered nutritive fluid; the consequence of which is, that both within the sarcolemma and the nervous tubes, similar nuclei and cells arise, accompanied with an absorption of the muscular fibre and of the contents of the nerve, and attended with the deposition of fat, by which these parts waste and are destroyed, while the surrounding membranes (sarcolemma and walls of the nervous tubes) remain.

REVIEW X.

The Medico-Chirurgical Transactions. Second Series. Vol. XIX.
London, 1854. 8vo, pp. 264.

THE present volume of the 'Medico-Chirurgical' is rather a thin one. Its contents, however, are, for the most part, of good quality, and do not detract from the reputation the 'Transactions' have so justly earned. We proceed to give a brief account of those papers which require notice.

I. *Scrofulous Caries of the Left Astragalus. Excision: Cure: with Formation of a Fresh Joint.* By S. F. Statham, Esq.

A record of a successful case, with nothing about it to call for particular remark.

II. *Pathological Remarks on the Kind of Palpebral Tumour, usually called in England, Tarsal Tumour.* By H. Haynes Walton, Esq., F.R.C.S.

The tumour in question is described by Mr. Walton as usually solitary, immovable, hard, spherical, and well-defined; as varying in size from that of a grain of small shot to that of a pea; as limited in position to the seat of the Meibomian glands. From examinations, by Dr. Druitt, of a specimen recently removed, and by Mr. Walton of two specimens in the College of Surgeons, it appears these tumours originate in a Meibomian gland, in which sebaceous matter and epithelium have collected, and round the orifice of which, fibro-plastic matter has been effused. Mr. Walton suggests the name of Meibomian tumour, and states, that since he has adopted the above pathological view, he has ceased to attempt its removal from the interior of the eye, but now divides the lid and cyst on the outside, squeezes out the contents, and, if possible, extracts the cyst with a pair of forceps.

III. *Notice of a Case of Skin Disease, accompanied with partial Hypertrophy of the Mammary Gland.* By James Alderson, M.D., F.R.S.

A young lady, aged 20, presented on the upper part of the left breast the following remarkable appearance. To the extent of about four inches in length, by one and a quarter in width, the skin assumed the appearance of a perfectly smooth, polished surface, of an opaque, yellowish-white colour, like polished vellum or ivory. Round the margin of this parchment-like surface was a vascular zone; but on the surface itself were no vessels and no desquamating epithelium. Below it, some of the glandular structure was enlarged, and there was a small gland in the axilla.

Various opinions were expressed as to the nature of this affection. Mr. Hodgson considered it to be allied to carcinoma; Sir Benjamin Brodie thought it might be dry gangrene; and Mr. Ure connected its appearance with deficient catamenia. Being treated with mild frictions and liquor potassæ, the tumour disappeared, without any desquamation of cuticle or destruction of the cutis. Other tumours of the same kind appeared, however, on the arm and thigh; but the progress of these is not recorded. The general health remained perfect.

IV. *Case of Mollities Ossium, preceded by Degeneration of the Muscles.* By T. K. Chambers, M.D.

A very interesting case of mollities in an unmarried woman, 26 years old, in whom, before death, spontaneous fracture occurred in many bones. The urine did not contain any of the peculiar albuminoid substances discovered by Dr. Bence Jones in a somewhat similar case. It was analysed by Dr. Beale; but as the composition in 1000 parts alone is given, no very certain inferences can be drawn from it. It appears, however, that the urea was relatively small, and the extractives large in amount; and that the earthy phosphates and the fixed alkaline salts were relatively very greatly increased.

After death, a piece of tibia examined under the microscope, was found to consist (with the exception of a thin external layer) of large fat vesicles, with various-sized dull-red spherules between them. The external layer contained small islands of opaque bone, with indistinct corpuscles; round the islands was a fibrous structure, in which were oil globules of various sizes.

A portion of rectus muscle was found totally degenerated, and consisted of little else than fat vesicles.

V. *On the Keloid of Alibert, and on True Keloid.* By Thomas Addison, M.D.

The object of this communication is to show that the keloid of Alibert is not the disease to which the term keloid should be applied, but that there is another and totally different affection to which this term is more applicable, and which, therefore, is to be termed the "true keloid," in contradistinction to the keloid of Alibert. There appears to us something strange in thus wresting away the original term, invented by Alibert, from the disease to which he undoubtedly assigned it, and applying it to another affection, of which he had no knowledge whatever. It would

have been better, we think, to have proposed another term for the distinct and novel affection now first fully described. Having said thus much against Dr. Addison's terms, we have only to add, that we have not one word to say against his descriptions; they are clear, precise, and so concise that we will not run the risk of injuring them by attempting condensation, but will refer our readers to the original.

VI. *On the Blood and Effused Fluids in Gout, Rheumatism, and Bright's Disease.* By A. B. Garrod, M.D.

The object of the author is to indicate a very easy mode of detecting uric acid in serum; viz., by placing a little serum in a watch-glass, at the bottom of which lies a fine thread, and adding acetic acid. The uric acid deposits on the thread, and is easily recognised under the microscope by the form of its crystals. The test does not indicate the presence of the acid unless this amounts to 0.025 grains in 1000 grains of serum; and as such a quantity is always abnormal, the appearance of the crystals is conclusive as to the existence of uric acid in morbid amount. In order to use this test, the serum must be fresh, for Dr. Garrod finds that the uric acid soon decomposes; and he believes there is little doubt that oxalic acid is one of the products.

The author then reports an examination of the sweat of a gouty patient, in which uric acid was vainly sought for. He then notices that he has found uric acid in pericardial and peritoneal effusions, in cases in which the blood contains an abnormal amount of this substance. Finally, he records the interesting fact, that in the fluid of a blister applied to a gouty subject, uric acid is detected by this thread experiment.

VII. *On Excision of the Knee-joint.* By G. M. Jones, Esq., M.R.C.S.E.

The writer gives a table of all the cases of excision of the knee-joint hitherto performed, 33 in number, of which 6 were by himself. Five of these operations were performed by Mr. Jones, by two lateral incisions, and a connecting transverse one carried over the centre of the patella. In the last case a longitudinal incision, four inches in length, was made on each side of the knee-joint, midway between the vasti and the flexors of the leg; the two cuts were connected by a transverse one just below the insertion of the ligamentum patellæ, the flap was reflected up, the ligamentum patellæ and patella were pulled aside by a spatula, the leg was forcibly flexed, the crucial ligaments divided, and the articulating surfaces thus exposed. Mr. Jones believes that this operation is not more dangerous to life than amputation, and the superiority of the natural over a wooden leg will scarcely be contested.

VIII. *On the Radical Cure of Reducible Femoral Hernia by a New Operation.* By T. Spencer Wells, F.R.C.S.

The new operation was devised by Professor Wutzer of Bonn. It has been performed twice by Mr. Wells, and 58 cases are referred to, in many of which it has been successful, and in only one of which did death ensue. The principle is to pass up a cylinder into the inguinal canal, pushing up,

of course, the skin before it; the instrument is fixed by a needle passed through the skin, and then pressure is exerted on the cylinder and on the internal ring on which it rests. The patient is kept quiet, to prevent excess of inflammation; the cylinder is kept applied for six, seven, or eight days, and is then removed, and the cavity is filled with lint. We refer to this interesting paper for the full details of the method.

IX. *Observations on Morbid Changes in the Mucous Membrane of the Stomach.* By Dr. Handfield Jones.

Tables are given of 100 cases of various diseases, in which the mucous membrane of the stomach was microscopically examined:

"The following deviations from the typically healthy condition are mentioned:

"1. *Nuclear masses*:—These, as I have stated, are the solitary glands, and it is doubtful what degree of their development is to be considered as surpassing the physiological limit. It seems probable, both from actual observation, and from the behaviour of the same structures in the intestines, that they may become hypertrophied, and encroach abnormally upon the proper secreting tissue. Again, it is certain that they may undergo atrophy, and thus occasion loss of substance and thinning of the mucous membrane in the spots they occupy. . . .

"2. *Diffused nuclear formation*, in extreme instances, extend uniformly throughout the mucous membrane. The nuclei are mingled with more or less granular matter, and the tubes are more or less atrophied and obscured by the interstitial deposit.

"3. *Inter-tubular fibroid formation*.—This is very commonly associated with the preceding, and consists simply in this, that the exudation in which the nuclei lie, passes into the form of a more or less fibroid or homogeno-fibroid stroma. In this, elongated or fibre-forming nuclei may sometimes be seen. The material is very similar to that which thickens the Glissonian sheaths in some cases of cirrhosis. In some cases a change takes place in the tubes themselves, so that they become converted into nucleated substance, similar to that which surrounds them. Their epithelial contents are changed into a granular mass, containing many more nuclei than in the healthy state, while the homogeneous wall of the tube wastes and disappears, and so the intra-tubular nucleated mass blends with the extra-tubular, and the whole mucous membrane is converted into an uniform material loaded with nuclei. In extreme cases the tubes are utterly atrophied, and the whole thickness of the mucous membrane is occupied by fibroid or granular stuff, in which some altered remnants of the tubes may be brought into view by means of acetic acid. The basement membrane of the surface is often absent in parts where there is much inter-tubular formation, and the nucleated fibroid tissue is then exposed. It may, however, have been covered in by the columnar epithelium during life.

"4. The tubes appear, in some instances, to decay spontaneously, or, at least, not from the atrophic pressure of new-formed fibroid tissue; the mucous membrane may then present a mere mass of granular and celloid *débris*, with interspersed fat vesicles and fatty matter.

"5. *Black pigment* may be deposited in the mucous tissue, sometimes in great quantity; it is occasionally within the tubes, more often between them. . . .

"6. *Cystic formation* is occasionally met with. . . .

"7. *Mammillation* is often seen in lesser degrees, and, not unfrequently, well marked. It affects especially the pyloric third or half of the stomach. To obtain a good view of it, or indeed not to overlook it, it may be absolutely necessary to wipe off a thickish layer of tenacious adhering mucus. It seems to be of two kinds, or to be produced in two ways. One may be called healthy, and appears to depend on some unusual contraction of the corium of the mucous membrane. . . .

The other form of mammillation is morbid, and seems to be essentially connected with fissuring of the mucous membrane, or local atrophy.

"8. *Gathering up of the lower parts of the tubes in the pyloric region so as to form a group of convolutions, something like the acini of a conglomerate gland, is often observed. It is not quite clear how the change is produced.*

"9. *There is much difficulty in determining exactly what conditions of the epithelium of the tubes are unhealthy. Their contents are often of a very opaque fatty aspect, especially in their lower half; but this scarcely seems to be abnormal. In a few instances I have observed an apparently true fatty degeneration of the epithelium, the nuclei and cells being converted into shrunken fatty masses. Not unfrequently the epithelium appears more or less stunted and atrophied, or of a less soft, finely mottled aspect, and its cells look withered and shrunk.*

"10. *Self digestion, in slighter degrees, is of very common occurrence, and is invariably confined to or most marked in the splenic region.*

"11. *Small, dark red, circumscribed spots, seen on the surface of the mucous membrane, are manifestly the result of hæmorrhage, or at least of the exudation of hæmatin. The microscope shows in these parts an abundance of dark pigment granules.*" (pp. 92—97.)

In the 100 cases, 28 only were quite healthy. In 47, the splenic and mid regions were healthy, while the pyloric was affected. In 11 cases there was a moderate, and in 14 cases a great, amount of destruction of tubes. The male sex was apparently more liable than the female to this organic disease. Seven cases of ulceration are referred to, all in persons over 48 years of age, the average age of the whole being 59 years.

The destruction of the tubes is not a very marked sequence of drinking: in 11 immoderate drinkers, 1 stomach was healthy, 6 were tolerably so, 1 had moderate and 3 great destruction of the tubes.

It would appear that considerable wasting of the tubes may occur without any marked symptoms.

We are happy to observe that Dr. Handfield Jones is about to give further instruction on this most interesting subject, and we defer all comment for the present.

X. *A Case of Fatal Asphyxia, caused by the Detachment of a Diseased Bronchial Gland impacted in the Larynx.* By George Edwardes, F.R.C.S.

This is a very remarkable case of a bronchial gland ulcerating through the walls, and entering into the cavity of the trachea, and then being carried up to the larynx by violent exertion, causing death by suffocation. There had been no previous reason to suspect any disease whatever: there had been no cough, hoarseness, or dyspnoea.

XI. *Remarks on a Peculiar Form of Tumour of the Skin, denominated Pachydermatocele.* By Valentine Mott, M.D.

The form thus described consists in hypertrophy of the skin and of the subcutaneous tissue, which, commencing in a congenital brown spot, or mole, gradually increased in size, although in some cases the bulk was very considerable, and necessitated operation. Five cases altogether are referred to. The description given by Dr. Mott does not add anything to our previous knowledge of the subject.

XII. *Case of Distortion of the Spine.* By Thomas Hodgkin, M.D., and William Adams, F.R.C.S.

It is apparently intended that our recollection of Dr. Mantell shall not merely be that of the ardent and genial labourer in the cause of science, but also that of the unlucky individual who was himself a scientific curiosity, and carried about with him a mysterious disease which the inquisitive scalpel of his friends laid bare after death. The dry record of the spine disease narrated by Dr. Hodgkin and Mr. Adams, gains nothing from our being informed that Gideon Mantell was the subject of it, and we cannot perceive the necessity of using the name of the patient to excite our curiosity about his disease.

The condition of the spine was that of transverse rotation of the bodies of some of the lumbar vertebræ, so as to cause passage of the spinous processes to one side, and unusual prominence of the transverse processes on the other side. We refer to the paper for the full details and remarks on the affection, which are extremely instructive.

XIII. *On Gout and Rheumatism. The Differential Diagnosis, and the Nature of the so-called Rheumatic Gout.* By A. B. Garrod, M.D.

The chief points of importance in this most valuable paper are as follows :

1. In 47 instances, the serum of the blood was found to contain an abnormal amount of uric acid. No less than 45 of these persons were males. Their average age was 47 years. All these patients suffered from articular disease of that kind which is termed "Gout," by writers. In 26 of these cases, inquiry was made into the diseases of the nearest blood-relations, and in no less than 13 some close blood-relation was found to be similarly affected. In 28 cases the habits were inquired into, and no less than 21 were found to be free livers. The occupation was noted in 33 cases, and it is very remarkable that 8, or nearly 25 per cent., were workers with lead in some form or other.

2. In 35 instances the serum of the blood was found to contain no uric acid. All these patients suffered, like the former, from an articular disease, which was so well marked as to necessitate at once the diagnosis of rheumatism, as defined by writers. In almost all other particulars these patients differed from those above referred to; and the following table, given by the author, shows at a glance the peculiarities of each class :

CLASS I. *Articular Affection with Uric Acid Blood.*

The average age of patients was	47 years.
The males formed	about 95 per cent.
Hereditary predisposition was traced	in 50·0 „
Free living and drinking had existed	75·0 „
Painters or plumbers formed	24·3 „
Drink acted as the exciting cause	in 39·5 „
The great toe had been specially affected	82·9 „
No great toe affection	5·7 „
Doubtful	11·4 „
Œdema noticed	68·5 „
Deposits of urate of soda	45·9 „
Acute cardiac affection	none.

CLASS II. *Articular Affection (Non-urethral) with no Uric Acid in the Blood.*

The average age	was 40 years.
The males formed	but 40·0 per cent.
Hereditary affection was traced	in 33·0 „
Cold acted as an exciting cause*	88·8 „
Œdema noticed	12·9 „
Acute cardiac affection	41·9 „
Deposits of urates of soda	none.
Great toe <i>especially</i> affected in	none.

Dr. Garrod therefore draws the conclusion, that to the differences already described between gout and rheumatism, another must be added—viz., the presence of uric acid in the blood in the first, and its absence in the second disease. He believes, moreover, that this character may be used as a diagnostic mark in those cases which cannot be referred by other symptoms to their proper heading.

3. In 6 cases of articular affection (discharge), clearly connected with urethral disease, in 4 cases no uric acid was found.

4. In 61 other patients, with various diseases, but without articular affections, the blood was examined for uric acid. It was present in 13—viz., in 5 persons with albuminuria, in 1 with cholera, in 1 with ophthalmia, in 1 with bronchitis, in 1 with pneumonia, and in 4 with a disease not named.

In a postscript, 14 additional cases are referred to; in 4 there was an abnormal amount of uric acid and coincident gouty symptoms, in 5 there was no uric acid and coincident rheumatic symptoms, in 6 there was no uric acid and no articular affection.

We can have no difficulty, it appears to us, in accepting the author's conclusions as to the presence and absence of uric acid in the blood of gouty and rheumatic patients. Evidently, however, the uric acid, *per se*, is not the cause of the articular affection in gout, as it is stated to have been present in variable, but, in some cases, in considerable, quantity in 13 cases in which the joints were untouched. Much remains yet to be done before the genesis of gout can be explained; and we shall hope to receive from Dr. Garrod a continuation of the important researches he has commenced. We defer all comment on Dr. Garrod's opinions on the impropriety of the usual use of the term "rheumatic gout," to another opportunity.

XIV. *Case of Traumatic Aneurism of the Ophthalmic Artery.* By T. B. Curling, F.R.S.XV. *English Statistics of Hooping-Cough.* By Edward Smith, M.D., LL.B. (London).

The information contained in this paper is derived from the returns of the Registrar-General; and is, therefore, based only on fatal cases.

In the London district only 6 diseases produce a greater mortality than hooping-cough—viz., phthisis, pneumonia, bronchitis, typhus, convulsions, and scarlatina, in the order here given. In ten years, out of 553,694 deaths, phthisis killed 68,204, and hooping-cough 18,666 persons. The

* And alcoholic fluid did not appear to be either a predisposing or exciting cause.

mortality of hooping-cough was to all diseases as 1 to 29·6. More than $\frac{2}{5}$ ths of the whole number of deaths were in children under 1 year old; $\frac{3}{4}$ ths of the children were under 2 years; and $\frac{1}{2}$ ths were under 5 years of age.

Hooping-cough is much more fatal in female than in male children, and the relative mortality increases with age.

“Thus, whilst under one year of age, the excess in the ratio of mortality amongst females is one-sixth, it is less than one-third in the fifth year of existence, and was reduced to one-fourth in the second year, and one-fifth in the succeeding intervals. It is unsatisfactory to pursue the comparison at later periods of life, on account of the smallness of the numbers to be contrasted, but so far as this is of value, it proves that this preponderance is maintained, and even increased at puberty, and for an indefinite period beyond that era.” (pp. 239-40.)

The mortality and the temperature of the air are in an inverse ratio to each other, or nearly so. The following sentence gives the pith of an interesting inquiry which is made into this point:

“The mortality of hooping-cough attends diminished temperature with considerable precision, and so far may have a point of correspondence with other seasonal affections; but there is one point in which it differs from others—viz., that it is not increased in intensity by any intensity of the opposite season, or that of summer. Excessively high temperature, so far from having given rise to increase of mortality, was directly the reverse.” (pp. 245-46.)

The author enters incidentally into the relative mortality of hooping-cough and some other diseases, and gives a diagram, showing the mortality of bronchitis, of hooping-cough, and the temperature, which is exceedingly interesting. It clearly appears that the mortality of these diseases bears a very close relation, and depends to a very great extent on temperature, being greatest when this is least.

We strongly recommend this paper as an excellent example of statistical inquiry.

Having now completed the notice of this volume of the ‘Transactions,’ we shall only add, that though small in bulk, its papers are, in many cases, of high merit. It is much better for the Society who print, and for the public who read books, that there should be careful winnowing, otherwise the Society loses caste, and the public gains only a mass of unreadable and useless dissertations.

REVIEW XI.

On Pain after Food: its Causes and Treatment. By EDWARD BALLARD, M.D. Lond.—London, 1854. 8vo, pp. 136.

DR. BALLARD has done himself injustice in the selection he has made of the title for this work; it reads somewhat *ad captandum*, and does not, without due reflection, convey the full extent of its comprehensiveness. The author has, in truth, composed a treatise upon indigestion. Pain after food is but one among many of the symptoms and effects thereof, which he treats of. The arrangement of the work is novel. We have first an “Introduction,” which discusses the physiology of digestion so

far as the stomach is concerned. We next meet with "The Symptom," pain after food, and its accompaniments. The seat, character, intensity, and variety of the pain; its dependence upon the quantity and quality of food, are all considered at length by the author; together with its attendants, eructation, vomiting, thirst, feverishness, &c. The author, in the third place, gives a tabulated arrangement of the causes and treatment of pain after food, with numerical references to the paragraphs which precede and follow it. In this table consists the novelty and ingenuity of the work. As reference is made in every section of it to other parts of the work, so that it is a kind of directory index to the whole, the "Commentary" which follows necessarily recapitulates the matter of the previous chapters, "Introductory" and on "The Symptom."

The treatment, dietetic and medical, laid down by the author, is in all cases scientific and judicious.

A few quotations will show the nature of the work and its value. With respect to the seat of pain after food, Dr. Ballard informs us that—

"The most frequent *seat of the pain* is the epigastrium or lower half of the sternum and neighbourhood of the ensiform cartilage. It is thus referred in considerably more than half the cases which are met with in practice: sometimes the seat of pain is so circumscribed that it may be covered with the point of the finger. The next most frequent seats of pain are the region of the umbilicus, and the entire upper portion of the abdomen, stretching from one hypochondrium across the epigastrium to the other. In some, it is referred to the lower part of the interscapular region, and in others to the situation of the heart or to a spot below the left mamma, and near the situation of beat of the apex of the heart; in a few, to the lower region of the abdomen or hypogastrium." (p. 11.)

The character of the pain is variously described, as—

"'Weight,' 'oppression,' 'tightness,' 'fulness,' or 'tension,' and all these terms, imply a *dull* kind of pain and uneasiness. The patient sometimes uses some special simile to illustrate his meaning, and the pain is most frequently compared to 'a cord drawn tightly round the body,' to 'a heavy load lying' upon the part or internally, or to a sensation of 'being blown up' with flatus. Other kinds of dull pain are designated as 'aching,' occasionally as 'throbbing,' and sometimes as 'sinking.' Next to the dull pains, in order of frequency, come those which are described as 'spasmodic,' 'twisting,' 'pinching,' 'tearing,' 'dragging,' 'gnawing,' and 'scraping,' all of which, with the 'darting or lancinating pain,' 'like a knife or sword running through,' are to be enumerated under the head of *acute* pain. . . .

"The several varieties of pain described are not confined each to its own locality; but yet it is possible, in a general way, to make some sort of *topographical distribution* of them. Thus the dull kinds of pain are, with few exceptions, referred to the sternum, epigastrium, ensiform cartilage, interscapular region, or upper part of the abdomen, rarely to the hypogastrium or inferior regions. The more acute kinds of pain are mostly located at the seat of palpable tumours, at the ensiform cartilage, the region of the heart, the iliac regions, the parts about the umbilicus, the lower part of the abdomen, or the abdomen generally. Those pains which patients describe as 'gnawing,' or 'scraping,' affect the epigastrium, and lower end of the sternum, and neighbourhood of the ensiform cartilage, much more frequently than any other part; while those described as 'soreness,' 'smarting,' or 'burning,' are most frequent at the epigastrium, lower part of the sternum, and upper part of the abdomen generally, sometimes also being referred to the situation of a palpable tumour." (pp. 12—15.)

The pain is connected frequently with the quantity, less commonly with the quality, of the food which has been taken; the quantity, tempe-

rature, and quality have variable effects. The time at which pain occurs after food is variable, from a minute or two to two or three hours subsequently. Its duration is equally uncertain. It is often associated with the following symptoms—tenderness on pressure, thirst, feverishness, vomiting, eructation of liquids or gas, borborygmi, or temporary abdominal swelling, from development of gas. The causes of pain after food, and the remedial measures necessary in each case, are then given in the table before referred to. We throw the former into the following scheme:

Causes of Pain after Food.

I. Unusual irritation	{	A. Irritating ingesta...	{ Temperature too high or too low. Alcoholic liquids. Stimulant condiments.
		B. Ingesta irritating from delay in the stomach	{ Food too hard originally or from preparation. Food badly masticated from various causes. Food imperfectly prepared by insalivation from various causes. Gastric juice deficient from too great, too rapid, and too frequently repeated eating; from mental distraction; from bodily fatigue; from atony, anæmia, hyperæmia, or structural changes of the mucous membrane; from flatulent distension; too great dilution of food. Muscular movements of stomach deficient from atony or hyperæmia.
		C. Ingesta not properly carried from the stomach	{ In atony, paralysis, organic disease, mechanical obstruction.
		D. Over-acid secretion into the stomach	{ From various causes of irritation, organic disease; gout, diabetes, blood disease, pregnancy, &c.
		E. Acid changes in the food	{ From delay, or from bad secretion of gastric juice.
II. Abnormal sensitiveness of the stomach	{	A. Exalted nervous impressibility	{ General. Local.
		B. Hyperæsthesia from hyperæmia and inflammation	{ From prolonged irritation, from intropulsion from surface, retrocession of cutaneous eruptions, febrile excitement, impediment to portal circulation, from suppressed hæmorrhages, irritant poisoning; cancer, chronic ulcer.
III. Perforation of the stomach.			
IV. Distension of the stomach	{	A. By excess of food. B. By flatus.	
V. Spasm of stomach.			
VI. Abnormal conditions external to the stomach	{	A. Contraction of the space which is normally allotted to the variations in size of the stomach.	
		B. Diseases in which the contractions of the intestines are painful.	
		C. Diseases of abdominal organs accompanied with tenderness.	

We have somewhat shortened Dr. Ballard's arrangement, and have not included all his minor points. We question whether he has not been unnecessarily minute, and whether, without any real sacrifice of accuracy, he might not have avoided repetition. However, the table is decidedly an useful one.

To every heading is affixed a number, which refers to a paragraph in the "Commentary" following the table, and composing the bulk of the work. We shall refer to one or two points.

The fourth cause of "unusual irritation" is stated to be over-acid secretion into the stomach. We turned with some curiosity to the paragraph in the "Commentary" in which this point is debated, to see if Dr. Ballard had any real evidence to offer of increased acid *secretion*, apart from acid *production*, by changes in the food. He accepts, however, without question, the assertion that the gastric juice may be more acid than natural, without referring to his authorities. The *production* of acid in the food, lactic, acetic, and sometimes hydrochloric (possibly from decomposition of the chloride of sodium), can be proved with tolerable certainty; and it is also likely that the secretion of acid may, as Dr. Ballard asserts, be too abundant. But as such evidence on this point as is known to us is very inconclusive, we should have been glad to know Dr. Ballard's precise grounds for his belief.

Under the head of flatulence, again, we have the common and obvious cause of changes in the food; but in addition, Dr. Ballard speaks of a secretion of gas from the mucous membrane. In proof of this, he refers in the "Commentary" to those cases, in aged and in hysterical persons, in which the stomach gets sometimes filled with air with great rapidity. But hysterical women certainly swallow vast quantities of air; and when, as in some cases, the distension of the stomach and intestines is so rapid as to lead us to suppose it must arise from actual secretion of gas, there is, it appears to us, usually no pain.

Under the head of atony, Dr. Ballard writes: "This state of the stomach may be, and commonly is, associated with general muscular debility or atony, the recognition of the presence of which, and of its known causes, is consequently most important in the diagnosis of the pain after food. When there is general atony, its signs are exhibited throughout the muscular system, voluntary and involuntary."

But surely there are many cases of general paralysis with atrophy of almost all the voluntary muscles, and many cases of fatty heart without the least coincident affection of the stomach. Do these affections run at all parallel? When atony of the stomach occurs without this general affection, by what signs can it be recognised? Are they entirely negative or positive signs?

We should have been glad to have seen these and other topics more fully treated of in the "Commentary;" and if Dr. Ballard had referred to his authorities, he might have written a longer and a heavier work, but it would have been one of greater permanent value. The work is, however, a very useful one; and all who have to treat the troublesome symptom it discusses, will find it an useful guide in discovering the causes, and in suggesting the remedies, of pain after food.

REVIEW XII.

1. *A View of the Formation, Discipline, and Economy of Armies.* By the late ROBERT JACKSON, M.D., Inspector-General of Army Hospitals. Third Edition.—London, 1845. 8vo.
 2. *The Queen's Regulations for Army Hospitals.*—London (unpublished). 8vo.
 3. *The Medical Codes of the Bengal, Madras, and Bombay Armies.* Fort William, Fort St. George, and Fort St. David (various dates). 8vo.
 4. *Etudes sur le Service de Santé Militaire en France, son passé, son présent, son avenir.* Par L. J. BÉGIN, Chirurgien-Inspecteur, &c.—Paris, 1849. 8vo.
- Remarks on the Military Medical Service in France; its Past, its Present, and its Future.* By M. L. J. BÉGIN.
5. *Réorganisation du Service Sanitaire de l'Armée Belge.*—Bruxelles, 1847. 8vo.
- Reorganization of the Sanitary Service of the Belgian Army.* Brussels. 1847. 8vo.
6. *Letter to the Right Hon. the Secretary at War on the Medical Department of the Army.* From SIR GEORGE BALLINGALL, Regius Professor of Military Surgery in the University of Edinburgh.
 7. *Recueil de Mémoires de Médecine, de Chirurgie, et de Pharmacie Militaires rédigé, sous la Surveillance du Conseil de Santé.* Par MM. JACOB, Docteur-en-Médecine, Ancien Pharmacien-Major; PARDIN, Médecin-en-Chef de l'Hôpital Militaire du Roule; et par interim A. JUDAS, Ancien Médecin Principal de Première Classe. Publié par ordre du Ministre de la Guerre. Deuxième Série. Troisième Volume.—Paris, 1854. 8vo.
- Collection of Memoirs of Military Medicine, Surgery, and Pharmacy, drawn up under the Superintendence of the Council of Health.*

FEW subjects connected with the war now waging with the greatest and most aggressive military power of modern times, have excited so intense and painful an interest in the public mind, as the state of the sick and wounded of the British army in the Crimea.

The medical department has been charged with the gravest sins of omission and commission. To its alleged inefficiency, many have not scrupled to attribute much of the deplorable sickness and suffering, which have called forth the active and generous sympathy of the entire nation.

One of the many features which characterize the contest to which the Cross is now committed in defence of the Crescent, is the marvellous rapidity with which the minutest details of operations destined to occupy so engrossing a page in history, are circulated to every hearth and home in the kingdom. They are typical of our times, and are to history what the photographic representations of evanescent scenes are to the pictorial art. The magic process by which the fleeting foam of the curling wave is stamped upon the fairy film, and caught ere its sparkling drops are

absorbed in the great ocean from which it for an instant rose, has been imitated in the records which daily and hourly reach us from the seat of war. The magnificent word-pictures of the many able correspondents present with the field force—than which the narrative of no human events contains matter more full of vivid and undying interest—have enabled us to realize the scenes of brilliant valour and patient suffering now enacting in the Crimea, with a clearness of which the published records of no preceding campaign afford a trace. The imperishable feat of arms which concluded the great contest in which our fathers were engaged, could not have more completely riveted the attention of the civilized world, than does the tremendous struggle in which the Western Powers are now engaged, to roll back once more the tide of Northern barbarism, which, again, after the lapse of so many centuries, threatens to Vandalize the fairest portions of the habitable globe.

The busy note of preparation, heard faintly and fitfully at Varna but a short while since; the sailing of the mighty armada, to which the epithet of grand must henceforth be transferred; the heroic rallying of its gallant host from the blighting influence of pestilence and death in the forms most abhorrent to the nature of the warrior; the bold and bloodless triumph of the unopposed descent upon the Crimea; the brilliant victory of the Alma; the masterly, but probably mistaken, movement on Balaklava; the wondrous tilting of the British cavalry, paling the romance and heroism of chivalry itself; and the crowning contest on the heights of Inkermann, to be ranked hereafter with Thermopylæ and Agincourt, are already, on the threshold of the contest, indelibly graven on the tablets of time. Not less striking and memorable has been the marvellous *morale* which has characterized this immortal band of islanders in sufferings and privations equalled only by those of the retreat from Moscow, or the disastrous destruction of the Cabul force. Any other army in the world would have succumbed, utterly demoralized, in such apparently hopeless circumstances; and yet, there was not a moment when a single soldier in that force capable of raising a firelock or wielding a sword, was not ready to meet and vanquish any human foe who dared to assail him in the broad face of day, or steal upon him exhausted by a night of vigil and fasting in the trenches.

That such men should have been sacrificed to a defective system of organization, and the absence of prudence and forethought, can now be only a source of profound and unavailing regret. It was believed that the genius of the Great Duke had raised the military character of the nation to a standard that secured it for all time against the recurrence of Walcheren expeditions, and the failures that earned for Great Britain the contempt of Europe in the times of the First and Second George. The correspondence of that eminent soldier, as contained in the record given to the world with his sanction, has shown how he acted in circumstances of peril and privation, the means which he adopted to rid his army of official incapacity, and the eminent success which crowned his efforts. The world was not prepared to find the pupils of that great master in the art of war so oblivious of his maxims, as to commit, without the faintest shadow of excuse, the very faults which he so strongly reprobated. The deeply humiliating spectacle has been exhibited of a British

army, unequalled in daring and discipline, perishing of want in the vicinity of abundance; naked and tattered within sight of stores of clothing; clothed, fed, and transported when sick, by a gallant and generous ally, to whose superior organization it owes its very existence.

The department upon which it has been attempted to cast the greatest amount of obloquy has, in truth, been the least deserving of censure. In spite of deficiencies which the parsimonious paring of peace had produced, to an extent that would have paralysed any other branch of the service, the labours of the medical officers have been more successful than those of any others of their fellow-sufferers in the camp before Sebastopol.

The skill of the engineers, aided by the best practice of the artillery, has failed, as yet, to produce any impression upon the stronghold of Russian power in the Black Sea. The gallant efforts of the fleets have been equally impotent against the seaward defences. Such has not been the result of the surgical labours consequent on the great battles fought, or the medical skill which, in the transport of more than twelve thousand sick from Balaklava to Scutari, has kept the mortality down to six per cent. Whenever professional knowledge could be applied with any reasonable chance of benefit, it has been granted cheerfully, willingly, and successfully; and the practice of the medical officers in the field will bear the strictest comparison with that of the best regulated and most efficient hospitals in any capital of Europe.

Such being, as we conscientiously believe, the case, we purpose to consider the shortcomings with which the medical department has been charged, the causes to which they are really due, the means by which they may be avoided for the future, the organization of the medical corps generally, and the measures required to place it upon the footing of complete efficiency demanded by the exigencies of the great contest in which it must necessarily take so prominent a part.

The present appears also to be a favourable opportunity of bringing to the notice of the profession the medical arrangements of the Indian army, by which provision is made for the care and treatment of a force of nearly 400,000 fighting men, as well as the introduction to a population exceeding that of the whole of Europe, of the art and science of medicine as now taught and practised in the West.

With regard to the alleged deficiency of medical officers in the Crimea, it is easy to prove that the presiding authorities of the department exerted themselves to meet the demands of the campaign with an energy that cannot be too highly commended. Had a tithe of the foresight exercised by the Director-General been exhibited by the heads of other departments, the world would not have been scandalized by the miserable picture of suffering daily exhibited in the public journals; and the reported capture of Sebastopol would have been the greatest fact, instead of the greatest fiction, of the past year.

The peace establishment of the medical department was, if possible, less calculated to meet the urgent demands of actual warfare, than any other branch of the army. *It had no purveyors, no hospital establishments deserving of the name, no apothecaries, and a complement of commissioned*

officers barely adequate to discharge the duties connected with the sick in garrison, in various parts of the world.

So rigidly was the number reduced to the minimum sanctioned by unwise economy, that medical officers were seldom able to leave their posts until worn out, and not unfrequently destroyed, because they could not be relieved from duties which humanity forbade them to abandon. When compelled themselves by sickness to visit Europe from distant and unhealthy colonies, if unable to return to active duty within a brief period, they were forced upon half-pay, because the regulated establishment could not be exceeded, and their places *must* be supplied.

Here, as elsewhere, there was no reserve to provide against contingencies inseparable from service in the tropics, or other places unsuited for the unacclimated European. The 21st Fusiliers in Bengal, in the early part of 1840, was not only without a single medical officer of its own, but was under the charge of a Company's assistant-surgeon of three months' standing, with two officers junior to himself in the same service, to aid him in the medical care of the regiment. Not an officer in the royal army in that Presidency could be spared for so important a charge, while a large portion of the corps was in hospital from cholera, dysentery, and fever. Until very recently, there was scarcely a Queen's regiment in India that was not indebted to the Company's army for one or more of its assistant-surgeons; and, even now, they are occasionally to be found performing duties for which the complement allowed by the Crown is insufficient. The hardship of punishing medical officers for afflictions too often caused by the arduous nature of their duties, is confined to that branch of the army. The sick engineer is not placed upon half-pay until he can recover his health; nor does any other staff officer forfeit his position in the active branch to which he belongs, in similar circumstances.

The demand for an increased medical establishment came upon the country almost simultaneously with a severe epidemic visitation of the most formidable of modern scourges, to combat which the entire available force of the faculty was barely sufficient.

Very many of those, whose standing in the profession rendered them the most desirable recruits, were absent, scattered over the vast surface navigated by the merchant navy of Great Britain. Some time, of necessity, elapsed ere their services gradually became available. There was no medical militia to fall back upon, to recruit the line of the profession. And yet, in spite of all these untoward circumstances, a larger number of medical officers was sent to the army in the East, than had ever before accompanied a British force to the field.

That they have proved numerically insufficient for the wants of that army, can scarcely be with reason attributed to any neglect of the medical authorities. The inducements to enter the service at all, are not sufficient to tempt the largest and best class of medical men to seek military employment. This is a matter for the State to rectify, and the sooner it is done, the better will it be for the army, and for the national credit.

The medical staff of the expedition sent out since the spring of 1854, amounts to 373, distributed as follows:—2 inspector-generals, 6 deputy-inspectors, 18 staff surgeons of the first class, 29 staff surgeons of the

second class, 110 staff assistant-surgeons. The remainder includes the regimental complement of a surgeon and an assistant to each corps of cavalry, a surgeon and three assistants to each regiment of infantry, and a surgeon and sixteen assistants for the artillery.*

The increment of an additional assistant to each regiment of foot was an unwise measure, as subsequent experience has proved, and as Dr. Smith originally pointed out. The sick and wounded of regiments in the field are most judiciously sent, as early as practicable, to field hospitals, and thence transferred to Scutari, where provision can be made for their careful and efficient treatment to an extent that is neither desirable nor possible with their own corps. These, whether engaged in the arduous duties of the trenches, in defending the outposts of the army, or otherwise employed in active operations against the enemy, could not be hampered with the helpless and disabled, without serious detriment to their own efficiency, and readiness to encounter the ever-varying incidents of a strenuous struggle with a highly-disciplined, watchful, and wary foe. The regimental surgeons have not, therefore, the same fixed hospitals as in time of peace, and except for the first few hours or days succeeding an assault or a general action, have not the same continuous demands upon their time and attention as the staff surgeons. The latter required strengthening to an extent proportioned to the casualties of the campaign, whether medical or surgical in their nature.

By the Royal Warrant of October 1st, 1840, the medical hierarchy of the Queen's army consists of—

- Assistant-surgeons, ranking with lieutenants.
- Regimental surgeons, and staff surgeons of the 2nd class, with the relative rank of captain.
- Staff surgeons of the 1st class, equivalent to majors.
- Deputy inspector-generals of hospitals, corresponding to lieutenant-colonels.
- Inspector-generals of hospitals, with the grade of colonels.
- A director-general to rule over the whole, with the rank of brigadier-general.

The accompanying tabular statement exhibits the full pay of the various officers mentioned, with the exception of the head of the department.

FULL PAY.									
Rates of daily pay, subject to the provisions of Warrant of 14 Oct. 1840.									
Rank.	After 25 years' actual service.			After 20, but under 25 years' actual service.			After 10, but under 20 years' actual service.		
Assistant-surgeon	£0	10	0	...	£0	10	0	...	£0 7 6
Regimental surgeon and staff surgeon, second class	1	2	0	...	0	19	0	...	0 13 0
Staff surgeon, first class	1	4	0	...	1	2	0	...	—
Deputy inspector-general of hospitals	1	10	0	...	1	8	0	...	—
Inspector-general of hospitals	2	0	0	...	1	18	0	...	—

The pay of the director-general is 1200*l.* annually, a sum utterly in-

* This statement is compiled from the Monthly and Quarterly Army Lists to the 1st of February, 1855.

adequate for the chief of such a department, and less than is earned by many general practitioners in the city of London.

In this, as in almost every other regulation relating to the medical department, it is treated with an illiberality discreditable to the country, and little calculated to secure the services of men of ability. The pay of a staff surgeon of the first class is barely sufficient to enable him to live in a manner consistent with his social position, and in the matter of horse allowance he and all his professional staff brethren are not placed in the same position as other staff officers.

It would be tedious to refer to all the regulations on the subject in proof of the injustice pointed out; it will be easy to do so, if the accuracy of the statement is impugned.*

The nominal rank of a medical officer is a mere sham, as it is invariably set aside whenever he comes in contact with his purely military brethren, even when no question of military command is concerned.

It is but a short while since he was placed on the footing of a combatant, although fully exposed to all the vicissitudes, and many of the casualties of war. Unlike his brethren in arms, there is no peace for *him*. He is perpetually engaged in warfare with disease, and more exposed to suffer in health and life from contact with contagion, than any other class of military men. He enters the plague ward or the focus of fever with a greater amount of heroism than animates the soldier in the deadly breach or the daring charge. There is none of the pomp and circumstance of war, with its glare and glitter, to urge him to face the grim enemy, and to crown him with the laurel of the victor amidst the shouts of his admiring comrades; but the moral courage, which the great Napoleon held to be double that of the physical impulse which urges men to deeds of daring, never fails him, or allows him to shrink from the performance of any duty, however perilous.

On the field of battle and in the presence of the enemy he is frequently exposed to the missiles dealing destruction around him, and "confronts death without seeking to inflict it." And yet, the honours grudgingly bestowed are as sparingly awarded to him, as if he incurred no risk and had done nothing to deserve well of his country. He serves, indeed, "under the cold shade of aristocracy," and has little more to sustain him than the consciousness of performing, fearlessly and faithfully, the highest and most responsible duties that fall to the lot of man. Mercy and humanity are his mission. To give sight to the blind, strength to the weak, assuaging of pain to the anguished, relief to all whom human aid can rescue from the grasp of the fell destroyer, are his daily task. "*Nullâ re homines propius ad Deos accedunt, quam salutem hominibus dando,*" said a wiser and a better man than any living War Minister, who damns the department with faint praise, and mentions the heroism of one of its members, whose life was sacrificed on the altar of duty, in an apologetic strain—as if apology were well-timed or necessary to introduce a name to

* The most unjust and detrimental of all are the fact of medical officers not drawing the pay of the higher rank to which they are promoted for some time—occasionally, years—afterwards, and the grossly illiberal manner in which acting assistant-surgeons are treated. As there are no acting ensigns and lieutenants, so there should be no acting assistant-surgeons. Every encouragement should, on the other hand, be held out to induce men to serve during the war, who may not wish permanently to adopt the army as a profession.

the courtly peers that is worthy of being recorded with those of Eldred Pottinger, and the gallant defenders of Silistria. In real heroism, the exertions of Dr. Thomson upon the cold and silent field of the Alma, exposed at every moment to receive death from the hands of those for whom he remained to preserve life, probably exceeded the chivalrous defence of Herat, and the wondrous preservation of Silistria against the prowess and skill of the best soldiers in the Russian camp. A courtly eulogy is all the reward likely to be bestowed upon the memory of one, whose rare virtue is deserving of a nation's gratitude!

In the French army, which there can now be no excuse for rejecting as a model in this and many other military matters, the Legion of Honour is thrown open to medical officers on the same conditions as to other branches of the army. Distinguished merit can secure it in all ranks, and the highest position in the hierarchy of decoration can be obtained by the military surgeon.

In the first and only distribution of the honours of the Bath yet made to the medical officers of the British armies, the second class was deemed sufficient for the director-general, and for one officer of the Indian army. It would not be difficult to discover in the list of Grand Crosses men without a tithe of the claims of Sir James Macgrigor to be placed there—general officers who have lost the baggage of their armies, gained doubtful victories, and failed in every important enterprise entrusted to them.

The fountain of honour appears to have dried up, after its first great effort to sprinkle incense upon the martial sons of Æsculapius.

In the recent Burmese campaign a field officer was made a Companion of the Bath who was never under fire during that passage of arms, and the chief of the medical staff was, as usual, left undecorated.

In the small Belgian army, when it was reorganized in 1847, there were medical officers decorated with the orders of Leopold, the Cross of Iron, and the Legion of Honour; and among them was one assistant-surgeon.

There are peace services as well as war services, and in many instances the eminence attained in the former is of a much higher order than can ever be acquired in the latter. All are alike unrequited and disregarded.

It may be urged, that a craving for earthly honours and distinctions is unbecoming a profession whose highest reward is the consciousness of doing good. It would be easy to expose the fallacy of such arguments, if they were deserving of consideration. So long as rank, and titles, and other badges of honour, are deemed marks of the approval of our fellow men, none need be ashamed of desiring to be classed among the deserving.

A large portion of popular indignation has been expended upon the reported deficiencies of lint, medicines, medical comforts, and even the most simple and necessary means of treating the sick and wounded. Subsequent inquiry, and the testimony of those best entitled to belief in such matters, has shown that ample provision for ten thousand sick and wounded was made by the director-general, as soon as the nature and extent of the proposed campaign was made known to him. Beds, bedding, lint, bandages, instruments, and physic, were provided upon the most liberal estimates furnished by past experience, for the number above mentioned.

That number has not yet been attained,* and never would have been approached if proper means had been taken to clothe, feed, and lodge the soldier, so as to protect him from all avoidable causes of disease and disaster.

In spite of the most urgent entreaties, remonstrances, and representations, a vast *depôt* of hospital stores was left at Varna, within forty-eight hours' sail of Scutari, while the sick and wounded at the latter place were undergoing much unnecessary suffering and mortality from their absence. The noble and unparalleled sympathy of a generous nation was roused, and a profusion of supplies of all descriptions found their way to Constantinople, and were available at Scutari, for many weeks before the short distance to Varna could be accomplished by the culpable and incapable authorities whose duty was so shamefully neglected.

The medical authorities furnished the means and appliances. *To convey them to the scene of action ; to see that they were carefully and properly packed ; to regulate their issue at the proper time and place—was absolutely beyond their control.* To hold them responsible for the result is about as reasonable as to charge them with the surprise at Inkermann, the sacrifice of the light cavalry at Balaklava, or any other of the acts of neglect and incapacity, which have paralyzed and nearly destroyed the field force in the Crimea.

Not only have the medical officers no authority in such matters, but their representations regarding the sick are said to have been disregarded, unacknowledged, and treated in a manner as unbecoming as it has proved prejudicial to the service. In one recent example, it has been publicly stated that a staff surgeon of the first class was threatened, by no less a person than Lord Raglan himself, for telling the truth in rather plainer terms than was palatable at head-quarters. Had his statements been false or exaggerated, the means of disproving them were easy and at hand. There is, in sad and sober truth, no circumstance connected with the disgraceful disorganization of the army in the East more discreditable, and less characterized by magnanimity, than the treatment of the medical department of that force. It is to be hoped that the public will be fully informed upon this matter, when the result of the Commission sent out by Dr. Smith shall have been made known.

The absence of the ambulances at the battle of the Alma, and their subsequent inutility, have also been, with equal injustice, charged against the medical authorities. The organization of that corps was the result of no medical mismanagement, for the selection of the men was made without the knowledge, approval, sanction, or official cognizance of any professional authority whatever.† Indeed, it is absolutely impossible that the junior assistant-surgeon in the service could have been guilty of the grievous ignorance of selecting a body of men past the period of active life, for such duties as are required in an ambulance corps. This body, such as it was, was brought down to the beach at Varna, and, after much difficulty on the part of the military authorities, actually embarked, when, at the eleventh

* February 1st, 1855.

† We have, since the above was written, seen a statement to the effect that it was organized by Colonel Tulloch and Mr. Guthrie.

hour, it was again disembarked and left behind. *It was displaced to make room for a troop of Dragoons, which was, after all, never taken on board;* thus forming an additional link in the unparalleled chain of blunders that will immortalize this expedition. The medical authorities were in no wise to blame in the matter.

The state of the hospital at Scutari has, by those who ought to have known better, been attributed to the neglect of the medical department. That much confusion, and a proportionate degree of personal suffering, will, in all cases, result from the sudden influx of an extraordinary number of sick into even a well-appointed civil hospital, is undoubted; but that it should therefore be regarded as a proof of the neglect or incompetence of the staff of that hospital, is by no means a logical or necessary inference. It has already been shown that the apparatus necessary for furnishing such a hospital was lying at Varna, beyond the reach of the medical officers. Any one at all acquainted with the necessary and complicated machinery which an army surgeon requires to set in movement to procure what should be under the immediate control of his own department, will at once understand the difficulties that must have been encountered at Scutari to procure even what was procurable upon an emergency that would not have existed but for the unpardonable neglect of those whose duty it was to transfer the invaluable resources rotting at Varna to Scutari, as soon as the campaign in the Crimea was determined on. The temporary expedients resorted to in all great cities during severe epidemic visitations of disease, when the ordinary hospital accommodation is unequal to the emergency, shows how great are the difficulties inseparable from such calamities, in the heart of civilization and abundance. All have borne testimony to the unwearied exertions of the surgeons, many of whom, from over-fatigue and anxiety, were fitter inmates for the wards than for the ranks of active duty. Nor can language convey too exalted an estimate of the invaluable labours of Miss Nightingale and her band of nurses. The employment of female agency in alleviating the horrors of war and bringing the most sacred of all sources of consolation to the pillow of the dying soldier, can only be fully appreciated by those familiar with such scenes.

Much stress has been laid upon the unnecessary exposure, suffering, and consequent mortality in the sick and wounded transported from Balaklava to Scutari. The returns recently published in one of the medical journals show that the number of deaths was grossly exaggerated; and although that document is incomplete in not showing the actual number disembarked after detention upon the completion of the transit, it proves that considerable success attended the exertions of the surgeons who were sent in charge of those freights of human misery.

It would, in the most promising circumstances, be almost impossible to exaggerate the horrors and sufferings of a sea passage, during the winter, for the sick and wounded. The angry Euxine is not very favourable for such expeditions; nor are the closure of ports and battenning down of hatches calculated to mend matters. *But the fitting-up of transports for the sick is to a very small extent under medical management, and the*

surgeon is compelled to take what is provided for him, however strongly he may disapprove, and vainly he may protest against it.

Having thus considered, however imperfectly, the shortcomings with which the department has been charged, let us for a moment turn to the causes to which they may fairly be traced.

The first and greatest of all has already, in some measure, been discussed. It is the utter inadequacy of the peace establishment to meet the emergencies of war.

The next in importance is the unwise neglect with which the counsels of medical officers of all ranks are treated, due probably, in some degree, to the improper position occupied by them in our military system. This is to some extent shared by the medical branches of all the armies in Europe, but in none is it carried to so injurious an extent as in that of England. This point is so important that we must stop to consider it.

It would not be difficult to prove that the medical department is by far the most highly educated and scientific corps of the service. The only one that can be compared with it is the engineer department, and even this does not embrace in its duties and acquirements the extent and variety of knowledge required and possessed by a well-educated member of the medical profession.

The young surgeon, after undergoing the usual school training in the ordinary classical and general education afforded by our public seminaries, commences his professional training at the age of sixteen. This extends over a period of five years, embracing the study of anatomy, physiology, chemistry, botany, natural history, and the purely professional subjects of surgery, physic, midwifery, and forensic medicine. To this is super-added the practical study of disease in hospitals. To obtain such an acquaintance with the subjects above mentioned as shall qualify him for the practice of his profession, demands considerable mental powers, unwearied application, and great self-denial. All graduates in medicine who have received a liberal education are usually acquainted with one or more modern languages, and are generally as well-informed as most members of the other learned professions in the departments of literature and science that form parts of university schemes of study.

The young engineer starts from school with perhaps as good a preliminary preparation, but his professional course of study cannot for a moment be compared in extent, variety, and importance, either as a mental training or as a special qualification, with that of the cultivated physician.

Nor can his subsequent duties be deemed more useful, or of a higher order, than those of the medical calling. Road-making, bridge-building, sapping and mining, fortification, the attack and defence of fortified places, geological surveys, and the various other functions that fall to the lot of the engineer, demand no higher powers of mind, no greater moral and physical courage, no larger development of intellectual resources, than do the duties of the medical officers in the camp and the field. The soldier, to fight, must be possessed of health and strength. The destructive agencies banded together against his life and limb are infinite in actual warfare, and the highest resources of the healing art are never more sorely and severely taxed, than in diminishing or neutralizing those agencies. We have no hesitation in declaring our belief—and the history

of all contests proves the accuracy of the deduction—that *the perfect organization of the medical arrangements is the most important element of success in war*. If they fail, and an army melts away from disease, the most brilliant valour, the most skilful generalship, and the most consummate science of the purely fighting portion of the machinery, are alike of no avail.

Yet how are the engineer and medical departments treated, as component parts of the same machine? In the former there are, at the present moment, eleven lieutenant-generals, six major-generals, fifteen colonels, and thirty-seven lieutenant-colonels, twelve of them with the brevet rank of colonel. In the latter there is one officer with the nominal rank of brigadier-general, and seven with that of lieutenant-colonel. By what principle of candour, fairness, or policy, can this be justified? It is not military authority that is contended for—that would be unreasonable to ask and impossible to grant—but it is the rank that adds dignity to office, and increases the usefulness of the officer in the performance of his duties.

One of the best officers the British army has ever possessed, and who bequeathed to it a legacy only surpassed by the Despatches of the Duke of Wellington, has said on this subject :

“The rank accorded to the medical officer does not injure, or even interfere with the military. Rank is of no intrinsic value in itself to a man of science ; but the opinion connected with the rank makes an impression on the soldier, which aids materially in giving force to medical authority, and, consequently, to medical utility. The soldier is accustomed to view things superficially, to estimate and judge by the exterior only ; for, as he is not permitted to reason and resolve to principles, the science of the medical art is less regarded by him than the authority of the rank under which it is applied to him. For this reason, we venture to assert, that if the medical officer stands in what may be called a degraded rank in military estimation, the usefulness of the medical art will lose much of its value as applied to a military subject.

“The matter now under view is of some consequence to the interests of the army ; and it is not, it is presumed, beneath the dignity of the higher powers of the State to consider it, if it be held to be a national concern to arrange the various departments of the army on a basis of justice and truth. Those who hold high official stations, and particularly those who wield the sword, are strongly disposed to depress men of science ; and, among others, the medical department, which is a department of science, has been degraded of late years—at least, barred from rising to a rank suitable to its importance. But, be that as it may, the history of our most brilliant campaigns will not permit our most celebrated generals to say that nothing is due to the medical staff, when that staff is allowed to act according to its judgment.”

The work of Dr. Jackson, from which the above is extracted, heads the present notice, and had it been carefully studied by the War Ministers, general and staff officers engaged in the present contest, many of their most glaring and mischievous errors would have been avoided.

Similar sentiments have been expressed and recorded by Percy, Larrey, Desgenettes, and others of the most distinguished military surgeons of the Continent of Europe. The former presented a memorial on the subject to the Emperor Napoleon after the battle of Eylau, strongly advocating the assimilation of the medical to the engineer corps. It was not adopted because the plan was incomplete.

The disregard of the ordinary representations of medical officers in the

Crimea is notorious: it has produced its usual fruits. No body of educated men, whether commissioned or otherwise, are bound to submit to insult from authority, however high. Finding remonstrance useless, they ceased to remonstrate, and the unnecessary sufferings of the sick, beyond the control of those most interested in their welfare, has caused the indignation of the entire nation, and earned for us the contempt of Europe.

Had the smallest pains been taken during the long peace to profit by past experience and the counsels of such men as Dr. Jackson, in this age of education and progress, there is no reason why the British soldier of 1854 should have been as little able to take care of himself as his predecessor of 1804. The contrast drawn between the French and English systems in the early part of the century, reads as if it had come from the camp at Balaklava:

"In the French, utility and effect prevail over uniformity of appearance; knowledge of animal structure, and acquaintance with capacity of action in different structures, are deemed necessary to adjust and measure the effect. The exercises of manual and manœuvre are performed in the French army with a celerity and precision that cannot, perhaps, be exceeded; the explosions from the firelock astonish by close repetition. The effects of movements and evolutions in the face of an enemy, as studied in their reasons, are presented to the eye of the soldier, while under training, in such a manner that he may be supposed to comprehend the design and execute the measure—not passively as a part of a machine, but actively and with energy as an intelligent being. Besides practice in manual and movement, which is the ostensible object in military training, pains are taken by the French tactician to lay the base of correct interior economy in the elements of the army; hence the recruit is instructed in the best manner of taking care of himself, with a view to enable him to maintain his efficiency as a part in an instrument of force. He is instructed, for instance, and scientifically instructed, in the best manner of dressing the raw material of the ration, so as to form a wholesome and savoury mess; and from this and other knowledge that belongs to interior economy, he suffers less privation and fewer hardships in the field than the troops of other nations similarly circumstanced—particularly than the British, who, the most brave, perhaps, of any soldiers in Europe, are the least competent of any to take care of themselves."*

The soldiery are no longer the scum of the country and the outpouring of the jails—the *enfants perdus* of society. They have advanced in intelligence to an extent that few anticipated before the publication of their letters, and the testimony of such men as the Rev. Mr. Osborne. There is no mystery in the art of cookery and the interior, to which Dr. Jackson refers, which could not be taught, and readily acquired by, the English soldier, who is as educable as any other class of his countrymen. But it is hopeless to expect any amendment in the lower ranks until the staff corps are properly trained; until the officers of the army as a body are real professional soldiers, instead of gay and gallant amateurs; until the entire system of military rewards and promotions is remodelled; until the ridiculous traffic in commissions is a thing of the past; and until professional and general competency, with distinguished service in the field, are the only passports to rank and command.

The third cause of failure has been one already more than once alluded to—the absence of inherent authority in the medical officers to act on

* Jackson, op. cit., pp. 145, 6, ed. 1845.

their own responsibility, in any matter beyond the immediate and direct medical and surgical treatment of the sick and wounded. This is restricting their usefulness within the narrowest limit, and deprives them of the greatest element of success in all human actions—vigorous and direct action, based upon individual responsibility. Although it is not necessary to enlarge further upon it in this place, it cannot be too frequently repeated, or too earnestly dwelt upon.

The absence of a permanent sanitary commission in the British army, with no executive duties to perform, is to be regretted. It should be armed with power to consider all questions relating to the medical economy and *hygiène* of the soldier, in peace and in war; in Europe and in the tropics; in garrison, on the march, and in the field; in all matters relating to food, dress, equipments, exercise, parades, duties, and punishments, so far as they are liable to affect the health, and thereby impair the efficiency, of the soldier. It should examine, collate, and extract all that is valuable from the sanitary returns, which now answer no other purpose than to encumber medical officers with much harassing, detailed, and unnecessary correspondence—for it is a physical as well as a moral impossibility that they can, in existing circumstances, be made much use of by the Director-general and his personal staff.* It should, above all, when war is contemplated—and sufficient time is always allowed for such a purpose—collect and render available, in a condensed and practical form, all information, medical and topographical, in its hygienic relations, procurable regarding the country or countries liable to be the seat of military operations. The purely military part of such information should be furnished by a properly organized staff corps; and thus, with all procurable information regarding the topography, military and medical resources, climate, and diseases of tracts of country about to be occupied, an army would take the field provided, as far as human means can make such provision, for accomplishing its political and strategic purposes with the least possible sacrifice of human life.

Such provision, or something equivalent to it, exists permanently in the armies of all the great military powers of Europe.

In the thirteenth volume of the ‘Selection of Memoirs in Military Medicine,’ published by order of the Minister of War in Paris, during the past year, is a report† by the Health Commission of the French army upon the countries which were regarded as the probable theatre of the future contest, before Silistria had driven back its assailants, and the lower

* Some such commission was proposed, many years since, by Sir James Macgrigor, but rejected on the ground of expense, in consequence of which a vast mass of valuable records has accumulated, and been turned to no account.

† The paper is entitled ‘Medical Instructions for the Army in the East,’ is plain and practical, avoids all speculation and discussion, makes no attempt at deep research, and prepared the French surgeons for what they were likely to encounter.

“Every one knows,” say the Commission, “that an army entering upon a campaign requires special precautions to preserve it from the destructive influences upon health which may result from a sudden change of habits, mode of living, fatigue, and irregularities, which this novel state either necessitates, or inevitably has a tendency to produce. Memorable examples have demonstrated the certain efficacy of these precautions every time they have been observed, as well as the disasters which their neglect or oblivion involve.”

This now sounds prophetic, for, with the exception of Dr. Smith, no person in authority in the English army, or in its home administration, would appear, from the result, to have

valley of the Danube, with the adjacent countries, were likely to be occupied by the French army. It embraced the medical topography and meteorology of the chain of the Balkan, Bulgaria, Roumelia, Wallachia, and the valley of the Lower Danube. It also considered the diseases, endemic and epidemic, of those countries; their pathology, *hygiène*, and therapeutics, as regarded sick and wounded soldiers; and concluded with

bestowed a thought upon the subject. The report concludes with a "Summary of the precautions to be taken for the preservation of the health of the Troops in the East:"

"The Council of Health has been charged by the Minister of War to examine the precautions most proper for maintaining the health of the troops in the country to which they are called to make war. This Council has drawn up detailed instructions, from which the following particulars are extracted as necessary to be observed with the greatest care:

"1. It is necessary to be always so clothed as to be proof against the sudden chills to which one is liable, at all seasons, from the abrupt changes of temperature which very frequently happen in nearly all parts of this country.

"2. In summer, the best protection against sun-strokes, which are often very dangerous, is never to leave shelter without having the head covered.

"3. Cleanliness of persons, clothes, and dwellings, is imperiously required by the nature of the climate.

"4. Whenever practicable, the face, and particularly the eyes, should be frequently washed daily, after exposure to dust.

"5. The feet should *not* be washed with cold water, especially when heated after a march.

"6. The greatest care is needed for protection against the freshness of the nights, even when the heat is extreme; it is dangerous to remain clad only with the shirt during the night.

"At the bivouac and in the tent, the soldiers should be very carefully covered.

"7. When the camp is pitched near a marsh, a tank, pools of stagnant water, or a valley, the chief openings of the barracks or tents should be in the opposite direction. In these bivouacs every possible means should be employed to counteract the invariably noxious vapours exhaled by such foci of infection. At night, the openings of the barracks, with the exception of those indispensable for ventilation, should be closed.

"8. It is wrong to sleep in immediate contact with the ground; perfectly dry substances, not easily permeated by moisture, should be interposed. For this purpose fresh branches or vegetable matters should never be used.

"9. Water, drunk in large quantity, is always injurious. If, after a fatiguing march, a stream of water is met with, thirst must be sparingly satisfied, and the water reserved for subsequent use.

"10. When only a small quantity of water is procurable, instead of swallowing it, the mouth should be gargled as long as possible, and the water rejected as soon as it is warm.

"11. When reduced to the necessity of drinking stagnant water, it should, by way of precaution, be strained through a cloth, to separate leeches imperceptible from their smallness, and which it is very dangerous to swallow.

"12. A mixture of wine and water, brandy and water, or infusion of coffee and water, is always an excellent drink, taken in moderation. It should be mixed at the time of use, and not prepared beforehand, as it in that case becomes heated, changes, and no longer fulfils its purpose.

"13. When salted meat and fish are substituted for fresh meat, they should be soaked before cooking, and when practicable, mixed with a certain amount of vegetables.

"14. Condiments, in small quantity, are good seasoning; in excess, they irritate the stomach, and render thirst more difficult to bear.

"15. Saffron increases the digestibility of rice and flour; it is particularly useful with maize.

"16. Food should always be taken before a march.

"17. Before and after guard-mounting it is very useful to drink a moderate quantity of warm water, with a little brandy or infusion of coffee in it. Night guards should always be well clothed.

"18. Sickness should at once be reported to the medical officer.

"19. These recommendations shall be published in general orders. Every commanding officer will take care that they are read to the troops once a week at least, and that the officers superintend and direct their execution."

Many of these directions read like truisms, and yet experience shows that they require to be enforced, for everything must be made plain to the soldier.

The above extract is given without curtailment, as showing the manner in which the functions of a permanent sanitary commission may become of importance in war.

So entirely convinced are we upon the matter, that we trust ere long to see such commissions in existence in the English and Indian armies, and composed of some of the best officers in their respective services.

a few valuable general recommendations to the medical officers of the force.

In this matter, however, the French were anticipated by the English, and the wisdom and forethought of the director-general were as marked in this as in all the other arrangements which he was permitted to carry out.

As soon as it was known that a field force was to be sent to Turkey, upon the advice and recommendation of Dr. Smith, deputy inspectors-general Dumbeck and Linton, and staff-surgeon Mitchell, were despatched to examine and report upon the countries north and south of the Balkan chain, the tracts between Adrianople and Gallipoli, and, in fact, the whole line that would have been the seat of war had the Russian generals succeeded in their original plan. Each of the experienced officers above-mentioned furnished detailed reports regarding the diseases of the countries referred to, for the use of the chief medical officer and of the general commanding the army.

Portions of these reports have, from time to time, appeared in one of the medical journals published in London.

In addition to this, Dr. Smith caused to be written and published valuable notes on diseases in Turkey by Dr. Schulkof and Dr. Bryce, with useful practical memoranda by Dr. Bryson and Mr. Drummond. No medical officer can have studied them without advantage.

Having thus drawn attention to what we believe to be the charges made against the medical department, and the causes to which the deficiencies undoubtedly existing, appear to us to be due, we next proceed to the means by which we think they may be avoided for the future. These have, in some measure, been already incidentally referred to; but as they are sufficiently important to be developed more in detail, we shall make no apology for dwelling more particularly upon them.

They embrace the future supply of men and material, the selection and promotion of officers in the department, the places they should occupy in the military body, and such other incidental considerations as may be involved in the discussion of the points above-mentioned.

The scientific branches of the army, under which may be classed the Engineers, Artillery, and Medical Departments, are, with the exception of the very faint efforts made at Sandhurst to train officers for staff employments, the only portions of the military machine in which anything approaching to systematic organization has been attempted. Unlike the raw material for British heroes, they cannot be created upon emergencies; hence the greatest care is requisite that they should, in time of peace, not only be perfect in themselves, but be so established as to be capable of indefinite expansion, to meet the demands of war.

The radical defects of the medical organization at the opening of the campaign have been already mentioned. We shall recapitulate them. They were—THE ABSENCE OF PROPER HOSPITAL ESTABLISHMENTS, INCLUDING AN AMBULANCE CORPS; THE NON-EXISTENCE OF THE INDISPENSABLE CLASS OF APOTHECARIES;* THE ABOLITION OF THE STAFF OF

* To prevent misapprehension, we may observe that the duties of the apothecaries are not to treat the sick, but to supply the medicines, instruments, and other appliances used by the surgeons and assistant-surgeons.

PURVEYORS, ESSENTIAL FOR THE PROVISIONING OF THE SICK; THE COMPLETE SUBORDINATION OF THE MEDICAL TO THE MILITARY AUTHORITIES, EVEN IN MATTERS OF PROFESSIONAL DETAIL, WHICH THEY ALONE COULD PROPERLY UNDERSTAND AND CARRY OUT.

The first of these is understood to be in the course of organization upon an ample and liberal scale, that will include all the essential ministerial agency, without which no military hospital or establishment, in or out of the field, can possibly be in a state of efficiency.

Under the old system, the selection of the unfit instruments allowed by the Government did not rest with the surgeon; in most matters they were beyond his control; and if capable of aiding him, he was forbidden by the regulations to employ them in any professional duty.

The general reader will best understand the destitution of the army in this important particular, when told that it was exactly parallel to that of an engineer corps without a trained band of sappers, or a regiment of artillery without gunners. It is manifest that, in such circumstances, all the skill and science of the former would be unavailing in the construction of redoubts, field-works, parallels, trenches, and the other machinery of siege operations; and that, if the latter had to limber and unlimber their guns, to load, fire, stop vents, cut fuzes, and discharge the other practical details of gunnery, the siege of Sebastopol could have advanced little, and that of Troy would be rivalled in length, if not imitated in results.

Yet an analogous feat—the capture of a fortress without sappers or gunners—was expected to be performed by the medical department!

The most important of the changes will be, it is hoped, to place this hospital corps beyond the reach of colonels and adjutants, and to reserve it exclusively for its own proper and most important duties. From it should be selected the members of the ambulance corps—strong, steady, trained soldiers, accustomed to accidents and injuries in the wards of an hospital, habituated to handle fractured and contused limbs without inflicting unnecessary suffering, and even able, upon emergencies, to apply tourniquets, and arrest hæmorrhage temporarily, until the scientific skill of the surgeon can be brought in contact with the wounded man.

The ambulances should be of the lightest materials consistent with safety; be so constructed as to be capable of transport over rough, difficult, and roadless countries; be adapted to receive the wounded in the recumbent or sitting posture with the least possible difficulty, so as to place them *citò, tutò, and may-hap jucundè*, within reach of professional aid.

It should be able closely to follow the movements of an advancing or retiring force, and rescue the wounded before hæmorrhage can, as it so frequently does, convert trifling into serious injuries.

Such a corps and machinery—with the modern mechanical improvements that experience in such matters has suggested—as Larrey organized during the campaign on the Rhine, and which was hailed with such unmixed satisfaction by the soldiers of the French army, is what is required.

The nature of the country would, in some cases, modify the means of transport. The mule panniers of the French army, or the camel panniers of India, would, in certain circumstances, be preferable to all

wheeled conveyances. It should be the duty of the medical staff to ascertain, and they should be empowered to provide for, all such contingencies, before the army takes the field in a new country. The packing up of the instruments, and employment of bandsmen in the carrying of the wounded, has always appeared to us to be a foolish and objectionable measure, as well as in itself ill-calculated to accomplish the end in view.

The influence of martial music is very powerful upon the soldier, and is not sufficiently employed in the British army as a moral means of management. The band, instead of being a lordly luxury for the officers, should be liberally maintained by the State for the use of the soldier. This, as well as every other point, however apparently unimportant in the organization of an army, is well understood in all Continental armies.

To return, however, to our more immediate subject—the next deficiency was the absence of a staff of apothecaries. This has also now been supplied, and when its subordinate dispensers are collected and trained, will doubtless prove of the utmost use to the present overwrought and underhanded medical establishment. In the selection of its members, their training and qualifications, they should be assimilated to the *pharmaciens* of the French and Belgian armies, but should be under the direct control of the medical authorities.

From the apothecaries to the supply of medicines, and the other means and appliances of military hospitals, the transition is easy and natural. There should, we incline to think, be a great central store for the accumulation and rapid and regular distribution of such essential munitions of war, similar to the medical stores at the three Presidencies of India. The circumstances of the case are not perhaps exactly parallel, because England is herself the great centre of supply and manufacture of all such material. Yet recent experience has shown, in the case of minié rifles, and a thousand essentials for warfare, that it is unwise and imprudent to rely upon the general market; and that, to be fully prepared for emergencies, the Government should always have at command the means and appliances necessary. The result would be one of economy, as well as efficiency; and instruments and medicines would be procured both better and cheaper, under the system of the East India Company, than as a department of the Medical Board of the Queen's army. They could always be kept ready packed and *sorted for detachments of all strengths*, so that a regiment, a division, or an army could be supplied with a celerity and precision unattainable under any other system. The subject will be again referred to in a subsequent portion of this sketch, when the medical storekeepers of the Indian army, and their duties, are considered.

If it be essential that the medical appliances of hospitals should be always at hand, and under the directly responsible control of the medical staff, it is not less so that the commissariat arrangements for the sick and wounded should be equally *special*, and proper to the department. The old purveyor's regulations appear to be sufficiently plain and practical, and are probably again adopted; but it would be better that the hospital department should be altogether independent, and not compelled to resort to the general commissariat of the army for the supply of "fresh meat, bread, wine, and every other article for the consumption of the hospitals."

It must simplify accounts, prevent irregularity of supply, and might easily be managed, by the introduction of a little of the tact and common sense which characterize the arrangements of French hospitals in such matters.

When the arrangements for the issue and supply of medicines and hospital materials, the training of an efficient staff of hospital attendants, including an ambulance corps, and the organization of a hospital commissariat, are complete, the whole machine will work harmoniously, if it is placed under the *absolute control* of the medical authorities.

But, if military interference and supervision in the details of arrangements are introduced, and the real control and authority are thus removed from the hands of the medical staff, the old irregularities will recur, and all attempts to render the medical department an honourable, useful, and efficient branch of the army will assuredly fail, as they have done heretofore. Like causes, in similar circumstances, produce like effects; and the experience of the past will have proved little useful as a guide for the future, if the whole system is not radically changed. It has been tried, and the first rude contact with war has broken the glittering bauble. The medical corps has never really had the management of its own concerns. It is only fair that it should at least have a trial of its own.

Before quitting the subject of the Queen's Medical Service, we have a few words to say on the modes in which the members of that service are selected, promoted, and educated for their special duties.

The existing system of appointments to, and promotions in, the Royal medical department, is not susceptible of amendment, for they are not suited to the spirit of the times in which we live, and must be altogether changed.

There can be no doubt, without questioning the fairness and integrity of the head of any department at present entrusted with patronage, that all branches of the public service, in every one of its numerous ramifications, should be thrown open to the whole body of the educated classes in the United Kingdom. Recent events, the memory of which will long outlive the deep feeling of national humiliation now experienced at the failure of the public administration, in departments where it was perilous to permit the existence of incapacity, have shown, that the corruption and nepotism of the past must be swept away with a firm, unwavering hand. The right to serve their country belongs to all her sons alike, and the country herself is entitled to the services of those best qualified to administer with credit and efficiency the duties entrusted to them. It is wrong in principle that any favour should have to be asked on such occasions. It is a sacred and a public right, which public opinion should, by all constitutional means, enforce, from the highest to the lowest offices of the State.

Purely personal and household appointments should, as at present, for obvious reasons, be matters of personal selection. Gold, silver, and all other varieties of sticks, may retain the *status quo ante*, without much fear of damage to the national reputation. But there the line should be drawn; and from the First Minister of the Crown to the lowest public servant in the most distant colonial possession of the country, the fittest men should be selected for the various offices, if there be any

means of gauging intellectual and general capacity, and of making such selection.

This is a great political problem, which appears likely to be partially solved at present, and which it is neither our province nor our wish to investigate.

With respect to the medical branch of the army, the solution has already been furnished, by what has been done in the case of the Indian civil and medical services. The magnitude and importance of the services performed by the officers of those branches of Indian administration, range from the government of vast provinces to the minutest ministerial details connected with their different offices, and their duties are inferior in gravity and responsibility to those of no other public servants in the civilized world.

In France, again, the medical staff is, like all other scientific branches of the army, recruited by *concours* from the entire body of professional men in the country who are willing to enter the military service. There is no lack of candidates and competence. The officers have an interest in the well-being of their department, and the State is well served in consequence.

The question of promotion is a more difficult one, but even here the difficulties are by no means insuperable. In France three elements enter into the question—*concours*, seniority, and selection ; but their system is faulty, and deservedly censured by their own best officers, inasmuch as the real selection rests with a mixed tribunal, in which the professional element is secondary. There can be no doubt that professional merit and competency can only be really ascertained by professional men, and that the introduction of an exoteric element, of high authority, with no inherent means or capacity of forming a right judgment, is fatal to the integrity of any scheme of promotion so framed. The *concours* applies only to the lower offices in the hierarchy, and to the professorships in the military medical schools.

The worst of all claims, when unaccompanied by capacity and competence, is that of seniority ; yet, when combined with those essential elements of advancement, it would be unwise, illiberal and unjust, to disregard it. It should, in all cases, be secondary to merit where administrative duties, requiring more than the average of professional attainments and mental and bodily vigour, are in question. In mere executive duties, such as fall to the lot of the regimental surgeon, the point is probably not quite so essential, yet even here it is lamentable to think of the amount of preventable misery and mortality, entailed by the appointment of an ignorant and incompetent surgeon.

The bane and curse of the Indian medical department has been the seniority system, which still flourishes there in all its vitality and vigour, as will be shown more in detail hereafter.

The least objectionable system at present in operation, with which we are acquainted, is that of the Belgian army, as promulgated in the 'Loi sur l'admission et l'avancement dans le Service Sanitaire de l'Armée et de la Marine,' in March, 1847. It is as follows:

"ART. 4. L'avancement aux grades de médecin de bataillon de deuxième et de première classe, et de médecin de régiment, aura lieu, moitié au choix, moitié à

l'ancienneté, à moins l'insuffisance de sujets capables parmi les plus anciens du grade immédiatement inférieur.

"L'aptitude des candidats sera constatée, pour les médecins-adjoints, par des rapports périodiques des chefs de service, et par les inspections générales; pour les médecins de bataillon par un examen.

"Ces rapports et ces examens ne porteront que sur des connaissances pratiques. Ils auront, en outre, pour objet, en ce qui concerne les médecins-adjoints, l'hygiène militaire et l'administration pharmaceutique."

The choice of the superior officers rests with the King, and in so small an army as that of Belgium, where the personal character and qualifications of every officer of any rank and standing must be well known, unfit instruments are not likely to be selected by the Sovereign, particularly when he possesses the most ample means of knowing all the men of real mark and merit, from the examinations passed in rising from the inferior ranks.

Some system of promotion by examination, to regimental surgeoncies, was apparently tried, and failed, in the British army a few years since. We are too imperfectly acquainted with the matter to be able to assign a reason for the failure; but, if it were properly conducted, upon so sound and rational a basis as that of the Belgian system noted at foot,* there is no more reason why it should not have succeeded, than that the French army before Sebastopol should be better fed, clothed, and organized, than the British force in the same position. The resolution to overcome the difficulty would soon cause it to disappear.

We cannot help thinking that the perfection of the regimental system of the English army has been considerably over-estimated, and that it does not sufficiently prepare the officers and men for acting in large bodies with the precision and unity necessary for complete success. It has, doubtless, many and great advantages in the *esprit de corps* which it fosters, and the close personal acquaintance and companionship that it maintains between the men and their officers; but it has, like our imperfect military system generally, a tendency to narrow and contract the views of all concerned. It is like all working in circles. The attention to minute micro-

* MESURES D'EXECUTION DE LA LOI D'ORGANIZATION DU SERVICE DE SANTE
RELATIVES AUX EXAMENS.

(Arrêté Royal du 20 Mai, 1847.)

LEOPOLD, &c.

Vu la loi du 10 Mars, 1847, relative au rang et au mode d'admission et d'avancement des officiers du service de santé de l'armée et de la marine;

Considérant qu'il est nécessaire d'arrêter les mesures d'exécution qui se rattachent aux examens auxquels les officiers de santé de divers grades et catégories sont soumis;

ART. 1. Nul ne sera admis à subir l'examen d'un grade supérieur, qu'après avoir servi pendant deux ans dans le grade immédiatement inférieur.

ART. 2. Les examens auront lieu à Bruxelles, une fois par an, à l'époque qui sera fixée par notre ministre de la guerre. Ils seront annoncés trois mois d'avance.

ART. 3. Chacune des commissions d'examen sera composée de trois ou de cinq membres, désignés par nous.

ART. 4. Les questions à résoudre, les sujets à traiter et les opérations à pratiquer ou à exécuter seront désignés par la voie du sort, conformément au programme à arrêter par notre ministre de la guerre.

ART. 5. La commission constatera l'aptitude ou l'inhabileté de chacun des concurrents à occuper le grade pour lequel l'examen a lieu. Elle réglera le rang de ceux qui auront été jugés aptes. Ses décisions seront prises à la majorité des voix, au scrutin secret, après délibération et discussion.

ART. 6. Les procès-verbaux des opérations des commissions d'examen seront inscrits dans

scopical details is unfavourable to the more enlarged views and the general laws which regulate the actions of mankind in the mass :

“ If viewed according to the reason of things, the genuine military principle, and many of the current practices of the day, are in direct contradiction to each other.”*

With the purely military bearing of the question we have nothing to do, but it is of consequence in its relations to the medical affairs of the army. The assistant-surgeon on first joining the army, with the exception of the short period he may pass at Fort Pitt, has no opportunity of learning his duties in such manner as to fit him to encounter the emergencies, that will rise up in the path of his future career.

Among the indisputable advantages possessed by the French army is the existence of two classes of military medical schools of instruction.

un registre à ouvrir expressément pour cet objet et qui restera déposé dans les archives du département de la guerre.

ART. 7. Après chaque examen, il sera délivré par la commission à ceux qui y auront satisfait un bulletin qui en indiquera la date et le résultat, soit qu'ils l'aient passé d'une manière satisfaisante, avec distinction, ou avec grande distinction.

PROGRAMME DES EXAMENS.

LE MINISTRE DE LA GUERRE,—

Vu l'art. 4 de l'arrêté royal du 20 Mai, 1847, ainsi conçu : “ Les questions à résoudre, les sujets à traiter et les opérations à pratiquer ou à exécuter, seront désignés par la voie du sort, conformément au programme à arrêter par notre ministre de la guerre.”

ARRETE :

ART. 1. Les programmes des examens prescrits par la loi du 10 Mars, 1847, pour les officiers de santé de divers grades et catégories, sont fixés comme suit :

A. Pour le Grade de Médecin de Régiment.

Traitement de six malades pris parmi les fiévreux, blessés, ophthalmiques et vénériens :— rédaction des histoires des maladies et des détails nécroscopiques, s'il y a lieu ;— discussion, de vive voix ou par écrit, sur les questions que pourra soulever l'examen des malades :

Pratique de trois opérations chirurgicales sur le vivant ou sur le cadavre, après avoir exposé les motifs qui peuvent les nécessiter, décrit les divers méthodes et procédés, et motivé la préférence donnée au mode opératoire adopté.

Application de deux appareils au moins, et appréciation de vive voix du mérite respectif du mode de déligation préféré.

Visite de deux hommes au moins pour s'assurer s'ils sont propres au service, et, en cas d'affirmative, à quelles armes ils conviennent spécialement.

Visite de deux militaires au moins, atteints d'infirmités ou de maladies chroniques, à l'effet de constater les causes patentes ou possibles de leurs affections, leur symptômes distinctifs, les chances probables de guérison complète ou incomplète ; rédaction, s'il y a lieu, des certificats détaillés exigés pour leur libération du service.

Solution par écrit, de deux questions d'hygiène militaire, se rapportant spécialement à l'emplacement, l'établissement et l'aménagement des casernes, des hôpitaux, des prisons, des campements.

Appréciation des qualités des denrées alimentaires à l'usage du soldat, aidée au besoin de l'analyse chimique.

B. Pour le Grade de Pharmacien de Deuxième Classe.

Deux préparations, l'une chimique, l'autre pharmaceutique :— exposé, de vive voix ou par écrit, des phénomènes dont elles sont accompagnées.

Deux analyses qualitatives.

Solution, par écrit ou de vive voix, de trois questions au moins sur les instructions relatives au service et à la compatibilité pharmaceutiques.

C. Pour le Grade de Pharmacien de Première Classe.

Deux préparations chimiques et l'exposé, par écrit ou de vive voix, des phénomènes qui les accompagnent.

Deux analyses quantitatives.

* Jackson, op. cit., p. 356.

The elementary schools of the second degree are called military hospitals of instruction, and there are three of them, placed respectively at Strasbourg, Metz, and Lille. The school of the first degree has received the name of the hospital of perfectioning, or finishing, and is the celebrated military hospital of the Val de Grace, in Paris.

The professors in these schools are all military surgeons, selected by *concours*. The details of their structure and management are well described in M. Bégin's valuable essay 'On the Military Medical Service in France.' In that military country, now our fast and firm ally, bound by ties of common danger, and baptized in the blood of the battle-field, it is recognised as an axiom, that "the degree of perfection of military medical science, is the true measure of the importance attached to the preservation of the soldier."

The Val de Grace, with its splendid museum, its valuable library, its commodious laboratories, theatres, and dissecting rooms, and its perfect commissariat and pharmaceutical arrangements, contains within itself the means of completing the professional education of the young military surgeons, in such manner as cannot fail to be of the utmost service to them in their after career.

A similar institution could easily be organized in England, and through it all candidates who have been selected for appointments in the army should pass, so as in no case to be drafted to the independent duties of their profession, until the strictest examinations, held periodically, had tested their fitness. Such an institution would be to military surgeons what camps of exercise are to their brethren in arms. Civil hospitals and ordinary courses of medical instruction cannot supply its place, for reasons too obvious to be discussed in these remarks.

The existing regimental hospitals, at home, at least, afford no such opportunities for all branches—professional, pharmaceutical, and commissariat—to become practically acquainted with their functions, so as to fit them for field service.

It only remains for us now to say a few words upon the organization of the department as at present constituted, which, although superior to the order of things which it succeeded, is not what the medical profession, as a scientific branch of the army, has a right to expect.

The promotion is too slow to secure men of vigour and ability for the higher offices of the department, and the proportion of field officers is so glaringly disproportionate to the strength and wants of the service, as to place it in painful and injurious contrast with the other scientific branches.

The expensive professional training of a medical officer is as an outlay of capital, more than equivalent to the purchase of a company; and the later age at which he, in consequence, enters the service, entitles him to much earlier promotion than he now obtains to place him on a par with his brother-officers. Sir George Ballingall has recently pointed out the necessity of providing such encouragement for the retirement of those who are no longer fit for active duty, as may secure for service active men, and prevent the accumulation on the muster-rolls of old men who are unfit for employment in the operations of war. It has been remarked by more than one writer from the camp at Sebastopol, that there is not a grey head to be seen in the French camp!

In the Belgian army, the preliminary studies of a medical graduate admitted to the army are reckoned as equivalent to six years of service. An assistant-surgeon can be promoted to a battalion surgeoncy of the second class after two years' service. A battalion surgeon of the second class, after two years' service in that rank, can be promoted to a battalion surgeoncy of the first class. Again, in two years, the battalion surgeon of the first class may become a regimental surgeon, and he in turn may become a garrison surgeon in four years. The garrison surgeon, after three years' service as such, may obtain the grade of "médecin principal." The "médecin principal" may become "médecin-en-chef" in two years, and the latter inspector-general in two years more. It is true that the numbers given are the minimum allowed, yet, it is equally certain, that an officer of energy and ability may rise to the head of his department in seventeen years, or before he is forty years of age.

The military rank of the Belgian medical officers is the following:

Inspector-general ranks with a	Major-general in the army.
The médecin-en-chef (chief physician)	Colonel.
The médecins principaux (principal physicians)	Lieut.-colonels.
Garrison surgeons	Majors.
Regimental surgeons	Captains of the first class.
Battalion surgeons of the first class	Captains of the second class.
Battalion surgeons of the second class	Lieutenants.
Assistant-surgeons	{ Sub-lieutenants. Ensigns.

In this scale there is something like an attempt to assimilate the medical to the military service. The head of the medical department is the inspector-general, and he is, in all respects, treated as a major-general.

In the French army, a student in medicine is eligible for the commission of assistant-surgeon of the second class on producing proof of being a born or naturalized Frenchman; of having fulfilled the law of recruiting; of possessing the title of doctor of medicine; of producing from the dean of the faculty in which he has graduated, a certificate of the manner in which he has obtained his degree, and of having obtained the remark *satisfactory*; of morality and good conduct during his pupilage; of not being more than twenty-six years of age; of passing satisfactorily a special examination; and of labouring under no physical infirmity to unfit him for service.

Having fulfilled all the above conditions, the assistant-surgeon of the second class must serve for two years before he can become an assistant-surgeon of the first class. The first class assistant-surgeon must serve as such for two years before he can become a surgeon (major) of the second class; and the latter requires at least four years' service in that rank before he can be promoted to a surgeoncy of the first class. The first class surgeon must serve for three years to become a principal (principal medical officer) of the second class, and the principal of the second class for at least two additional years ere he can attain the rank of principal of the first class. A principal of the first class is eligible for an inspectorship after three years' service in his existing rank. The period of promotion by length of service may be diminished *by one-half in time of war, or by service in the colonies*. The only other means of dispensing with the regulations above

related is—1st, For an act of devotion and courage, duly certified and published in the army or division order of the day ; and 2ndly, When it is not possible otherwise to fill up vacancies. Thus, in time of peace, a French medical officer of ability, experience, and acquirements, may mount to the highest round of the ladder of preferment in sixteen years; and in time of war, in half that space. Distinguished bravery and devotion may procure his immediate advancement.

Is it a wonder that, under such encouragement, French hospitals and ambulances are models of imitation, and that the French army is able to transport 2000 British sick in comfortable panniers, while our ambulances are “nowhere”?

The comparative scale of ranks in the French army is as follows—viz. :

1. Inspector-general	General of brigade.
2. Principal inspector	Colonel.
3. Principal	Lieutenant-colonel.
4. Major (surgeon)	{ first class } { second class } (Major in the English army.)
5. Senior assistant-surgeons (aides-majors.)	
6. Sub-assistant surgeons	Captains.
7. Pupillary sub-assistants (élèves sous-aides)	Lieutenants.
	Sub-lieutenants.

Here, again, although some of the classes have no existence in the English army, the advantage is in favour of our neighbours, and there is something like a military scale in the disposition.

In the English army, the following is the scale:

Director-general	Brigadier.
Inspector-general	Colonel.
Deputy inspector-general	Lieutenant-colonel.
Staff surgeons of the first class	Majors.
Regimental surgeons and staff surgeons, second class	Captains.
Assistant-surgeons	Lieutenants.

The greatest objections to this scale are, that the lower ranks are altogether out of proportion to the higher; that the rank itself is a sham, and not a reality; that the passage from one class to another is too slow and indeterminate, and that promotion is based upon no principle which secures a deserving officer from neglect or supercession; and that it is not calculated to place men in the prime of life and energy in the highest offices of the hierarchy.

In all matters relating to military honours, funerals, quarters, prize money, passage in transports, and uniform, there should be no class distinctions drawn, as there too frequently are, in favour of the general and against the medical staff. Their own pensions and those of their widows should be assimilated to those of their brother officers of corresponding rank and standing.

In a word, the medical should be treated as a substantive branch of the army, and placed, as far as circumstances will permit, upon the same footing as the engineer corps.

To sum up, in a few sentences, the result of the arguments imperfectly developed in the foregoing pages:

1. Without in any degree diminishing its subordination in the military corps in all field and strategic operations, and in regiments, the medical department should possess the power of managing its own affairs, independent of external interference.

2. It should possess within itself, and liable to no deviation from its own peculiar functions, its staff of apothecaries, purveyors, dispensers, and assistants of all classes, down to the lowest functionary necessary in a hospital.

3. Its ambulances, stores, and appliances of all descriptions, should be under its own control and management.

4. Its rank in the military hierarchy should be distinct, defined, and substantive; it should possess its proportionate share of field and superior officers; it should participate in military honours in the same manner as all other branches; its dress should not remain ridiculous and unsuitable; and in all regulations regarding pensions, and allowances of every description, it should be fully on a par with all other branches of the army.

5. Its promotion should be so regulated as to secure rapid preferment for the energetic and deserving, and a comfortable retreat, after a fixed period of service, for those who wish to retire or are unfit for active duty in the field.

Having thus, however imperfectly, attempted to place in a clear point of view the defects and just requirements of the medical department of her Majesty's army, we will now proceed to that of the East India Company.

The medical department of the three Presidencies of India are similarly constituted, upon an antiquated principle, quite unsuited to the wants and to the development of the real usefulness of the profession. They consist of senior surgeons, surgeons, and assistant-surgeons, with a subordinate staff for the minor duties of civil and military hospitals. They are all essentially seniority services, and although attempts have been made at various times to introduce a principle of selection for the higher staff appointments, they have invariably failed to attain the desired end. It is lamentable to know that this has resulted more from narrow professional jealousy, than from any indisposition on the part of the Government to fulfil faithfully the good intentions of the authorities in Europe. But more of this anon.

The senior surgeons are all officers of thirty years' standing and upwards, and rank as majors in the army. The three first on the list in general constitute the medical board, and are designated respectively physician-general, surgeon-general, and inspector-general—titles of no meaning with reference to the duties they have to perform. They are classed comparatively as brigadier-generals, a grade which has no real existence, as a substantive rank, in the army. Their duty is to receive all returns and reports, to regulate the medical affairs of the Presidency, and to act as the professional advisers of the Government in all things pertaining to the medical department generally.

It is no libel upon the conscript fathers to state what is patent to and acknowledged by all in India who know anything of the subject—viz., that for all purposes of usefulness they are absolutely and essentially impotent. They attain their position by sheer longevity, without the

smallest reference to capacity or fitness; and, in many instances, have been notoriously inefficient from the very commencement of their career. They become the senate of the medical corporation when, in the course of nature, the mental and bodily faculties are fast declining, and the judgment becomes as obtuse as the senses. Even if, in their brightest days, they were the most active, intelligent, capable, gifted, and learned members of their class, they would be unsuited for active administrative duties at the period of life when they usually mount upon the medical throne.

The truth is, that the instances in which the really distinguished members of the profession have either lived, or remained in the service long enough to become members of the board, are rare, although that position has been, and is, *occasionally* occupied by some of the most efficient officers in the department. They are, however, *rari nantes in gurgite vasto*.

The men of mark, and of high intellectual development, are almost invariably the earliest to suffer the inevitable penalty of decay and decline. It is so in all professions, and the medical calling is no exception to the rule. The easy, quiet, good-natured man of moderate capacity, has, in all circumstances, the best chance of outliving his mercurial, hard-working contemporary. The result is, that the nominal heads of the service, however amiable and excellent as individuals—and many of them have been, and are, most worthy, estimable men—are not so respected in their public capacity as to command the confidence of their rulers, or of those serving under them. The patronage of the department is not in their hands; they are seldom, if ever, consulted upon any question of importance; their advice and recommendations are set aside, without scruple or delicacy; they have no means of controlling or encouraging professional exertions; and are, in every sense, a detriment to their branch of the army.

If the constitution of the board be, as few will now-a-days dispute, radically defective, and mischievous in point of policy, it is still less defensible as a means of rewarding long and faithful services. The old and tried servant is deserving of every consideration from the State, to which the best years of his life have been honestly and conscientiously devoted. It is in the ease and dignity of retirement that the recompence of a well-spent life of usefulness should consist, and not in the performance of duties, for the efficient discharge of which the greatest mental and bodily vigour is needed. To place an old man in such a position is as cruel to him, as it is injurious to the public interest.

The executive officer of each of the boards is a secretary, by whom the current duties of the office are performed, and from whom any efficiency possessed or exhibited by the board, really emanates. The character of this functionary generally determines that of the body, whose right-hand and mouthpiece he is. The secretaries have generally been able and efficient officers, but for whose exertions the venerable and effete institutions would long since have been consigned to the oblivion which has gradually overtaken them in Europe. They have little real power of doing good, experience considerable difficulty in reconciling the crotchety differences of their chiefs, and are too much absorbed in current details of office, to be able to produce any work that would tend to elevate or advance their profession.

The general inspection of hospitals in the different circles of command into which each Presidency is divided, is performed by the senior surgeons next below the members of the medical board on the list. They are designated superintending surgeons, and rank as lieutenant-colonels. They are invariably appointed in the order of seniority, and seldom attain their rank before they have served for thirty years. In Bengal, the Punjaub, and Burmah, there are 11 superintending surgeons; for Madras, 10; and for Bombay, including Scinde, 5. The correspondence regarding all professional matters in their respective divisions passes through them, and they are supposed to visit and inspect the various hospitals in their circles, as often as practicable. The professional supervision so exercised is usually an utter farce, for it rarely happens that the senior officer is even equal in attainments to most of those serving under him. Being selected by seniority, without the smallest reference to qualification, their interference, as a general rule, would, in the majority of instances, be more mischievous than useful. Even physical inability to discharge their duties has not been a barrier to their promotion, and the blind, deaf, and infirm have been known to have held an office, which, in any other army in the world, would have been bestowed only on the most active, deserving, and efficient members of the corps. The intention of the home Government has always been to make this important staff office essentially one of selection, seniority only constituting a preferential claim, and being held subordinate to capacity and qualification. This just and wise measure has, heretofore, proved a dead letter, chiefly from causes little creditable to the profession itself. The supreme Government, wisely acting upon the maxim that, in purely professional matters, professional men alone are competent judges, has, on one or two recent occasions, been reputed to have consulted one of the medical boards as to the propriety of promoting officers, generally believed to be useless and inefficient, to higher offices in the department. The conscript fathers so consulted are believed to have been destitute of the courage necessary to give an honest opinion upon the subject, and the result was the elevation of men notoriously unequal, from age and infirmity, to discharge the most ordinary duties of their offices.

The clamour raised in the very few cases in which the Government has departed from the rule of seniority, appears to have deterred the ruling authorities from giving full effect to a principle of selection that was intended to reward merit and efficiency, with due regard to the claims of mere length of service. With a single capable, responsible head of the medical service, possessing the confidence of the Government, and proof against the idle clamour of the drones and incapables, such discreditable derelictions of duty could not have occurred.

Between the superintending and assistant surgeons there is no intermediate substantive rank, similar to that of staff surgeon of the first class in the Royal army. All are surgeons, ranking as captains, until they have served for thirty years, and then classed as majors, without any additional pay, emolument, or consideration of any kind. The average interval thus passed is generally not less than fifteen of the best years of a surgeon's life in India, and it is during this period that the majority of the most efficient retire. Some inducement is necessary to tempt men

in the prime of life and usefulness to remain in the service. Few are inclined to bide their time for promotion, until they are unfit for active exertion; hence the greater number leave at the very time their experience renders their services most valuable. This very detrimental exodus might easily be prevented, without much cost, and with great benefit to the State.

The assistant-surgeon, shortly after his arrival in Bengal, after being supposed to have picked up some scraps of tropical pathology and therapeutics at the General Hospital, is either appointed to do duty with a European regiment, attached to a native corps, or sent to a civil station. He requires to pass an examination in colloquial Hindustani, before he can draw any staff allowance.

With European corps, in which, from the habits of the soldiery and other causes, much sickness usually prevails, he has a good opportunity of becoming acquainted with tropical disease, and of the mode of treatment adopted by men of experience. In native regiments, the practice is usually unsatisfactory, and little professional skill can be attained. In civil stations, the surgeon is generally thrown entirely upon his own mental resources, has charge of a jail and dispensary, attends the European and native officials and their families, and in many stations may obtain a lucrative practice among the neighbouring planters. If possessed of ability and energy, his opportunities of obtaining professional experience are considerable, particularly in operative surgery. Some assistant-surgeons hold lucrative appointments; one is now surgeon to the Governor-General, upon a salary of 1440*l.* a-year; another obtained the office of residency surgeon at Lucknow as a reward for distinguished service in Burmah, and his emoluments cannot be far short of 2000*l.* a-year. None of these appointments are much less than 400*l.* a-year, and some range between this and 1200*l.* annually. Some are professors in the medical college, and in the enjoyment of lucrative private practice in addition.

In Madras, the assistant-surgeons really undergo a sort of professional probation before they are entrusted with independent charges, and in other respects are very much in the same position as in Bengal, except that the prizes are neither so numerous, nor so valuable.

The only distinction in the Bombay service is that most of the assistant-surgeons serve for two years in the Indian navy.

While we have freely animadverted on the senior appointments of the service, we are glad to acknowledge that, upon the whole, there is no country in the world in which the younger members of the profession enjoy greater opportunities of advancement, by ability and exertion, than they do in India. Real merit is seldom overlooked, and the paths in which they may seek reputation are many and varied.

The charge of regiments of irregular cavalry, detached troops of horse artillery, contingent corps of all arms, sanitary stations, and assistant opium agencies, assay offices, all of which are more or less considered as prizes, fall to them.

Their advantages over their brethren in the Queen's army are many, and in no particular more than in the independent action which they attain at an early age. For the full development of the character, and usefulness, nothing can be of greater service to an individual with a well-trained mind, habits of industry, and the determination to succeed, which is so often the key to success.

The interval of promotion in the Indian army is much too long. It averages from ten years—a very rare event—to fifteen or more, so that an officer may thus be nearly forty years of age before he attains the rank of captain, as many assistant-surgeons do not enter the service before they are twenty-four or twenty-five years of age. We have seen that before that time he may have attained the rank of a general of brigade in the French service.

The surgeons of all the three armies are in very nearly the same position as regards pay and other advantages; but, as usual, the greatest number of prizes is in Bengal. Six civil surgeoncies of large cities, all of them possessing some private practice, in addition to four garrison surgeoncies, the office of apothecary, presidency surgeoncies, and a marine surgeoncy, are theirs. There is also a surgeoncy of the general hospital in Calcutta, at present held by the patriarch of the service, and one of the most distinguished members of the profession who ever went to India; but he has long been incapable of performing any duty.

In Madras there are district surgeoncies, and other appointments of some value; and Bombay has a fair quota of good things for its full surgeons.

The value of few surgeoncies is below 800*l.* a year, and with the emoluments of private practice at the Presidency, as the capital is called, some are worth from 3000*l.* to 5000*l.* annually.

In the purely military branches, the most valuable surgeoncies are those of the brigades of horse artillery, European regiments, and light cavalry corps.

The office of superintendent of the Botanical Garden in Calcutta is held by a full surgeon, upon a consolidated salary of 1800*l.* a-year, with a handsome house rent free, and a princely establishment. For this he has, in addition, to perform the duties of Professor of Botany in the Medical College.

The garden at Saharunpore, with the tea plantations in the Dhera Dhoon, is superintended by another surgeon, upon a lower scale of remuneration; it is, nevertheless, a valuable appointment.

The superintendent of teak forests in Burmah is a third surgeon, who enjoys an income of about 1200*l.* a-year, with travelling allowances. In the Madras and Bombay Presidencies are gardens under the care of surgeons on those establishments.

From the above it will be perceived that the prizes thrown open to the profession by the competition for medical appointments in the Indian service, are neither few nor unimportant.

As we are writing for readers in Europe, where little accurate information in matters relating to India exists, we make no apology for introducing at length the following quotation from a pamphlet which has recently appeared in Calcutta.* It is entitled 'Notes on the Condition

* As we have not Dr. Macpherson's pamphlet at hand to refer to, our extract is taken from a paper in No. 45 of the Calcutta Review, entitled, "Surgeons in India: Past and Present," written in an excellent spirit, by one evidently well acquainted with the subject. With all its trials and privations, its exile and banishment, and the difficulties attendant upon scientific research in a tropical climate, there is much to render an Indian home desirable. There is nothing of the struggle with poverty associated with its early trials, that destroys so many promising aspirants at home; and for those who survive the chequered chances of an Indian career, there is always a handsome provision for an old age of honourable retirement.

of the Indian Medical Services,' and is written by Dr. John Macpherson, the acting Secretary to the Medical Board; its accuracy may, therefore, be relied on.

"The medical service of Bombay consists of thirty-five surgeons and one hundred and five assistant surgeons, making a total of one hundred and fifty-eight, with usually a certain number of supernumeraries, never exceeding twenty in number, and generally falling short of that number. It may be said in a general way, that about seventy officers are employed with the army, or in the Indian navy, about fifty are on staff or civil employment, and the large number of thirty-two on leave or furlough, almost all of them on sick certificate.

"The zillah or civil stations appear to be about sixteen in number, and their pay seems to be much the same as that of similar appointments in Bengal, 360 rupees a month, sometimes with an additional hundred for the charge of civil or insane hospitals, or the same sum for a duty which is never assigned to them in Bengal—that of assistant magistrates; and which, since commencing this article, we find has been withdrawn from all assistant-surgeons; they appear to be scarcely ever post-masters or registrars of deeds. There are also in civil or district employ, four superintending vaccinators, each receiving 350 rupees a month, besides their military pay. . . .

"The proportion of staff appointments at the Presidency itself, is, as compared with the other Presidencies, large: including the members of the medical board, they amount to about twenty, or almost as many as in the much larger Presidency of Bengal. There are three members of the board, and its secretary, a superintending surgeon, five professors of the medical college, a store-keeper, a surgeon to the General Hospital, and an assistant, a garrison surgeon and assistant, a surgeon of the marine battalion, a civil surgeon and assistant, a police surgeon and assistant, port surgeon, &c., surgeon to the Jamsetjee Jeejeeboy Hospital, an oculist: generally one officer holds two or more of these appointments. . . .

"Most assistant-surgeons in the earlier part of their career are made to serve for two years in the Indian navy. If their stay be not too long protracted in it, there is very little hardship in this, as they have the opportunity of seeing, in fine vessels, a good deal of the coasts of India, Persia, Arabia, &c.; the pay is small, somewhat larger we believe than that for doing duty with a regiment; but as the temptations to extravagance, and even the opportunities of spending money, are few, it may be considered ample. . . .

"Staff appointments at the Presidency are as much sought after at Bombay as in Calcutta, and several of the officers at the Presidency enjoy pretty good incomes from practice, as also do one or two private practitioners. A good deal of the practice lies among the Parsees, who are, after the Europeans, the leading class, and certainly the most intelligent and enterprising of Orientals. They are, however, much in the habit of employing private practitioners. The best medical practice is not nearly so remunerative as in the larger city of Calcutta, nor even equal to Madras. . . .

"The Madras medical service consists of about seventy-two surgeons and one hundred and fifty-four assistant-surgeons, making a total of two hundred and twenty-six. Of these some eighty are on staff employ, ninety in regimental employ, and some fifty absent on sick or other leave.

"Of those on staff employ, about twenty-eight are zillah surgeons, ten employed in residencies. The pay of zillah surgeons is, we believe, the same as in Bengal and Bombay. We should suppose that civil surgeoncies, generally speaking, cannot be very remunerative. We have heard of Salem as a good civil station. The Neilgherries must yield a considerable income, and of the surgeoncies, Hyderabad is of course the best, rivalling Lucknow; the surgeoncy to the Mysore commission and some other residency surgeoncies, as that of Cochin, are comfortable appointments. There are ten officers in the Nizam's service, all well paid, five garrison surgeons, no fewer than ten superintending surgeons, three members

of the medical board, and a secretary, the latter at present being an assistant-surgeon.

"There are at the Presidency fifteen medical officers, including the medical board, being eight surgeons and seven assistant-surgeons. Their duties are those of garrison surgeon, medical store-keeper, four district surgeons, superintendent Eye Infirmary, surgeon General Hospital, one permanent assistant, and one assistant-surgeon to it, and six chairs in the Medical College. . . .

"Assistant-surgeons in Madras are, on their first arrival, made to do duty at the General Hospital, and keep case-books, until they are reported duly qualified for the general duties of the army. . . .

"The Bengal medical service consists of a hundred and twenty-nine surgeons and two hundred and thirty assistant-surgeons, making a total of three hundred and fifty-nine. There are supposed to be a certain number of supernumeraries attached, but this is by no means always the case. The service may be said generally to be divided into two hundred employed in purely regimental duty (including irregular cavalry and local corps), about a hundred and twenty on civil or staff employ, and forty on furlough or leave, the proportion of the latter being much smaller than in Madras, and little more than one-third that of Bombay,—a very remarkable fact! We may here remark that it is a subject of some just complaint in Bengal, where promotion is so slow, that a surgeon of thirty years' service, when on furlough, draws no higher pay than one just promoted, but this is also the case with the captain who is unlucky in his promotion.

"Some of the chief civil and staff appointments, besides the eleven superintending surgeons and the members of the board, are the following: viz., upwards of fifty civil stations in Bengal and the north-west; of these only six are assigned to full surgeons, namely, Delhi, Agra, Benares, Patna, Dacca, Berhampore, and are all more or less sought after. Of the appointments in the north-west the most lucrative for assistant-surgeons are Bareilly, and the civil surgeoncies of Simla and Mussoorie: the two last only held for a period of two years. In Bengal there are many very excellent civil appointments, supposed to vary in value from 700*l.* to 1100*l.* a-year, as Kishnaghur, Howrah, Jessore, Tirhoot, Bhaugulpore, Chuprah, Hooghly, Ghazeepore, &c. The registry of deeds is in some of the cases the most valuable addition to the appointment, while in Kishnaghur the ferry gives a handsome return, but in all these cases the value of the appointment depends much on the popularity of the civil surgeon with the station and the neighbouring planters and landholders. In Bengal and the north-west the civil surgeons very generally hold the post-office, and are also often registers of deeds. But under the new changes they are to be deprived of the post-offices, and the civilians always endeavour to get hold of the registries for themselves.

"Of political appointments, strictly speaking, only two are now held by members of the medical service, namely, the charge of Darjeeling, and the custody of the young Maharaja. The two chief residency surgeoncies are excellent appointments, Lucknow being worth 1500 rupees a month to any one of common judgment, and Nagpore about 1200 rupees. The superintending surgeoncy at Gwalior is a desirable appointment, as indeed must the charges in the Gwalior contingent generally be considered. The opium examinerships at Ghazeepore and Patna are excellent appointments; that at Indore is now held by a Bombay assistant-surgeon. Two mint appointments, one in Bombay and one in Calcutta, are held by Bengal surgeons, as well as the charge of the botanical gardens in Calcutta and Saharunpore; one of the examinerships of the College of Fort William is also held by a medical man.

"Including the medical board, there are twenty-five members of the medical service performing medical duties at the Presidency, and this is including the surgeon to the Governor-General and to the body-guard, who cannot be looked on as fixtures. In the last ten years the offices of second garrison-assistant, deputy apothecary, and marine assistant-surgeon, have been abolished, and one Presidency surgeoncy absorbed in the marine surgeoncy. Some of the appointments at the

Presidency are the medical store-keeper or apothecary, the garrison surgeon and assistant, the surgeon to the General Hospital and his two assistants, the marine surgeon, the oculist, six or seven professorships in the Medical College, and five Presidency surgeoncies.

A peculiar feature of the Indian medical services, in which they bear some resemblance to the medical department of the French army, is that they furnish the instructive staff for four medical schools—viz., the Colleges of Bengal, Madras, and Bombay, and a school of more restricted dimensions established in the Nizam's territory. The former are highly-organized institutions, have been eminently successful in their results, and are justly held in high esteem by the Government and the public. They have done much to overcome prejudices of caste, have proved that the natives of India are susceptible of a high degree of moral and intellectual training, and in their systems of examination for diplomas are far in advance of all examining bodies in Great Britain, with the exception of the University of London. The practical tests, in particular, are so extended and severe, as to render it impossible for men to be sent abroad to practice in the country, who have not been in diligent attendance upon hospitals and dissecting-rooms during their pupilage. Cramming and grinding are unknown in those schools.*

It would obviously be out of place in this paper to discuss the subject of the means adopted by the various bodies licensed in the United Kingdom, to ascertain the qualifications of those who present themselves for the degrees and diplomas necessary for the killing or curing of her Majesty's lieges, *secundum artem*. It is inseparably connected with the still more important matter of medical education itself—a subject of the greatest importance to the well-being of the profession.

It is sufficient for our purpose to declare our belief, that the possession of a degree or diploma from any examining body in Great Britain and Ireland, except perhaps the University of London, is not of itself a proof

* The subject of medical education in India is too important to be discussed as an incident in remarks devoted to another purpose. Should no one better qualified to do justice to the task undertake it, we shall esteem it a pleasure to introduce the matter to European readers at some future period. As a link in the chain of human progress—as an evidence of the earnest and honest manner in which the character of the profession is upheld in the far East—and as a type of the mode in which the civilization of the West is beginning to leaven the masses in India, it will, we doubt not, recommend itself to the sympathies of our brethren in Europe.

There is probably no department of the public service in India that has produced a greater proportion of distinguished men than the medical fraternity. To the patriotism of two of their number, Boughton and Hamilton, the foundation of the empire which is now the brightest jewel in the British Crown, may fairly be traced. The professional skill of the former, successfully exercised upon the daughter of Shah Jehan, procured the long-desired privilege of establishing trading factories in Bengal, free from taxation. When this privilege was on the point of expiring, from causes which are well known to all readers of the history of British dominion in the East, it was renewed by the surgical success of the latter upon the person of the Emperor Furmkshere. Both of these men died in the land of their adoption, an honour to their country and their profession.

To Oriental and general literature they have contributed some of the greatest names in the annals of the Eastern Empire; and there is no department of usefulness to which they have not contributed a fair quota of men of eminence and ability.

To one of their number India owes the successful introduction of the lightning which transmits intelligence with the swiftness of the passage of light. To others, working with equal zeal and ability in a still higher path of human exertion, is, in some degree, due the rapid progress of education, which, like a mighty and irresistible torrent, is sweeping away, in the current of knowledge and truth, the superstition of ages, and restoring to the East the light of civilization, with which she originally dispelled the Cimmerian darkness of the West.

of the fitness of the possessor to be entrusted with the care of the life and limb of the soldier, or of the communities in India who are entirely dependent upon their skill in the dread hour of disease and suffering. So long as such degrees and diplomas are procurable by mere book knowledge, or the objectionable system of preparation known under the term of "*grinding*," there can be no security against the introduction into the profession of men altogether unequal to the emergencies liable to arise in the course of practice.

This is of little comparative consequence in the civil practice of Europe, where the beneficial effects of competition generally correct any mischief that might result from entire ignorance or incapacity. The public are free to choose their medical attendants, and it is their own fault if they risk their lives in the hands of the unskilled. To be sure there may be some difficulty in the selection, for presumption and ignorance usually go hand in hand, and no man of character and capacity can venture to proclaim the infallibility to which empiricism pretends.

The soldier has no choice in his medical attendants, and must submit to whatever the Government provides for him. It is lamentable to reflect upon the amount of mischief that must ensue from incompetence in a military surgeon. Two examples, among several that have occurred within our own knowledge, may be mentioned in proof of the extreme care that should be exercised in such appointments.

A large amount of sickness and mortality prevailed in a European corps in India some years since, and the principal inspecting officer was directed by the Government to proceed to the station, to ascertain personally, the cause of so costly a loss to the State. When he arrived at his station, he proceeded at once to the quarters of the surgeon, whom he found dead in his chair, with the inspector's letter lying open on the table before him. He was unequal to the performance of the duties of his office, and ended his sorrows with a dose of morphia, to avoid the shame of exposure.

The other example happened some fifteen years since, in a small field-force, of which an assistant-surgeon was in medical charge. In the first encounter with the enemy, an artilleryman was so severely hurt as to require the immediate amputation of a limb. The surgeon entered his tent, ostensibly to procure his instruments; but, as he was rather long absent, an inquiry was instituted to know what had become of him. He was found lifeless. He had committed suicide to avoid the performance of an operation with which he was practically entirely unacquainted.

Such things are merely mentioned to impress the fact that more care is necessary in regard to the practical tests in the case of officers selected for service in India, than in ordinary cases.

The recent regulations which have thrown open the medical services of India to the profession at large, will, when fairly carried out, secure for Hindustan a similar quality of professional acquirement as the system of *concours* in France has secured for the Polytechnic School.

We look upon the institution of the principle of the *concours* on the large scale, now for the first time practically introduced in England, with the greatest interest, and are convinced that it is fraught with much that is of the most vital importance to the well-being of the profession at home

and abroad. At the same time, let us pay a tribute of gratitude to the East India Company, in whose hands this great patronage has existed for so many years, and by whom it has been exercised with a purity and honesty utterly unknown to the political distribution of patronage in England. That eminent body has been the kindest, most liberal, and considerate government in existence towards its servants; and its service is justly esteemed the best of the whole world by those who have passed their best days in it, and have seen how their less fortunate brethren are treated elsewhere.

All medical officers in the Indian armies are entitled to retire, without reference to the state of their health, at fixed periods, commencing from seventeen years' service and extending to thirty-five, the pension from the State varying from 200*l.* to 750*l.* a year, at the extremes of the scale.

In addition to this, they subscribe to funds somewhat similar in effect to the purchase of annuities for the remainder of their lives, and to which the Court of Directors contribute handsomely. In Bengal, this amounts to an addition of 300*l.* annually to the income of the retired surgeon, of which the subscriber must pay a minimum of its half value, as calculated upon his age at the time of retirement. It is now obtained after an average service of twenty-four years.

In Madras, the fund is administered with some differences of detail. At the end of seventeen years' service (subject, however, to the state of the fund), the medical officer is entitled to a certain allowance, amounting to 200*l.* per annum; and after a certain additional number of years, spent either in the service or not, he becomes entitled to a further increase of 200*l.* per annum. It is usually, we believe, some six or eight years before an officer who has received the smaller, becomes entitled to the larger, allowance. Occasionally, when there have been many claimants upon the fund, officers wait till they have put in twenty, or even twenty-two or twenty-three years' service, before they can obtain even the small allowance.

The retiring allowance from the Bombay fund is, we believe, administered on the same principles as that of Bengal, but we think its allowances are not quite so large.

For his widow and orphans, should the medical officer die before he has been able to provide for them, and even after his retirement from active service, a handsome provision is made, chiefly at his own expense, but again with seasonable help from his rulers.

In all these respects, how superior is his treatment to that of his compeers in the service of Her Majesty, and to those of any class of public functionaries on the Continent of Europe!

An invaluable appendage of the Indian army is the subordinate medical department attached to it. This, in Bengal, consists of the European establishment, and of a special class of subordinate agency for the native army, and for duty in civil hospitals appropriated to natives.

The European establishment consists of apothecaries, assistant-apothecaries, apprentices, stewards, and assistant-stewards.

The apothecaries are charged with the preparation and administration of medicines, the care of wounds, accidents, and injuries, during the

intervals of the visits of the surgeons, the admission of patients, and, in fact, are the general assistants of the medical officers in the performance of their professional duties in the field, in garrison, and in all the circumstances in which the troops are employed. It would be impossible to exaggerate the usefulness and importance of this excellent class of public servants. As a body, they are a credit to the service, and are of more real use, from their careful professional training, than any body of nurses could possibly be, to the sick and wounded.

They are usually the sons of soldiers, educated in the regimental schools, or in the Military Orphan School. They are admitted to the service after examination by special committees of medical officers—a *concours* upon a small scale—and after doing duty in regimental hospitals for two years, are (if in Bengal) transferred to the medical college in Calcutta for two additional years of training. There they are under strict military control; are instructed in anatomy, materia medica, medicine, and surgery; are carefully trained in hospital duties as clinical clerks; and, after undergoing a tolerably strict examination—in some particulars more severe than that of the College of Surgeons of England—are reported qualified. If they fail, are idle and insubordinate, and otherwise misconduct themselves, they are removed from the army, and forfeit all the advantages of their previous service.

In the recent Burmese campaign, and in the late Punjaub war, they were found most efficient field-assistants; and we are able, from personal knowledge, to state that some of them are more efficient members of the profession, and generally better informed, than some assistant-surgeons with whom we have come in contact, armed with degrees and diplomas from British schools of old and great pretensions.

Now that the rank of apothecary in the British army has, like that of the French service, been made that of a commissioned officer, we trust that the Court of Directors and Board of Control will extend a similar boon to the most deserving members of this invaluable class of public servants. Their length of service, their high personal character, and their qualifications, fairly entitle them to such a mark of consideration.

If so great an effort is made at home to reward the private soldier for distinguished conduct in the ranks, to raise him in the estimation of his country, and to encourage a better class of men to enter the army, there is no reason why the boon should be limited to any particular class of the soldiery.

We are acquainted with some members of the European medical subordinate department in India, who would confer greater honour upon a commission, than the parchment patent could possibly confer upon them. Had such a department, so organized and instructed, existed at the outbreak of hostilities with Russia, some of the most serious charges against the medical department, and most of the painful details which have harrowed the feelings of the public, would not have been heard of,—for they could, under such a system, have had no existence.

The stewards and their assistants are charged with all the details relating to the food, clothing, and similar interior economy of military hospitals. Both classes aid the surgeon in the preparation of official reports and statements.

These documents are much too cumbrous and complicated, employ a most unnecessary portion of the surgeon's attention, in times of severe sickness, in the field, and in all emergencies, and frequently convert him into a species of professional clerk. As checks, they are too complicated to be effective—as records, they are too voluminous for examination. A sensible sanitary commission, such as we hope, ere long, to see established in both armies, would soon reduce them to convenient working dimensions, and extract from them invaluable matter for future guidance. At present, they are *caviare* to every one connected with the medical services in India and in England.

In the native army, and in the ordinary civil hospitals of the country, there is a special class of indigenous subordinates, somewhat quaintly termed Native Doctors. They are instructed in the medical college, in their own vernacular language, in anatomy, materia medica, medicine, and surgery, taught by their own countrymen previously educated in the English class of the same college. They are not so highly qualified as the European subordinate class, but are nevertheless an extremely useful body of men.

Another feature peculiar to the Indian system is the existence of great store departments in the three Presidencies, whence issue all the medicines, instruments, and other necessities of military and civil hospitals. The establishment of such magazines was doubtless rendered necessary, in the first instance, by the great distance of the country from the central source of supply, the uncertainty and length of communications with the mother country, and the impossibility of procuring what was required on the spot.

In Bengal, the principal medical store-keeper is termed apothecary to the Company, and, under the authority of the medical board, regulates the issue of all medicines, instruments, and apparatus, and indents upon Europe for all future supplies. All indents are forwarded through the superintending surgeon of the division, by whom they are, in the first instance, checked and countersigned. They then pass through the medical board, and, after undergoing further scrutiny, are sent to the apothecary to supply. They are prepared and packed with the greatest care and attention, and, in cases of urgency, with the least possible delay.

All stores and instruments arriving from Europe are examined and passed by a committee, selected in rotation from the medical officers at the Presidency. All remarks made by this dispensary committee are forwarded to the medical board, and transmitted to Europe with the next succeeding annual indents, should they contain suggestions for improvement, or objections to the nature and quality of the medicines and instruments forwarded by the Court of Directors.

There are branch depôts in the North West Provinces, to save the time and trouble involved in references to Calcutta, and to enable the frontier stations to be supplied without delay.

When an army takes the field, a special store-keeper, generally an active assistant-surgeon, is appointed to take charge of the medicines and instruments for issue to field hospitals. In addition to the wants of each regiment about to proceed on service being fully supplied, a calculation is made of the probable requirements of the field hospitals, with a large

margin for contingencies. In this manner a force of 100,000 men can be supplied as readily as a division of 10,000; and it is no fault of the Government if there is any deficiency of the means and appliances required by the exigencies of any campaign, with its attendant casualties.

In Indian warfare, disease is, if possible, more destructive than in Europe, although even in the latter the loss by the sword, and from actual contact with the enemy, is seldom one-fourth of the entire loss sustained. In the Peninsula campaign, out of 40,000 dead, less than 10,000 fell in action or died of their wounds. Such exceptional cases as Walcheren, the retreat from Moscow, the Cabul disaster, and, unfortunately, the Crimean expedition, cannot enter into such a calculation, as they are wholesale destructions of life beyond the average of ordinary warfare. This exhibits most strongly the necessity for having magazines well supplied to meet urgent demands, as time is of the utmost consequence in such cases, and the early supply of medicines in sufficient quantities, may be the means of saving hundreds of valuable lives. It is unsafe to depend upon the market, or upon ordinary sources of supply, in time of war, for reasons too obvious to need any detailed demonstration.

Such is, we believe, a faithful outline of the constitution and principal peculiarities of the Indian Medical Service. We have purposely, in all our remarks, avoided minute details; our object being rather to present a general view of the subject, than to exhaust any portion of it.

In the present great crisis of public affairs, we deem it of the utmost importance to make known, as widely as possible, the state of our medical departments, that the remedies for the removal of the evils surrounding them may be applied rapidly and efficiently.

England has been taunted with the absence of the administrative ability necessary to organization, and to accomplish great ends with the means at her command. We can proudly point to India to disprove the assertion, and to show that the talent exists in abundance, if the right means are sought to turn it to advantage. There, the mightiest results have been accomplished in circumstances sometimes apparently as hopeless as the present paralysis of our arms in the Crimea; and when great disasters *have* occurred in India, they have been due to the incapacity of the chiefs entrusted with the guidance of affairs—in no instances to the unfitness of the instruments at their disposal.

And now, let us turn, in conclusion, to the means necessary to place the Indian medical establishments upon the most efficient footing, and to the rewards necessary to encourage first-rate men to enter the service of the East India Company, and to remain in it as long as they are capable of doing good service. To beat up for recruits by public competition, and then to consign them to oblivion when fairly caught, is neither prudent nor politic. If the means of fostering merit, rewarding acknowledged zeal, and rapidly advancing men of more than ordinary capacity, are not adopted, the system of competition will fail, and a secondary class of men only will enter the lists. The able and zealous are sure to succeed, wherever their lot is cast; and they will certainly not subject themselves to the chances of a contest which leads to no honour. Hence the present is, we believe, the most favourable juncture for a radical change in the present constitution of the medical departments of the Indian

army. It is manifestly more difficult to introduce fundamental changes into a service, the existing members of which have entered it upon an implied guarantee of necessarily rising by seniority, during good conduct, than into the Queen's army, where no such system prevails, beyond that which obtains in all services, viz., that length of service, combined with fitness, constitutes strong claims to advancement.

In the Indian army this really applies only to promotion to a surgeoncy, and to pensions for specified periods of service, which it would manifestly be a breach of faith to disturb. The higher offices in the hierarchy have long been declared by authority to be *staff appointments*, and, as such, to be selective. To make them so absolutely, will only be to reduce the written law to practice.

The first and most important change required is to place the department under a single responsible head, selected from the whole service, in direct communication with the Government, and possessed of authority to enforce obedience to all departmental instructions. At present not only is the medical service imperfectly represented, but its representations are distilled through other channels in which they may be, and frequently are, diluted and destroyed, before they reach the head of the Government. There are a thousand ways in which this operates injuriously.

With regard to the members of the service generally, the smallest boon that can be extended to them is to assimilate their state to that of the Queen's army, in the matter of establishing staff surgeoncies of the first class, and in the more rapid promotion of assistant surgeons. No officer should remain in that class, in any circumstance, longer than ten years; and, if possible, men of distinguished merit should be able, as in the French army, to run through all the ranks of the hierarchy in fifteen years.

The present injurious distinction in regard to dress should be abolished, and a departmental uniform adopted, which no man need be ashamed to wear. At present, we doubt if any medical officer would, if he could avoid it, ever appear in the full dress of his rank. What motive can have suggested the personal degradation of an entire class in such a matter, we have never been able to divine.

Every medical officer, particularly in the Indian army, should, in our opinion, be mounted. It would cost nothing additional to the State, and, in the field, would undoubtedly increase his usefulness.

The Indian army should be provided with a proper ambulance corps, organized and equipped in a manner suitable for service in Asia.

There should be established a sanitary commission to regulate and report upon all matters connected with the health of the soldier, and the health of the general community in India.

The important subjects of registration, statistics, vaccination, and, in fact, everything relating to public health, in the most extended sense, should be confided to its care. It should consist of at least three members, selected, without reference to rank and standing, from the whole service, should be liberally paid, and act directly under the head of the medical department.

The labours of such a commission would in a few years far more than repay its cost to the State, in the improved economy of hospitals, jails, and

similar institutions, and in devising the most suitable means of diminishing the preventable causes of disease, which the present advanced state of medicine, as a science, enables us to anticipate with confidence.

This is the merest sketch of the principle which should guide improvements in the Indian army. It would be easy to work out such a scheme in detail, were it advisable in the present instance to do so.

With regard to honours, if it be deemed not desirable to bestow an occasional baronetcy or knighthood on any officer of very superior and decided merit, upon the recommendation of the Governor-General of India, the honours of the Bath should be opened to the military surgeon for service in the field, as freely as to other branches of the army.

For distinguished services in the civil ranks of the profession, it would not be difficult to devise a suitable badge of distinction, upon the principle of the corresponding branch of the Legion of Honour in France. Ribbons and crosses are the cheap rewards of governments. "The reward of individual services," says a recent writer on this subject, "is an important part of the debt due by the State to its officers; but its importance does not end here. For one individual marked out for reward and distinction, we make a hundred others emulous of similar rewards, and eager, through the exercise of their talents, and the amount of their services, to deserve similar recompences."

REVIEW XIII.

Mode of Communication of Cholera. By JOHN SNOW, M.D.
Second Edition.—London, 1855. pp. 162.

NONE of our readers can be ignorant of the opinions of Dr. Snow on the communication of cholera by means of drinking water, nor of the perseverance and energy with which he has sought for facts to corroborate this view. The present work is a recapitulation of all the evidence he has hitherto published, with the addition of certain facts lately acquired.

We have read this work carefully, and shall endeavour, in the following critique upon it, to do full justice to Dr. Snow, while we shall strictly examine, as it is our duty to do, if there is anything hollow or unsound in the facts brought forward, or in the arguments founded upon them.

Dr. Snow believes not only that cholera is propagated by means of water, but that it is solely and exclusively so propagated. He is therefore obliged, at the very outset of his inquiry, to assume that cholera only spreads where human intercourse is possible. Thus he writes,

"It travels along the great tracks of human intercourse, never going faster than people travel, and generally much more slowly. In extending to a fresh island or continent, it always appears first at a sea-port. It never attacks the crews of ships going from a country free from cholera, to one where the disease is prevailing, till they have entered a port, or had intercourse with the shore. Its exact progress from town to town cannot always be traced; but it has never appeared except where there has been ample opportunity for it to be conveyed by human intercourse." (p. 2.)

We do not wish to argue the several clauses of this paragraph, but we do most decidedly protest against its reception, as a complete and final expression of the mode of spread of cholera. While we admit at once

that there are now several cases which show human intercourse to be occasionally influential in some way in transmitting cholera, we deny altogether that the phenomena, either of its rise or decline, can be always, or even frequently, so explained.

But our object now is not to discuss the general question of the contagion of cholera, but to see the strength of Dr. Snow's evidence on one particular presumed mode of propagation.

At page 10, Dr. Snow informs us that he was led to his view of the spread of cholera by a consideration of its pathology. He believes that the symptoms commence in the intestinal canal, and (apparently) that the disease is entirely local, and that neither the blood nor the nervous system are primarily affected. Having arrived at this opinion on grounds which appear to us insufficient to warrant so grave a conclusion, he writes as follows:*

"As cholera commences with an affection of the alimentary canal, and as we have seen that the blood is not under the influence of any poison in the early stages of this disease, *it follows that the morbid material producing cholera must be introduced into the alimentary canal*—must, in fact, be swallowed accidentally, for persons would not take it intentionally; and the increase of the morbid material, or cholera poison, must take place in the interior of the stomach and bowels." (p. 15.)

We cannot admit the cogency of the *must* in this quotation; since we do not see that it is satisfactorily made out that the blood is "not under the influence of a poison."

Dr. Snow then goes on with the following sentence, for which we can find no warrant in anything which has gone before; it appears to us to be a mere hypothesis.

"It would seem that the cholera poison, when reproduced in sufficient quantity, acts as an irritant on the surface of the stomach and intestines, or, what is still more probable, it withdraws fluid from the blood circulating in the capillaries, by a power analogous to that by which the epithelial cells of the various organs abstract the different secretions in the healthy body." (p. 15.)

Dr. Snow afterwards says, that there is sufficient "to prove the communication of cholera here explained, independently of the pathology of the disease." (p. 16.) We do not hesitate to say that the argument from pathology seems of little value, and it somewhat lessens our faith in Dr. Snow, to find that so doubtful a view, to say the least of it, should have suggested a special opinion of the propagation of the disease.

However, this is of little moment, and we proceed to the grand object of the work, and of this review—viz., the evidence for the propagation by water, and an examination of it.

From page 16 to page 22, Dr. Snow makes general reflections on the habits of the persons among whom cholera chiefly prevails, in order to show that their want of cleanliness would render it likely that portions of cholera evacuations might get mixed with their food or drinking water. We pass over this as not of great weight, and arrive, at page 22, at the first case in which direct evidence is sought to be given in favour of the hypothesis.

1. In two small adjacent courts in Horsleydown (Surrey-buildings and

* In this and in subsequent extracts we have italicized some passages which contain the pith of the sentence, or which appear to us illogical.

Trusscott's-court), cholera prevailed (in 1849) with very unequal severity, 11 fatal cases occurring in Surrey-buildings, and 2 cases (1 fatal) in Trusscott's-court. In all other respects similar (according to Dr. Snow), the only difference was in the water supply; the well supplying Surrey-court was contaminated by the slops and dirty water in which the clothes were washed being poured by the inhabitants into drains which communicated with the well; while the water of Trusscott's-court was free from this impregnation. A curious fact was, that all the fatal cases were in women and children.

Admitting Dr. Snow's hypothesis would explain all the other cases, we must account in some different way for the first case. The mode in which this is done shows Dr. Snow's perfect faith in his creed; instead of leaving the origin of the first case uncertain, he says the "two first cases were probably caused by the cholera evacuations contained in the Thames water, as it came from the waterworks." (p. 24.) Considering that this is the very point to be proved, we think the probability of this alleged mode is rather too easily admitted.

We visited these courts in 1849, not long after Dr. Snow's visit, and found that the number of the population of Trusscott's-court was very much below that of Surrey-buildings. We have not been able to find the note made on the point, but we distinctly remember that the disproportionate mortality in the two courts was in this way partly, though perhaps not altogether, explained.

We again visited these courts a few weeks ago, and found the differences between them to be as great as we had conceived them to be. Thus, Surrey-buildings consists of fifteen houses with four rooms each, while Trusscott's-court contains only ten houses with two rooms each. There are, therefore, sixty rooms in the first-named court to twenty in the second; and if we suppose the density of population to be two to each room, there would be 120 persons in Surrey-buildings to 40 in Trusscott's-court; and if the cholera had been equally severe in the two courts, it would of course have affected three persons in one, to one in the other. The disproportion is thus partly, but not altogether, removed. Trusscott's-court, however, with its small, low houses, is much more airy than its neighbour; it is close to an unoccupied piece of ground, of the benefit of which Surrey-buildings, facing the other way, and having no through ventilation, is deprived. We do not think that any one seeing these two courts, and knowing how much accumulation of persons in the same house tends to aggravate cholera, and how ventilation checks it, but would see in these circumstances an explanation, as satisfactory as that of contaminated water, of the difference in the mortality which remains unexplained by the difference in population.

2. The next case is that of Albion-terrace, Wandsworth, where, in 1849, cholera prevailed in a single row with considerable severity. Dr. Snow informs us that

"The water got contaminated by the contents of the house drains and cesspools. The cholera extended to nearly all the houses in which the water was thus tainted, and to no others.

"These houses were numbered from 1 to 17, in Albion-terrace, and were supplied with water from a copious spring in the road in front of the terrace, the

water of which was conducted, by a brick barrel-drain between Nos. 7 and 8, to the back of the houses, and then flowed right and left, to supply tanks in the ground behind each house, the tanks being made of brickwork and cement, covered with a flat stone, and connected with each other by stoneware pipes six inches in diameter. A leaden pipe conveyed water from each tank to a pump situated in the back kitchen. There was a cesspool behind each house, under the privy, and situated four feet from the water-tank. The ground was opened behind the houses No. 1 and No. 7, and the drains examined under the superintendence of Mr. Grant, the assistant-surveyor to the Commissioners of Sewers. The cesspools at both these places were quite full, and the overflow-drain from that at No. 1 choked up. At this house the respective levels of the cesspool and the water-tank were measured, and the top of the overflow-drain from the cesspool was found to be fifteen inches above the top of the tank, and the intervening ground was very wet. The overflow-drain mentioned above had no bottom, or one so soft that it could be penetrated with a stick; and it crossed, at right angles, above the earthenware pipe of the water-tank, the joints of which were leaky, and allowed the water to escape. Behind No. 7, Mr. Grant found a pipe for bringing surplus water from the tanks, communicating with a drain from the cesspool; and he found a flat brick drain laid over the barrel-drain before mentioned, which brought the water from the spring. It appears, from a plan of the property, that this drain, which was continued in a direction towards the sewer in Battersea-fields, brought surface-drainage from the road, and received the drains from the cesspools, the house-drains from the sinks in the back kitchens, and the surplus water, or some of it, from the tanks. There was every reason to believe that this drain was stopped up, but that was not ascertained: at all events, it was unable to convey the water flowing into it during the storm on July 26th, as it burst near the house No. 8, and inundated the lower premises of that and the adjoining house, No. 9, with foetid water; and it was from this time that the water, which had occasionally been complained of before, was found by most of the people in these seventeen houses to be more or less impure or disagreeable." (pp. 25—7.)

And he gives a description of the drains and water pipes, to explain how this took place.

The first person was attacked on July 28th:

"There are no data for showing how the disease was communicated to the first patient, at No. 13, on July 28th; but it was two or three days afterwards, when the evacuations from this patient must have entered the drains having a communication with the water supplied to all the houses, that other persons were attacked, and in two days more the disease prevailed to an alarming extent." (p. 28.)

The premises were examined by Dr. Milroy, who attributed the outbreak to an open sewer, to effluvia from sinks, and to an accumulation of offensive rubbish, in the house in which the first case occurred. Dr. Snow considers, and perhaps correctly, this explanation to be incorrect.

We must observe, however, that in addition to the impregnation of the water, it appears that there must have been, immediately before the attack of cholera, contamination of the air also, as on the 26th of July, two days before the first case, the lower premises of two houses were flooded with foetid water from the blockage of the drain; and doubtless the same effect was produced, in a less degree, in the other houses. Although we are not informed how long this stagnant water remained, it would probably, for some days, be sufficient to render the air impure. We are led to believe, from Dr. Snow's account, that the water had been previously contaminated by faulty arrangement of the drain and water-

tanks, and yet no decided cholera seems to have occurred till this overflow, two days after which the first case (the origin of which Dr. Snow leaves doubtful) took place.

One fact mentioned by Dr. Snow strengthens our suspicion that the air may have had as much to do with it as the water, for it is mentioned, that

“There were two or three persons attacked with cholera amongst those who came to nurse the patients after the water was condemned, and who, consequently, did not drink it; but these persons were liable, in waiting on the patient, to get a small portion of the evacuations into the stomach in the way first pointed out; and there might be food in the houses previously prepared with the tainted water.” (p. 29.)

These conjectures as to how the cholera matter got into the stomach of the nurses who did not drink the water, appear to us to be rather out of place. The point is to prove the fact of water being the agent, and not to assume it, and then to seek for some other explanation of those cases for which the presumed contamination cannot account.

3. Dr. Snow then quotes from Dr. Lloyd two instances in which cholera prevailed in limited districts, where water contaminated with sewage matter was used. The first instance is very inconclusive; the second is the following:

“Charlotte-place, in Rotherhithe, consists of seven houses, the inhabitants of which, excepting those of one house, obtained their water from a ditch communicating with the Thames, and receiving the contents of the privies of all the seven houses. In these houses there were twenty-five cases of cholera, and fourteen deaths; one of the houses had a pump railed off, to which the inhabitants of the other houses had no access, and there was but one case in that house.” (p. 31.)

In this example, as in almost all the other cases adduced by Dr. Snow, we miss the very necessary information as to the number of persons resident in each house; their ages, occupations, and habits; the kind of house in which they lived, &c. In six houses there were altogether twenty-four cases of cholera, in the seventh house only one case. For anything we are told to the contrary, however, there may have been only a single case in one of the six houses, and a greater number than the average in some of the others. If this were so, the point and force of the argument at once disappears.

4. Another case, quoted from the Board of Health, is stronger:

“In Manchester, a sudden and violent outbreak of cholera occurred in Hope-street, Salford. The inhabitants used water from a particular pump-well. This well had been repaired, and a sewer which passes within nine inches of the edge of it became accidentally stopped up, and leaked into the well. The inhabitants of thirty houses used the water from this well; among them there occurred nineteen cases of diarrhoea, twenty-six cases of cholera, and twenty-five deaths. The inhabitants of sixty houses in the same immediate neighbourhood used other water; among these there occurred eleven cases of diarrhoea, but not a single case of cholera, nor one death.” (p. 31.)

5. A fifth instance is mentioned, on the authority of Dr. T. K. Chambers:

“Dr. Thomas King Chambers informed me, that at Ilford, in Essex, in the summer of 1849, the cholera prevailed very severely in a row of houses a little way from the main part of the town. It had visited every house in the row but one. The refuse which overflowed from the privies and a pigstye could be seen

running into the well over the surface of the ground, and the water was very fetid; yet it was used by the people in all the houses except that which had escaped cholera. That house was inhabited by a woman who took linen to wash, and she, finding that the water gave the linen an offensive smell, paid a person to fetch water for her from the pump in the town, and this water she used for culinary purposes, as well as for washing." (p. 32.)

We think we ought to be careful how far we attach great weight to an argument of this sort; we are not informed how many persons lived in the house with this woman, how many houses there were in the row, and other circumstances of the like kind. If the woman was the only inhabitant of the house, as we are led to conclude from the mode in which the sentence is worded, her escape would have nothing wonderful about it, as only the minority are attacked with cholera.

6. The next case is one mentioned to Dr. Snow by a friend. Cholera prevailed at Locksbrook, near Bath, and the owner of some houses was informed that the water was bad:

"He sent a surveyor, who reported that nothing was the matter. The tenants still complaining, the owner went himself, and on looking at the water and smelling it, he said that he could perceive nothing the matter with it. He was asked if he would taste it, and he drank a glass of it. This occurred on a Wednesday; he went home, was taken ill with the cholera, and died on the Saturday following, there being no cholera in his own neighbourhood at the time." (p. 32.)

Thus the surveyor finds nothing wrong, and the hapless owner is so convinced that the water is good, that he drinks a glass of it, and dies three days afterwards; therefore it was the water.

Nothing could be very much looser than this statement; we might just as well affirm that the man having been in the very place, and in the very houses, where cholera was, received it, as we believe many persons do receive it, through the medium of the air. Dr. Snow will say that the other evidence in proof of the propagation by water gives weight to his view of the case, but we may quite as well say that the evidence by transmission through the air is much more stringent, and gives weight to our hypothesis.

7. The next case appears to us to be quite worthless as evidence. The village of Newburn, near Newcastle-on-Tyne, suffered frightfully from cholera in 1832, at which time the sources of water-supply were the same as at present. Dr. Snow applied to Mr. Davison, surgeon, of Newburn, who informed him that—

"It was considered that the evacuations of the people could not get into any of the wells; but the vicar thought that the water of a little brook which runs past the village, and falls into the Tyne immediately afterwards, might find its way into that well which is chiefly resorted to." (p. 33.)

On further inquiry, it was found that the brook received "the refuse of a small village," and also passed "through a privy used by the workmen of a steel factory." The drain conveying water from an old coal-pit to the well chiefly used in Newburn passed underneath the brook, and then ran alongside it for about three hundred yards:

"Mr. Davison said that it was disputed whether there was any communication between the drain and the brook, but that it was highly probable that there might be; and that an occurrence which took place a few months previously seemed to prove that there was. Some gas-water from the steel manufactory mentioned

above got by accident into the brook, and *some of the people affirmed that the water in the well was strongly impregnated with it.*" (p. 34.)

Thus the water of the brook, it is thought, *might* find its way into the drain leading to the well. Although no communication can be traced between the two, the affirmation of *some* of the people, that the well had been impregnated with gas-water some months before, is taken as conclusive evidence that there was such communication during the time of the cholera: that conclusion arrived at, the first case of cholera occurs; it is left unaccounted for, but from it, it is easy to deduce all the rest.

"As several days elapsed between the first case of cholera and the great outbreak, it is probable that the water in which the soiled linen must have been washed, and which would necessarily run into the brook, was the means of communicating the disease to the thirteen persons taken ill on the night between the 9th and 10th of January." (p. 35.)

This surely cannot be considered as sound evidence. The mere *possibility* of the well being contaminated cannot be held sufficient in an inquiry demanding such accuracy and care in the collection of the facts.

8, 9. After an allusion to a very obscure Indian case, of which nothing can be made, Dr. Snow refers to the late attack of cholera in the Black Sea fleet. He quotes from the 'Medical Times and Gazette,' September 30th, the following passage from the letter of a naval medical officer:

"A week after the return of the fleet to Baljik, on the 7th of August, about four thousand French troops encamped on the heights abreast our anchorage. These were part of the first division of the army that had marched to Kostenje, about ten days before. By it the first blood had been drawn on the part of the allied army. The loss in battle was small, but they had encountered an enemy more terrible than the Russians. The cholera had broken out among them, and attacking four hundred on the first night, had destroyed sixty. The total loss had been something incredible. It was said, that out of eleven thousand men, not less than five thousand had perished in a few days. This dreadful calamity was attributed to drinking water from wells that had been poisoned by throwing in putrid carcasses.

"Putting aside the question of intentional poisoning, which always presents itself as the most ready way of accounting for such destruction, perhaps some support to the theory, that water is the medium by which cholera poison is conveyed, may be found in this circumstance, and in another of which I was witness. These soldiers, wearied by marching from a focus of cholera infection, were seen, many of them, washing their persons and clothing in the stream from which all the French ships of war, and the majority of the English fleet, obtained their supply of water. This was going on on the 7th and 8th, and, on the nights of the 9th and tenth, the disease burst out with great violence among the crews of several ships." (pp. 36, 37.)

We do not find this point alluded to in the 'Report' on this attack of cholera, just published by the Admiralty. It would appear, from that able document, that the main outbreak of cholera appeared in the fleet at Varna and at Baljik nearly at the same time. At the latter place, the intense outbreak was certainly from four to six days after the French troops had marched down; but it would appear that a solitary case had occurred on board the *Diamond* on the 16th of July, and that bowel complaints, gradually assuming a more severe form, and at last with "decided choleraic character," were common on board the fleet between the 1st and the 7th of August—i.e., the day that the French troops arrived to whom the outbreak is attributed—and on the 7th, a fatal case

occurred on board the *London*. The evidence, therefore, that connects these two circumstances—the arrival of French troops and the outbreak on board the ships—is not so stringent as it seems. But even if the French troops brought the cholera with them, the evidence of its being communicated by the water is most imperfect.

10. Dr. Snow makes no remark on the suggestion contained in the letter quoted in the 'Medical Times,' but proceeds to consider the terrible outbreak of cholera in a limited district near Golden-square, London, which occurred in 1854:

"There were a few cases of cholera in the neighbourhood of Broad-street, Golden-square, in the latter part of August; and the so-called outbreak, which commenced in the night between the 31st August and the 1st September, was, as in all similar instances, only a violent increase of the malady. As soon as I became acquainted with the situation and extent of this irruption of cholera, I suspected some contamination of the water of the much-frequented street-pump in Broad-street, near the end of Cambridge-street; but on examining the water, on the evening of the 3rd September, *I found so little impurity in it of an organic nature, that I hesitated to come to a conclusion.* Further inquiry, however, showed me that there was no other circumstance or agent common to the circumscribed locality in which this sudden increase of cholera occurred, and not extending beyond it, except the water of the above-mentioned pump. I found, moreover, that the water varied, during the next two days, in the amount of organic impurity, visible to the naked eye, on close inspection, in the form of small white, flocculent particles; and *I concluded that, at the commencement of the outbreak, it might possibly have been still more impure.* I requested permission, therefore, to take a list, at the General Register Office, of the deaths from cholera, registered during the week ending 2nd September, in the sub-districts of Golden-square, Berwick-street, and St. Ann's, Soho, which was kindly granted. Eighty-nine deaths from cholera were registered, during the week, in the three sub-districts. . . .

"On proceeding to the spot, I found that nearly all the deaths had taken place within a short distance of the pump. There were only ten deaths in houses situated decidedly nearer to another street-pump. . . .

"With regard to the deaths occurring in the locality belonging to the pump, there were sixty-one instances in which I was informed that the deceased persons used to drink the pump-water from Broad-street, either constantly or occasionally. In six instances I could get no information, owing to the death or departure of every one connected with the deceased individuals; and in six cases I was informed that the deceased persons did not drink the pump-water before their illness." (pp. 38—40.)

Dr. Snow then refers to workhouses and large establishments in the neighbourhood, which did not use the pump water, and the inmates of which escaped cholera. He then quotes the case of a gentleman who went to Poland-street, where his brother had just died of cholera, drank a small tumbler of water mixed with brandy, left the neighbourhood, and was seized with cholera on the following day. This, however, is inconclusive, as the person was in an infected neighbourhood.

Finally, Dr. Snow relates the following most extraordinary case, which, if there is not some fallacy, is certainly unanswerable:

"In the 'Weekly Return of Births and Deaths' of September 9th, the following death is recorded as occurring in the Hampstead district: 'At West-end, on 2nd September, the widow of a percussion-cap maker, aged 59 years, diarrhoea two hours, cholera epidemica sixteen hours.'

"I was informed by this lady's son that she had not been in the neighbourhood of Broad-street for many months. A cart went from Broad-street to West-end

every day, and it was the custom to take out a large bottle of the water from the pump in Broad-street, as she preferred it. The water was taken on Thursday, 31st August, and she drank of it in the evening, and also on Friday. She was seized with cholera on the evening of the latter day, and died on Saturday, as the above quotation from the register shows. A niece, who was on a visit to this lady, also drank of the water; she returned to her residence, in a high and healthy part of Islington, was attacked with cholera, and died also. There was no cholera at the time, either at West-end or in the neighbourhood where the niece died. Besides these two persons, only one servant partook of the water at Hampstead West-end, and she did not suffer, or, at least, not severely." (pp. 44—45.)

It will have been observed, that the contamination of the pump water with drains, or by any other method, is not even attempted to be proved, and the disease had commenced to decline before the supply of the suspected water was stopped.

"The greatest number of attacks in any one day occurred on the 1st of September, immediately after the outbreak commenced. The following day the attacks fell from one hundred and forty-three to one hundred and sixteen, and the day afterwards to fifty-four. A glance at the table will show that the fresh attacks continued to become less numerous every day. On September the 8th—the day when the handle of the pump was removed—there were twelve attacks; on the 9th, eleven; on the 10th, five; on the 11th, five; on the 12th, only one; and after this time, there were never more than four attacks on one day.

"There is no doubt that the mortality was much diminished, as I said before, by the flight of the population, which commenced soon after the outbreak; but the attacks had so far diminished before the use of the water was stopped, that it is impossible to decide whether the well still contained the cholera poison in an active state, or whether, from some cause, the water had become free from it. The pump-well has been opened, and I was informed by Mr. Farrell, the superintendent of the works, that there was no hole or crevice in the brickwork of the well, by which any impurity might enter; consequently in this respect the contamination of the water is not made out by the kind of physical evidence detailed in some of the instances previously related. I understand that the well is from twenty-eight to thirty feet in depth, and goes through the gravel to the surface of the clay beneath. The sewer, which passes within a few yards of the well, is twenty-two feet below the surface. The water at the time of the cholera contained impurities of an organic nature, in the form of minute whitish flocculi, visible on close inspection to the naked eye, as I before stated. Dr. Hassall, who was good enough to examine some of this water with the microscope, informed me that these particles had no organized structure, and that he thought they probably resulted from decomposition of other matter.

"A very important point in respect to this pump-well is *that the water passed* with almost everybody as being perfectly pure, and it did in fact contain a less quantity of impurity than the water of some other pumps in the same parish, which had no share in the propagation of cholera. We must conclude from this outbreak that the *quantity of morbid matter which is sufficient to produce cholera is inconceivably small.*" (pp. 51—54.)

Dr. Snow also observes, that he inquired of many persons if any change in the character of the water had been observed, and was answered in the negative. Mr. Gould, the eminent ornithologist, however, noticed that on the 2nd September "it had an offensive smell," although it was perfectly transparent.

The chief circumstances which Dr. Snow appears to rely upon are, that the great mortality took place in the district supplied with this pump-water, and that persons in the district who did not drink the

water escaped. He has not been able to prove that all were attacked who drank this water, and that none were attacked who did not drink; but such precision of evidence could not fairly be demanded from him.

The weak points in this array of evidence are, 1st, the want of proof of contamination of water, or, rather, the evidence in favour of its purity; 2ndly, the deficiency in negative evidence, that there was no other local cause which produced this partial outbreak; and, 3rdly, the fact that the disease ran rapidly to its acme, and then declined, while the water supply remained the same.

On examining a map given by Dr. Snow, it would clearly appear that the centre of the outburst was a spot in Broad-street, close to which is the accused pump; and that cases were scattered all round this nearly in a circle, becoming less numerous as the exterior of the circle is approached. This certainly looks more like the effect of an atmospheric cause than any other; if it were owing to the water, why should not the cholera have prevailed equally everywhere where the water was drunk? Dr. Snow anticipates this by supposing that those nearest the pump made most use of it; but persons who lived at a greater distance, though they came farther for the water, would still take as much of it. We observe also that there are several other pumps in the neighbourhood, and in one of these the water was notoriously offensive; yet comparatively little cholera took place thereabout. There are, indeed, so many pumps in this district, that wherever the outbreak had taken place, it would most probably have had one pump or other in its vicinity.

11. The last case noticed by Dr. Snow is that of a limited outbreak in Deptford, which seems to us as deficient in weight of evidence as those which have preceded it.

“Deptford is supplied with very good water from the river Ravensbourne by the Kent Water Works, and until this outbreak there was but little cholera in the town. . . . On going to the spot on September 12th and making inquiry, I found that the houses in which the deaths had occurred were supplied by the Kent Water Works, and the inhabitants never used any other water. The people informed me, however, that for some few weeks the water had been extremely offensive when first turned on; they said it smelt like a cesspool, and frothed like soap suds. They were in the habit of throwing away a few pailsful of that which first came in, and collecting some for use after it became clear. On inquiring in the surrounding streets, to which this outbreak of cholera did not extend, viz., Wellington-street, Old King-street, and Hughes’s-fields, I found that there had been no alteration in the water. *I concluded, therefore, that a leakage had taken place into the pipes supplying the places where the outbreak occurred, during the intervals when the water was not turned on.*” (pp. 55—56.)

We have now given, as far as possible, in Dr. Snow’s own words, an abstract of his evidence. In estimating its weight we must bear in mind the object for which it is adduced. It is not to prove that bad water acts as a predisposing cause, but that the water contains itself the cause of cholera. To prove so weighty a fact, we require not only positive, but negative evidence. If the cause of cholera cannot be absolutely discovered in the water, we must at least have proof that the water is contaminated, and we must also have negative evidence that no other circumstance existed which could explain the attack except the contaminated water. Now, certainly in no less than seven of the eleven cases

(Nos. 3, 5, 6, 7, 8, 9, and 11), the evidence to prove the effect of the water is so loosely stated, and the accessory circumstances of the outbreaks are so utterly disregarded, that we do not think any one can feel that even a tolerable case is made out in favour of Dr. Snow's opinion.

In the four remaining examples, the evidence is stronger. The case of the two courts at Horsleydown is the best, though it is not conclusive; and next to this is the instance at Salford quoted from the Board of Health, although even here the account is so meagre that we scarcely know whether to accept it. The outbreak at Albion-terrace, singularly localized as it was, is yet susceptible of another explanation, viz., that the air was contaminated by the bursting of the drain; and the attack in Broad-street wants entirely one material item of evidence, viz., proof that the water was contaminated; indeed, we have seen that Dr. Snow is here absolutely obliged to admit, that the water may be apparently pure, and that the quantity in it "of morbid matter sufficient to produce cholera is inconceivably small." If we accept this opinion, we can never exclude the agency of water in any case.

Considering, therefore, the imperfection of both the positive and negative evidence, and the want of explanation of the earliest cases, we conclude that Dr. Snow has not yet proved that cholera is *always* communicated by means of water; and that he has not even *proved* that it has been so communicated in a single case. Yet, in the face of the evidence furnished by the attacks at Horsleydown and Salford, we cannot entirely reject Dr. Snow's views. We have made no reference to the other phenomena of the spread of cholera which Dr. Snow's hypothesis cannot explain; but have simply taken the facts given to us by Dr. Snow.

We must now, however, turn to the second part of Dr. Snow's book, in which a fresh argument for the influence of water is brought forward. Almost half of the work is taken up with a most elaborate inquiry into the water supply, as compared with the number of deaths from cholera. The object is to show, that when the water was supplied from an impure source, and was therefore probably contaminated with sewage matters, cholera was most prevalent.

It is impossible for us to go minutely into the mass of evidence so laboriously collected and arranged by Dr. Snow. In this part of his work, as in the former half, Dr. Snow does not sufficiently discuss the other conditions under which the people living in various districts of London were placed, besides those of varying water supply. He alludes, indeed, to, but speedily dismisses, the important law of the influence of elevation, demonstrated by the Registrar-General; and refuses, indeed, to admit the effect of elevation, and refers the difference of prevalence entirely to the water supply. He does not, in our opinion, pay sufficient attention to the density of the population, nor to the other causes of impurity of the air.

We shall now shortly advert to some part of the evidence.

In 1849, two water companies—the Southwark and Vauxhall, and the Chelsea—took their water from the same part of the Thames; the district supplied by the former company suffered severely, that supplied by the latter, very little. Dr. Snow explains this as follows:

"The Chelsea Company, which supplies some of the most fashionable parts of

London, took great pains to filter the water before its distribution, and in so doing no doubt separated, amongst other matters, the greater portion of that which causes cholera. On the other hand, although the Southwark and Vauxhall and the Lambeth Water Companies professed to filter the water, they supplied it in a most impure condition." (p. 64.)

We doubt the correctness of this answer. Both companies filter, says Dr. Snow, but one filters more thoroughly than the other, and therefore gets rid of that which causes cholera. But what evidence is there, that at the time when cholera prevailed, the one company did filter so carefully, and the others so carelessly? There appears to have been no examination made of the water at the time, and certainly we are not inclined to believe, without definite testimony, that one company could purify their water completely, and the other not at all.

But in a later page we find Dr. Snow modifying his opinion, for he discovers a fact which shows that filtration is not all-powerful. Till lately, Milbank prison was supplied with Thames water, which was filtered most carefully through sand and charcoal, yet cholera prevailed in the prison. As filtration is not, then, sufficient, the purification of the Chelsea water, previously referred to filtration, is now ascribed to retention in the reservoirs causing the decomposition of the choleraic substance.

A second point of evidence on which Dr. Snow relies, requires rather a longer discussion.

Part of London, on the south side of the Thames, is supplied by the Southwark and Vauxhall Company, and part by the Lambeth Company; the former takes its water from Battersea, the latter from Thames Ditton. One portion of the town, however, is supplied by both companies; some houses being supplied by one, some by the other company. Now, it is evident, that if in this part of London it should be found that the inmates of the houses supplied with the Battersea water suffered greatly, and those supplied with Ditton water suffered little, then of course, as all other circumstances (locality, air, trades, &c.) in the district are the same, the influence of the water would be proved beyond a doubt. Dr. Snow, with most praiseworthy industry, proceeded to investigate the point:

"The inquiry was necessarily attended with a good deal of trouble. There were very few instances in which I could at once get the information I required. Even when the water-rates are paid by the residents, they can seldom remember the name of the water company till they have looked for the receipt. In the case of working people who pay weekly rents, the rates are invariably paid by the landlord or his agent, who often lives at a distance, and the residents know nothing about the matter. It would, indeed, have been almost impossible for me to complete the inquiry, if I had not found that I could distinguish the water of the two companies with perfect certainty by a chemical test. The test I employed was founded on the great difference in the quantity of chloride of sodium contained in the two kinds of water at the time I made the inquiry. On adding solution of nitrate of silver to a gallon of the water of the Lambeth Company, obtained at Thames Ditton, beyond the reach of the sewage of London, only 2.28 grains of chloride of silver were obtained, indicating the presence of .95 grains of chloride of sodium in the water. On treating the water of the Southwark and Vauxhall Company in the same manner, 91 grains of chloride of silver were obtained, showing the presence of 37.9 grains of common salt per gallon. Indeed, the difference in appearance on adding nitrate of silver to the two kinds of water was so great, that they could be at once distinguished without any further trouble.

Therefore, when the resident could not give clear and conclusive evidence about the water company, I obtained some of the water in a small phial, and wrote the address on the cover, when I could examine it after coming home. The mere appearance of the water generally afforded a very good indication of its source, especially if it was observed as it came in, before it had entered the water-butt or cistern; and the time of its coming in also afforded some evidence of the kind of water, after I had ascertained the hours when the turncocks of both companies visited any street. These points were, however, not relied on, except as corroborating more decisive proof, such as the chemical test, or the company's receipt for the rates." (p. 77, 78.)

We looked now anxiously for the result of this inquiry, and found it stated further on :

"There were three hundred and thirty-four deaths from cholera in these four weeks, in the districts to which the water supply of the Southwark and Vauxhall and the Lambeth Company extends. Of these it was ascertained, that in two hundred and eighty-six cases the house where the fatal attack of cholera took place was supplied with water by the Southwark and Vauxhall Company, and in only fourteen cases was the house supplied with the Lambeth Company's water; in twenty-two cases the water was obtained by dipping a pail directly into the Thames; in four instances it was obtained from pump-wells; in four instances from ditches; and in four cases the source of supply was not ascertained, owing to the person being taken ill whilst travelling, or from some similar cause." (pp. 79, 80.)

On first reading this paragraph, we thought that the deaths referred to, took place only in the district with the intermingled supply, and that this was the answer to the "experiment on a grand scale," so laboriously inquired into by Dr. Snow. But, on re-perusing the passage and its context, we found that these deaths had taken place in *all* the districts supplied by the two companies, separately or conjointly. If this reading be correct, we doubt if the comparison can be safely made, for the Lambeth Company supplies, to a considerable extent, a good neighbourhood on elevated ground (including the healthy districts of Streatham, Forest-hill, and Sydenham); while the Southwark and Vauxhall Company supplies the greater part of the poorest, lowest, and marshiest district in London.

If, however, the deaths referred to in the paragraph just quoted, represent those only of the subdistrict supplied conjointly by the two companies, we have still some grounds of objection. First, are we to rely on the chemical test referred to by Dr. Snow? Does the Ditton water never contain *more*, and the Battersea *less*, chloride of sodium? Dr. Snow himself mentions that :

"When the water (Battersea) of the Southwark and Vauxhall Company was examined by Messrs. Graham, Miller, and Hofmann, at the latter part of January, 1851, it contained only 1.99 grains of chloride of sodium, or about one-twentieth as much as it contained last September, and one fifteenth as much as on 21st November, 1854." (p. 97.)

May not this small amount have occurred on some of the days on which water was collected by Dr. Snow? Did he ascertain that the water in the reservoirs of the company contained always this large amount of chloride of sodium during the prevalence of cholera, or was the water he tested merely taken from the cisterns supplied by the Southwark and Vauxhall Company? We certainly do feel great doubt, on the evidence

before us, whether the source of supply could safely be inferred from a chemical test alone.

Another weak point in the argument seems to us to vitiate Dr. Snow's whole case. He has not told us how many houses in the district referred to were supplied respectively by the Battersea and Ditton waters. It may be that the Ditton water was furnished to so few houses, that the small number of deaths from cholera were merely proportionate to the smaller number of houses. Dr. Snow endeavours to meet this difficulty, by giving, from the Parliamentary return, the number of houses supplied respectively by the two companies. But this return applies to the entire districts, and not to the special district where the supplies are intermingled; so that really we are in doubt whether the Ditton water is supplied to half of this special district, or to a quarter or a tenth part of it.

However, we learn from the Parliamentary return that the total number of houses supplied by the Southwark and Vauxhall Company is 40,046, and by the Lambeth Company 26,107, or as three to two, nearly. If this proportion be the same in the district common to both (of which, however, we have no certainty), then the deaths, of course, should properly be one-third more numerous in the houses supplied by the Southwark and Vauxhall than in those of the Lambeth Company. The difference in the mortality is, however, much more than this, if Dr. Snow's facts are to be received.

A better instance of the influence of water than either of these cases is given by the Lambeth Company. In 1849, it drew its supplies from Battersea; in 1854, from Thames Ditton. Was the mortality in the districts supplied by it diminished in the latter year? Of this there is proof, so far, that whereas the districts supplied solely by the Southwark and Vauxhall, show an equal mortality in the two years, those supplied partly by the Lambeth, as well as the Southwark and Vauxhall Company, show in several cases a remarkable diminution of mortality.

We shall not follow Dr. Snow into his account of the provincial towns, where we find little that is satisfactory. Here, as in all other cases, Dr. Snow refers only to the water-supply, and neglects all the other circumstances, as we could easily prove, had we space to refer to the cases of Birmingham, Leicester, or Exeter. The conclusions as to the attack at Newcastle do not appear to us to be borne out by the evidence.

Besides cholera, Dr. Snow thinks other diseases, yellow fever, intermittents, plague, and typhoid fever, may be also propagated by drinking water. The evidence for this is so slight, that we shall not discuss the point now.

We have already said, that from the positive evidence adduced by Dr. Snow, we were unable to do more than conclude that he had rendered the transmission of cholera by water an hypothesis worthy of inquiry; we cannot draw any other conclusion from his researches on water supply, than that the predisposing effects of impurity of water are also rendered highly probable. We may be mistaken in this, and the evidence which seems weak to us may not be so to others. If so, when additional evidence shall be given, we shall receive it with the greatest pleasure; for though we think

Dr. Snow's hypothesis, if proved, cannot explain all the phenomena of the spread of cholera,* it would yet clear up some of the mysterious phenomena of its diffusion. Its establishment would therefore be an immense gain to science, and, we need not add, an important service to the State.

We cannot conclude without one remark. We have taken Dr. Snow's facts, and have not only criticised them as carefully as we could, but have expressed our opinions without reserve. In no other way could we treat this all-important question; and Dr. Snow, we are sure, would not have wished us to adopt any other course. But it is only simple justice to Dr. Snow to state, that no man could have pursued the inquiry with greater diligence. Although we think that he is biased by his creed,† and obstinately looks only in one direction, we close his book with the conviction that he is an honest and conscientious observer. If his discovery should be established, the prevention of cholera would be easy, and for this reason we think that the most careful inquiry should be made at once by the government into the subject, and proof or disproof given of Dr. Snow's opinions. In India, the point could be soon decided.

E. A. Parkes.

REVIEW XIV.

Vertigo: a Paper read to the North London Medical Society, April 12th, 1854. By J. RUSSELL REYNOLDS, M.D. Lond., University Medical Scholar.—London, 1854. 8vo, pp. 46.

WE are glad that Dr. Reynolds should have taken an opportunity of directing the attention of his professional brethren to the subject of vertigo, because we believe that the time is come at which a far more precise estimate can be formed respecting the nature of this condition, than was possible whilst the physiology of the sensori-motor apparatus was in a less advanced state; and because a careful appreciation of its phenomena will not only increase its semeiological value, but will, in its

* We would mention also here the experiments of Dr. Lauder Lindsay, who appears to have given dogs cholera by making them breathe a choleraic atmosphere, but who, like others before him, could not cause cholera by feeding the dogs on cholera dejections.

† As an instance of this we may cite Dr. Snow's mode of accounting for the outbreak of cholera in London in 1854. On the 25th of July, the mate of a merchant steamer which had returned three weeks previously from the Baltic, died of cholera in London. "This patient was the chief-mate to a steam-vessel taking stores to, and bringing home invalids from, the Baltic fleet. Three weeks ago he brought home in his cabin the soiled linen of an officer who had been ill. The linen was washed and returned." The time when this steam-vessel arrived in the Thames with the soiled linen on board, was a few days before the first cases of cholera appeared in London, and these first cases were chiefly amongst persons connected with the shipping in the river. It is not improbable, therefore, that a few simple precautions, with respect to the communications with the Baltic fleet, might have saved London from the cholera this year, or, at all events, greatly retarded its appearance." Was any deduction of so extraordinary a kind ever made on such grounds? We are not told that the officer whose clothes were brought home had had cholera; we are not told where the clothes were washed, by whom they were washed, or when they were washed. Supposing the mate to have sent them to be washed the day he arrived—three weeks before his death—as is most probable, can the most perverse ingenuity connect his death with these clothes? And then with regard to London itself, as many cases of cholera had occurred before July, how is it possible for one moment to adhere to the hypothesis that the soiled linen of a sick officer in the Baltic fleet being washed in the Thames was the cause of the epidemic?

turn, help to elucidate the normal actions of that part of our organization. Our author uses the term *vertigo* in a more extended sense than that in which it is usually understood: making it to comprehend not merely vertiginous *sensations*, but also vertiginous *movements*. And he proceeds first to the examination of the latter class of phenomena, because he thinks that they throw some light on the nature of the former.

A very complete historical summary is given of the phenomena of *vertiginous motion*, as they present themselves to the observer of natural disease, or to the physiological experimenter; and from the latter order of facts the conclusion is drawn, that the known occasions of vertiginous movement are lesions of one or more of the following parts:

"1. The transverse or middle crura of the cerebellum, with their continuation upon the Pons Varolii.

"2. The crura cerebri.

"3. The thalami optici.

"4. The medulla oblongata.

"5. The tubercula quadragemina.

"6. The semicircular canals of the ear.

"7. The eye itself." (p. 15.)

Now, as the functions of these organs, so far as we are able to assign them with probability, are as follows—

1. The centre of co-ordination;

2. The centres of common sensation and mobility;

3. The organs of the special senses (particularly of sight and hearing);

4. The fibres placing these organs in functional relationship;

the following general conclusion may be drawn:

"The vertiginous motions follow injuries to the organs of special sense, the organ for the co-ordination of movement, and the fibres connecting the latter with the former, or with the systemic muscles. In either case, some part of the nervous system, or its appendages, ministering to the '*consensus* of the nerves,' is injured, and the mysterious conversion of sensorial impressions into motor impulses is disturbed, or prevented from taking place." (p. 22.)

We believe, with Dr. Reynolds, that the real clue to the nature of this condition is to be found in that relation of muscular movement to *sensational* states, which has gradually come to be more distinctly marked-out by physiologists, in consequence of the exclusion of the excito-motor actions (*not* involving sensation) on the one hand, and of the emotional and volitional movements on the other. And this clue has been thus sagaciously followed up by our author. After pointing out our dependence upon guiding sensation in the execution even of our most purely volitional movements, he continues:

"The lower animals probably depend much more extensively upon the indications of sense, than do those possessed of greater intelligence, spontaneity, and educability; and, *à fortiori*, much more closely than Man. His energy of volition is displayed in subjecting thought, emotion, and muscular action, to the dictates of judgment; and it is certain that the power of doing so varies widely in individuals. In accomplishing the subjugation of the latter (muscular action), and in its direction to definite ends, he is guided mainly by sensation, and principally by touch, sight, and the muscular sense; and all our involuntary attitudes and motor impulses to change an unpleasant position are directed in the same manner. There is a feeling of physical rest or equilibrium, which we strive (it may be involun-

tarily) to attain by certain movements, and in this effort we are guided by impressions from without, and by the sense of our own muscular conditions. This feeling of equilibrium results from the harmony of our different sensations among themselves, and with the motor impulse which is their combined effect. When, therefore, any one group of the sensorial impressions is distorted or removed, the balance is *pro tanto* disturbed; and inasmuch as these impressions are themselves the stimuli of muscular action, attempts are made for its restoration. In the lower animals these attempts become much more marked than in Man, producing vertiginous or allied movements, which he, by a judgmatic discrimination between the inharmonious impressions, and by a volitional corrective power (which they do not possess to the same degree), is able to avert. The vertiginous movements, then, are the result of an effort to produce equilibrium; an effort developing itself in muscular action, through the agency of the nervous system, and under sensational guidance." (pp. 24, 25.)

Dr. Reynolds next considers *vertiginous sensations*, which he defines to be "the sensation of motion without (or independently of) its real existence." This sensation, he remarks, may be either *objective* or *subjective*; the motion being referred, in the former case, entirely or principally to surrounding objects; whereas, in the latter, it is referred to the person of the individual himself. We do not think these terms happily selected, because they more properly apply to the distinction next drawn by Dr. Reynolds, between the vertigo originating in some cause external to the individual, and therefore referable to some change in his relationship to surrounding objects; and that which is not dependent upon any such change, its immediate cause being internal. It is only when employed in this latter sense that the terms have a meaning parallel to that which attaches to them when they are used to designate sensations of other kinds; and in this very mode they are employed by Dr. Reynolds himself in a later part of his essay.

Most persons are personally familiar with the vertigo which originates in impressions on the organs of sense, especially those of sight; and an analysis of their own feelings will satisfy them that, when not produced by the movement of surrounding objects or by movement of the individual himself, they result from his finding himself placed, to use Dr. Reynolds's phrase, "in some totally unaccustomed relationship with the world around him." An instance of this kind lately occurred to the writer of this notice. He had never experienced the least giddiness in looking down from the top of a high building or a lofty precipice; but on lately mounting to the prospective gallery at the summit of one of the slender minarets of the Panopticon, in Leicester-square, and finding himself sustained, as it were, in mid-air without that feeling of firm support which is given by the proximity of a *mass*, he felt an incipient vertigo which warned him to descend in time; and he has learned that the experience of several others has corresponded in this respect with his own. The same state may be produced by various *internal* causes; and that which most clearly connects the two classes of phenomena is the production of vertiginous sensations by the *remembrance* of past feelings, as when some "sensitive subjects" are made giddy by the sight of *another* person ascending a ladder or looking down from a precipice, through being led by association to recur to their own former experience in like circumstances. The state of the nervous centres, on which vertiginous sensations immediately depend,

may, however, be induced by various physical conditions; and these Dr. Reynolds distinguishes, according as they affect the nervous system primarily or secondarily, into—1. “Conditions of the nervous system,” as changes in the circulation, organic lesions, or toxic influences; and, 2. “Conditions of the general system,” as pyrexia, cachexia, toxæmia, or particular organic disturbances (of the stomach, for example). We cannot, however, see the justice of this classification; for, in all cases, a disordered action of the nervous system must be the immediate antecedent, or proximate cause, of the vertiginous sensations; and the changes in the circulation, which constitute part of his first class, surely belong to the same category with the pyrexia of the second, and the toxic influences of his first class to the toxæmia of the second. The only natural arrangement seems to us to be that which should group the causes according as they affect—1. The *organic structure* of the nervous centres, either by mechanical injury or by perversion of their nutrition; and, 2. Their *functional power*, by alterations in the supply or quality of blood, or by nervous sympathy with remote organs.

“It must be remembered,” Dr. Reynolds justly remarks, “that, with the whole of our extended organism, the nervous centres are placed in intimate relation, receiving from its several parts indications or impressions which become the impulses to varied acts and functions; and that the balance of conscious health depends upon a thousand unconscious stimuli. The effects which similar static or dynamic diseases of different organs induce through the nervous centres vary widely; there is sickening depression from one, irritability from another, and dull hypochondriasis from a third; but from almost any disturbance there may be vertigo, although it is, perhaps, more frequently connected with derangement of stomach and liver than of any other vegetative organ.” (p. 43.)

We have ourselves witnessed a curious case, in which not only vertiginous sensations, but vertiginous movements (a continual turning to the right), were produced by the presence of indigestible food in the stomach; the symptoms, which at first seemed very alarming, passing off completely when the stomach had emptied itself by vomiting.

In concluding the short ‘Memoir,’ of which we have thus noticed the more prominent points, Dr. Reynolds remarks upon the importance of more accurately noting the phenomena of vertigo than has been usually done; its mere occurrence, as one of the symptoms of a disease, being usually alone recorded, and no notice being taken of the varied modes in which it manifests itself, or of the different bodily conditions with which it seems connected. In order to direct attention to these, he has drawn up a useful scheme in which the points of chief interest are systematically arranged; and expresses the anticipation, in which we believe him to be fully justified, that—“If the diagnostic worth of this symptom can be increased, not only will an addition be made to the science of pathology, but we shall receive some aid in the more slowly-progressing science and art of therapeutics.” And it has especially been rather with the view of opening a path for future investigation than of presenting any practical results already gained, that he has brought the subject before the profession.

PART SECOND.

Bibliographical Record.

ART. I.—*Medical Jurisprudence*. By ALFRED S. TAYLOR, M.D., F.R.S., &c. &c.—London, 1854. Small 8vo, pp. 935.

IN the year 1852, the fourth edition of this most comprehensive and useful Manual extended to eight hundred and twenty-six closely-printed pages. That edition speedily disappeared from the publisher's shelves, and two years afterwards the fifth (the present) edition was printed, and extended to one hundred and ten more pages. Including this edition, there have issued from the press *ten thousand seven hundred and fifty copies* of this work in the short period of eleven years! Much more need not be said about it; certainly in this instance commendation would be supererogation. We may, however, indicate the parts which have received the greatest amount of additions. These are chronic poisoning; the operation of prussic acid, morphia, strychnia, and aconite; wounds; injuries; blood-stains; burning of the human body, and spontaneous combustion; infanticide; pregnancy; abortion; gestation; insanity. This new matter comprises reports of most of the important trials that have involved medico-legal evidence. These additions are diffused throughout the work, and illustrate each part according to the relative cases or observations.

Obviously, we can, with such an abundance of material, do no more than record our conviction, derived from a close perusal of every page of the work, that Dr. Taylor has brought this edition up to the very level of medico-legal science, and maintains therein the reputation he has achieved, and which has not only placed him at the head of British legal medicine, but has redeemed the same from the imputation under which it long laboured, of having no literary representation.

ART. II.—*Handbuch der Speciellen Pathologie und Therapie*. Redigirt von R. VIRCHOW, Professor in Würzburg.
Manual of Special Pathology and Therapeia.

THIS great work, edited by Virchow, and written by himself and by many of the leading physicians of Northern Germany, is rapidly approaching completion. The first volume is finished, and parts of the second, fifth, and sixth volumes have been published. As the character of the work unfolds itself, however, we are sorry to find it assuming too

much the character of an encyclopædia. The immense length and fulness of detail with which many of the subjects are treated, appear to us quite to destroy its simple character of 'Handbook.' The diseases of the lungs are prefaced by a most elaborate, but most misplaced, treatise on the acoustic phenomena of auscultation and percussion, which is practically of little use. No less than three hundred and thirty-one closely-printed pages are filled by Dr. Falck, with an account of the various "Intoxicative agents." Yet this part, though quite too long in proportion to other subjects, is so admirably done, that we should regret to see it shortened. The diseases of the male genital organs and bladder (without syphilis) are described by Pitha at enormous length, two hundred and eleven pages being given to this subject alone. At this length, we tremble to think how much space, diseases of the heart, lungs, or brain, must occupy.

The first part of the diseases of the chylopoietic viscera, by Bamburger, occupies three hundred and seventy-six pages, and as much more will be required to complete the subject. Lebert discusses, in one hundred and fifty-two pages, the diseases of the blood and lymph vessels. Simon, the well-known dermatologist, describes syphilis at great length; and Virchow narrates, with his accustomed power, the account of the diseases communicated from animals to man.

ART. III.—*Food and its Adulterations; comprising the Reports of the Analytical Sanitary Commission of the Lancet.* By ARTHUR HILL HASSALL, M.D.—London, 1855. pp. 659.

WE are not aware that so complete a work on the adulteration of food has ever been published in any language as the one now before us. The labour which has been given to its production must have been excessive, and we are astonished that any one man could have found time enough to prosecute so vast an inquiry with such success. To analyze such a work is impossible, and we must satisfy ourselves with directing the attention of our readers to it, and with assuring them that the information contained in it should be familiar to every medical practitioner.

When these reports originally appeared in the 'Lancet,' the names of the dealers from whom the various substances were obtained, were published, in order, apparently, to check as much as possible the practice of adulteration, by showing dishonest tradesmen that the law would not protect them from deserved exposure. It is remarkable that no action has been brought against the 'Lancet,' for any of the statements made by Dr. Hassall in its columns. This is itself a guarantee of the correctness of the facts given by the author.

The adulteration of food is one of the most important subjects which can be considered by any government; and various have been the proposed schemes for preventing or limiting this most hurtful practice. The author proposes that a central Board, with analytical chemists attached to it, whose duty it should be to look out and detect adulteration in wholesale and retail practice, and to bring the offenders under the penalties of the law, should be instituted. Some such plan as this is, we believe, indis-

pensable. But as the knowledge of the microscope and of chemistry is now so general, it would be an accessory means of great importance to publish, in some cheap and accessible form, accurate microscopical representations of pure articles of food, and of the substances with which they may be adulterated; and to give brief directions for the application of chemical tests, when these are to be used.

A work of this kind, if published cheaply by authority, would soon find its way everywhere; those who have not already learnt to use the microscope, would acquire the art; and as every tradesman would know that many of his customers would have an infallible means of detecting imposition, food would very soon cease to be adulterated. The diffusion of practical knowledge of this kind would be better than a host of inspectors. We should advise Dr. Hassall to write a short work of this kind, in which his illustrations might be used. It would be one of the most useful works of the time.

ART. IV.—*Report on the Mortality and Public Health of Oxford, during the years 1849–50.—Oxford.* (Printed for the Ashmolean Society.) 1854.

WE owe this very valuable addition to statistical and sanitary knowledge to the labours of Dr. Greenhill. The Report is not carried down later than 1850, on account of the removal of the author to Hastings; but we trust that some one will have labour and zeal enough to continue the work so well commenced. The average annual mortality of Oxford was, during these two years, 24·09 to every one thousand persons living; and of the deaths no less than 24·73 per cent. were caused by zymotic diseases. The fatality was greatest in autumn and winter; summer was healthier, and spring healthiest.

In an appendix, an interesting account of the cholera in 1849 is given. The epidemic was much lighter than that of 1832; the introduction of the disease could not be traced to personal intercourse, nor was there any strong evidence of contagion observed during its course; yet Dr. Greenhill informs us, that at the close of the epidemic “it was the general conviction that cholera was occasionally communicated directly or indirectly by personal intercourse.” One part of the town, which had suffered severely in 1832, was scarcely affected in 1849, and this is ascribed to improvement in the water supply.

ART. V.—*Notes on some of the Developmental and Functional Relations of certain Portions of the Cranium.* Selected by F. W. PAVY, M.D. Lond., from the Lectures on Anatomy delivered at Guy's Hospital by JOHN HILTON, F.R.S.—*London*, 1855. pp. 93.

THESE ‘Notes’ are by no means dry details of the anatomy of the cranial bones, but are interesting disquisitions on various surgical and physiological problems, which naturally arise for discussion when the bones of the head are being demonstrated.

As an example of this we may quote one or two passages:

"On examining the interior of the adult cranium by transmitted light, certain ridges or elevations of bone are observed connected with the internal osseous plate; which, although pursuing an irregular and tortuous course, yet may be distinctly traced to converge towards the anterior clinoid processes of the sphenoid and the petrous portions of the temporal bones. In accordance with this anatomical arrangement and with the physical laws of nature, vibrations derived from a slight blow, or other external influence; instead of being diffused over the walls of the skull, meeting on the opposite side, and producing the injurious effects of *contre-coup*; travel along these ridges, which, by virtue of their greater thickness and solidity, form better conductors of vibrations than the surrounding parts, and converge towards the anterior clinoid processes, and the petrous portions of the temporals, where they terminate in the following manner.

"The anterior clinoid processes lie surrounded by cerebro-spinal fluid, without having, as I have already said, the slightest degree of connexion with the superimposed brain. And the vibrations conducted to these points, become here broken or lost in this fluid, which thereby intercepts their transmission to the tissue of the cerebral organ.

"The petrous portions of the temporal bones, being separated from the basilar process by an intervening layer of soft or membranous structure, the vibrations conducted in this direction become here also completely obstructed without transmission to the brain. Were it not, indeed, for this interception to the transmission of vibrations, by the interruption to the osseous continuity between the petrous portions of the temporal bones and the basilar process of the sphenoid and occipital, the latter would be constantly exposed to serious concussions, or even the chances of fracture, at the angle or point of collision of the vibrations travelling along and meeting from each petrous portion of the temporal." (pp. 42, 43.)

Afterwards, the same fact is noticed in another way:

"I have previously, by means of preparations copied from Nature with the greatest care and accuracy, shown you that the various elevations and depressions observed on the internal aspect of the dome of the skull do not correspond with the opposed surface of the brain. It is therefore evident, as I have also already stated, that they cannot be intended for adaptation to the convolutions of the cerebral hemispheres, and, if we examine them with care and attention, we find that they present a definite and designable arrangement: the elevated portions forming undulatory ridges which converge towards certain points projecting from the base of the skull. These ridges, or prominences, no doubt, give greater strength and additional security to the osseous parietes; but, forming the most dense and solid parts of bone, they also act as better conductors of vibrations; and, therefore, as it were, determine the course of their transmission from their seat of origin, in a blow, or otherwise, on the exterior of the head." (p. 52.)

The observations on the effects of the growth of the sphenoid are extremely interesting:

"The primary idea or primary intention of the development of the sphenoid seems chiefly with reference to the masticatory function; but, in the changes that it produces in the direction of the cranial and facial bones, it may not unaptly be compared to the scaphoid bones of the carpus and tarsus; for, in its growth and final development, it effects for the cranium and face precisely the same object that these bones effect for the hand and foot.

"Like these bones, the growth and completion of the sphenoid, in spreading out the cranium, and in enlarging the cavities of the organs belonging to the face, supplies the deficiency of the muscular tension, which in other parts of the body has so large a share in determining the final or perfect form of the bones. For example, with regard to the long bones of the extremities, the surrounding muscular tension, acting in different directions, forms a material influence in the determination of their precise ultimate configuration. In the case of the cranium, no

such comparable muscular tension exists. Indeed, did it exist, it would rather tend to compress than to expand the cranial cavity. But its absence is supplied by the growth and development of the sphenoid, which, wedged in amongst the other bones, alters their position and direction, and thus influences the ultimate configuration of the whole cranium and face." (pp. 73, 74.)

Mr. Hilton then points out the change in the direction of the petrous bones which occur during growth, and traces this to the expanding effect of the spinous processes of the sphenoid. The effects, also, on the temporal, and, through them, on the parietal bones, are then clearly narrated. These and other points are illustrated by some very admirable plates.

We are sorry to find, from the preface, that the Guy's Hospital Reports (in which part of this volume has appeared) are definitively given up. We regret this extremely, as there is not merely much historical interest connected with this journal, but the later numbers have contained very admirable papers, and are not unworthy to be placed by the side of those which contain the classic articles of Bright or Addison. We trust, however, to see the Guy's Hospital Reports some day resuscitated.

ART. VI.—*The Prize Essay on the Changes since the time of Pinel in the Moral Management of the Insane, &c. &c.* By DANIEL H. TUKE, M.D., Assistant Medical Officer to the York Retreat.

THE Society for Improving the Condition of the Insane will assuredly have done much towards the accomplishment of their benevolent object, by the publication of Dr. Tuke's essay. The very name of Tuke, so honourably associated with the York Retreat (the author is the great-grandson of the founder), is itself of good omen. In this slight volume of 119 pages we are presented with an admirably-written history of the gradual revolution which has been effected in the treatment of the insane in public institutions,—a change from barbarism, and its accompanying ignorance and ferocity, to the enlightened mildness of growing civilization: and, nevertheless, a change that can scarcely be dated before the present century. The names ever to be connected with this reformation are Pinel, Tuke, Charlesworth, Hill, and Conolly. The last honourable name deserves particular commemoration, because the bearer of it had the courage to try the experiment of non-restraint on a very large scale, and under the inspection of jealous witnesses; carrying it out with a noble mixture of firmness, patience, and forbearance, in spite of censure and ridicule; and commending it to the reason and the humanity of the public and the profession, in a quick succession of reports, essays, lectures, and reviews, worthy of a pen that has so often done good service in medical literature. We have already praised the execution of Dr. Tuke's essay, and we now only add, that it is not only clear and succinct as a history, but that it is also calm, unimpassioned, and free from any attempts at rhetorical denunciations of the old system, or from enthusiastic encomiums on the new. The reader will find many valuable hints as to methods of treatment to be used in substitution of mechanical restraint.

ART. VII.—*The Pathology of Drunkenness.* By CHARLES WILSON, M. D.
—*Edinburgh*, 1855. Small 8vo, pp. 230.

THE object of the writer has been to present to the general, as well as to the scientific reader, a sketch of the effects of drunkenness on the mind and body. He has produced an extremely instructive and readable book, in which the results of very extensive reading and observation are embodied. We most strongly recommend the work to all our Temperance Societies, as it would be a worthy companion to the treatises on the use and abuse of alcohol, by Drs. Carpenter and Spencer Thomson, which have already so much assisted the cause of Temperance by diffusing a correct knowledge of the action of alcohol on the frame.

We shall not fail to analyze Dr. Wilson's book at length, on some future occasion.

ART. VIII.—*Transactions of the Belfast Clinical and Pathological Society for the Session 1853-4.*—*Belfast*, 1854.

THIS is an admirable little volume, and contains many well-reported and interesting cases, and many valuable remarks and discussions on them.

In addition to the exhibition of morbid specimens, the Belfast Society have adopted the practice of proposing certain queries, to be made the subject of debate.

We shall probably make further use of this volume, and shall take the opportunity of comparing it with the 'Transactions' of the London Society.

ART. IX.—*Archives de Physiologie, de Thérapeutique, et d'Hygiène.*
Par M. BOUCHARDAT. No. II. Octobre, 1854. *Mémoire sur l'Action Physiologique et Thérapeutique des Ferrugineux.* Par T. A. QUEVENNE.—1854. pp. 356.

On the Physiologic and Therapeutic Action of Preparations of Iron.

THE first number of this serial was entirely occupied with the researches of MM. Hoinolle and Quevenne on "Digitaline, and its Action." The second number contains a not less elaborate inquiry by the last-named gentleman into the action of the ferruginous preparations. "Iron," says M. Quevenne, "is an essential constituent of the body; introduced into the stomach, its effect is to render the alimentary substances more easily precipitable, by contact with the serum of the blood, and more readily transformed into globules. Introduced into the circulation, it is a carrier of oxygen. In the ordinary conditions of life, sufficient iron is introduced with the food; when iron is deficient, it must be added to the food. Of all the insoluble preparations, the iron reduced to the metallic state by hydrogen (*fer réduit*) is most easily assimilated." These propositions contain nothing novel, but they are illustrated and placed on a secure basis by a vast number of carefully conducted experiments, which render this work a very valuable contribution to therapeutical knowledge.

ART. X.—*Lithotomy Simplified.* By GEORGE ALLARTON, M.R.C.S.
London, 1854. pp. 80.

THE mode of operating practised by Mr. Allarton is a modification of an old process, and is thus described :

“I introduce a grooved staff in the usual manner, and of the usual size, and confide it to an assistant, with directions to keep it perpendicular and hooked up against the pubes ; I then introduce the index finger of my left hand into the rectum, placing its extremity in contact with the staff, as it occupies the prostate, and press it firmly against the staff, so as to steady it ; then, with a sharp-pointed straight knife, with tolerably long and rough handle, I pierce the perineum in the middle line, about half an inch above the anus, or at such distance as may appear necessary to avoid dividing the fibres of the external sphincter,—I carry the knife steadily and firmly on till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger, which enables me to judge of the distance as the knife passes along. If the incision be not made exactly in the median line, the contracting fibres of the injured muscles draw the point of the knife from its direct line, and interfere with the accuracy of striking the staff. Having struck the groove of the staff, I move the point of the knife along the groove towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three quarters of an inch to one and a half inches, according to the presumed size of the stone—the escape of urine indicates the entrance to the urethra. I then introduce a long ball-pointed probe or wire through the external opening into the groove of the staff, and slide it into the bladder, to sufficient depth to insure its safe lodgment in that viscus, and withdraw the staff. I then well grease the index finger of the left hand and pass it along the probe, with a semi-rotary motion, through the prostate into the bladder ; which procedure is achieved without difficulty, and when the stone is free it comes at once into contact with the finger, and, if of moderate size, passes at once into the wound on withdrawing the finger, the patient having power to strain upon and thereby facilitate the extraction of the stone ; this last-mentioned power being one of the great advantages of this operation. The incision being made strictly in the median line, no muscles are divided, and the integrity of the bladder being preserved, it is under the control of the patient, who exerts, at the wish of the surgeon, a powerful propulsive effort which keeps the stone in or in contact with the internal extremity of the wound, where it is easily seized by the forceps and extracted by mild persevering traction. Now, as the aperture is necessarily the size of the finger which produces it, if the stone be large some other dilating power must be employed in addition to the dilating effect of the forceps and stone combined ; for this purpose Weiss’ three-bladed female dilator, Arnott’s hydraulic dilator, or, what is at once ready and effective, the addition of the vulcanized India-rubber finger-stalls one over another until the finger is sufficiently enlarged for the purpose, the outer covering being well lubricated with lard before being introduced. But Arnott’s dilator, where it can be procured, is by far the most efficacious though not the most expeditious means.” (pp. 22—26.)

The advantages of this operation are said to be—

“The impossibility of missing the bladder—the smaller amount of cutting than in the lateral operation—the neck of the bladder being uninjured—the smaller amount of blood lost—the prostate being merely dilated, not incised—the urine being at once passed by the urethra as well as by the wound, unless union by the first intention be effected—the facility with which the stone is reached, the patient being able to propel it towards the wound—the very short distance between the external opening and the interior of the bladder—the capability of breaking or crushing the stone, and washing out the bladder and freeing it from any minute particles—the small amount of pain—the absence of danger from urinary infiltration

—no muscle or vessel of any consequence being divided, no subsequent imperfection can arise—no danger of wounding the rectum—the rapid recovery, the patient being able to go about the next day—and the great facility with which the operation can be done by any practitioner of ordinary skill and ability.” (p. 34.)

Without using any dilator, Mr. Allarton has extracted a stone one inch and a quarter in diameter, and he believes a stone of two inches diameter might be extracted with Arnott’s dilator. For any larger mass he would suggest, it appears, crushing the stone, after the bladder has been opened.

We should have been glad to have been informed how many cases have been operated upon, how long each operation lasted, and how often the dilator has been used.

ART. XI.—*The Practitioner’s Pharmacopœia.* By JOHN FOOTE, M.R.C.S. Lond.

A LITTLE work, called ‘The Prescriber’s Pharmacopœia, is the best example we know of this class of books. Mr. Foote’s work appears to be also an extremely useful one. A great number of formulæ are given, and, as far as we have examined them, they appear to be well selected.

ART. XII.—*The Diagnosis of Surgical Cancer.* (*The Liston Prize Essay for 1854.*) By JOHN ZACHARIAH LAURENCE, Surgeon to the Northern and Farringdon Dispensaries.—London, 1855.

A PORTION of the money subscribed by the friends of the late Mr. Liston, in order to found some memorial of that distinguished surgeon, was applied in the establishment of a prize, for the best observations in clinical surgery by the students of University College Hospital. Although the Liston prize has been awarded only during the last three or four years, two of the prize theses sent in by candidates have already been published, and give us a very favourable impression of the surgical training of the successful competitors. The first treatise, by Mr. Gamjee, ‘On the Use of Starch Bandages,’* was an excellent work; and the treatise by Mr. Laurence, now before us, if it contain more doubtful points, and be liable to greater criticism, is so perhaps because the subject is one of far greater intricacy and obscurity.

We question, indeed, whether Mr. Laurence has done wisely in adopting so wide a subject as the ‘Diagnosis of Surgical Cancer’ for the subject of his thesis; or whether it might not have been better to have limited the debate to some part of the diagnosis—such as the symptoms, or the anatomical characters of the tumour respectively.

The diagnosis of cancer is considered under the following heads:

1. Diagnosis from study of the causes.
 - (a) Hereditary predisposition.
 - (b) Personal predisposition.
 - (c) Exciting causes.
2. Diagnosis from the symptomatology.
 - (a) Previous history, including the progress of the growth, and the patient’s health during that time.

* Vide No. xxv. p. 184.

- (b) Condition of patient when seen, as respects local phenomena (consistence and aspect of growth, pain, hæmorrhage, discharge), and general phenomena (cachexia, wasting).
- (c) Anatomy of the growth.

3. Characters obvious to the unassisted eye, and to the microscope.

Mr. Laurence then considers each of these points *seriatim*, and refers especially to the works of Lebert, Paget, Walshe, and Velpeau, and to his personal observations, for facts. We pass over the first of these chapters in order to give, by a quotation, some idea of the mode in which Mr. Laurence treats the difficult subject of the micrology of cancer. After describing the cancer cell as it exists in its most perfect form, Mr. Laurence makes the following remarks on the fibro-plastic cell:

“It is well known that fibro-plastic cells are not uncommonly found amongst the cancer cells of well-marked specimens of cancer. Indeed, as we shall hereafter have occasion to advert to, there are on record cases of tumours which were composed exclusively of fibro-plastic elements, yet proved subsequently quite as malignant as the most marked cases of cancer. Lebert, as is well known, strenuously insists that the cancer cell and the fibro-plastic cell are two distinct entities—distinct in their anatomical relations, distinct in their clinical import. But it is not uncommon to find cells which have as much the garb of one as the other, and now and then cancerous tumours will be found composed of caudate cells, scarcely, if at all, distinguishable from some forms of fibro-plastic cells. . . . And conversely, cells are sometimes observed in non-cancerous tissues, which cells, had they occurred in a surgical tumour, would at once have been pronounced prognostic of the worst to the patient from whom the tumour had been removed. Velpeau excised a portion of the calcaneum and astragalus for caries. M. Broca found abundance of cancer cells in some of the granulations. The case did perfectly well. I met with a case of precisely the same description. Mr. Erichsen excised the head of the femur for old-standing hip-disease. In some granulations coating the carious bone, I found some cells which certainly approached most closely the ‘cancer cell’ in their forms and proportions. . . . Another class of facts that has presented itself to my notice is, that in several tumours, the pathology and obvious anatomy of which stamped them cancerous, while I have found typical cancerous and typical fibro-plastic cells, I have found such different gradations between these two extremes, that it was often impossible to say to which type a given cell belonged. . . .

“From the above considerations, I venture to lay down the following proposition:—*That there do exist cell-forms, which it is difficult or impossible to refer either to the cancerous or fibro-plastic type exclusively; and, as a corollary, That the existence of such forms brings us to the conclusion, that the two forms of cell cannot but be regarded as the extreme links of a chain of forms connected by intermediate stages.*” (pp. 69, 70, 71.)

Very good drawings are given of these various cells.

At the conclusion of his work, Mr. Laurence sums up his opinions of the diagnostic value of the microscopical elements, by stating that, in most cancerous tumours, the so-called cancer cell will be found; that it may also be found in growths manifestly innocent; and that growths anatomically innocent—i.e., without cancer cells—may be clinically malignant.

We shall not at present discuss these opinions, but conclude this short notice by thanking Mr. Laurence for his little work, and by begging him not to think he has exhausted the subject, but to extend and confirm his observations.

ART XIII.—*Summary of New Publications.*

WE have already noticed the majority of the works received during the three months ending February 28th. The following have now to be mentioned:

In *Medicine*, Dr. Sibson has commenced the publication of an 'Atlas of Medical Anatomy,' of the same size and price as Maclise's 'Surgical Anatomy.' One number only has reached us, but from it we have no hesitation in concluding that it will be a work of the first order, and of the greatest utility. We defer a critical examination until other parts have appeared.

Lebert has issued three parts of a work* on 'Pathological Anatomy,' illustrated by some magnificent plates. It is to be published in about twenty numbers, and the price of each number will be fifteen francs; the cost of the entire work will be in England about 25*l*.

A very elaborate work on 'Foreign Bodies in the Air Passages,' has been published by Dr. Gross of Philadelphia. It is a complete summary of the whole subject, and will be an useful book of reference.

An important work, demanding special review, entitled 'Eutherapeia; or, an Examination of the Principles of Medical Science,' has been written by Mr. Garner. We hope to notice it in our next number.

Dr. Black has published the second part of his 'Pathology of the Bronchio-Pulmonary Mucous Membrane.' The subject is pulmonary tuberculosis. We shall notice the paper soon.

In *Surgery*, we have received one volume of M. Roux's posthumous work;† reparative surgery forms the subject of this volume.

Mr. Harvey has written a little work of two hundred and thirty pages, on 'The Ear in Health and Disease.' It seems to contain much useful information, and is altogether a better work than the last we saw from the same pen.

The fasciculi of the second edition of Mr. Maclise's beautiful 'Surgical Atlas' are appearing regularly, and we trust meet with universal approbation.

Dr. James Arnott has written another pamphlet on 'Benumbing Cold;' which contains, however, nothing new.

In *Midwifery*, a work by Dr. Meigs, on 'Child-bed Fever,' shall be reviewed shortly; and two German Inaugural Dissertations on 'Placenta Prævia,' and on 'Hæmorrhage from the Umbilicus,' shall receive due record in the next Report on Midwifery.

In *Materia Medica* we have only received the second division of the first half of the second volume of Werber's 'Specielle Heilmittellehre,' a work of very considerable utility, but which may perhaps be completed by the time the present readers will administer no more drugs. Mr. Swan's work on 'The Brain,' Dr. Frederick Bird's translation of M. Bouchut's work on 'The Diseases of Children;' a short elementary work on Chemistry, by Dr. Gregory; and the sixth, seventh, and eighth parts of the 'Micrographic Dictionary,' complete the list of books received during the last quarter. Of these, Mr. Swan's treatise, and Bouchut's work in its new dress, will be specially reviewed.

* *Traite d'Anatomie Pathologique.* Par H. Lebert. Liv. 1—3. Paris.

† *Quarante Années de Pratique Chirurgicale.* Par Ph. J. Roux. Paris, 1854.

PART THIRD.

Original Communications.

ART. I.

On the Mode of Development of Tubercle in the Lungs in Chronic Phthisis: its connexion with Fatty Degeneration of the Epithelium of the Air-Vesicles, and its Early Manifestation by Fatty Epithelial Cells in the Sputa. By C. RADCLYFFE HALL, M.D., F.R.C.P. E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay.

To ascertain with precision the earliest deviation from the healthy state in the part invaded by tubercle, is obviously a point of the highest interest and importance; and the difficulty of obtaining the information, at first sight, appears to be commensurate; for, when tubercle is already present, the condition of tissue which immediately preceded its deposition may have passed away, and when no tubercle yet exists, we cannot be quite sure that any would ever have ensued. The difficulty, however, diminishes when we consider that tubercle is deposited not all at once, but progressively; that it increases by accretion at its circumference; and, consequently, that the peripheric portion of a slow tubercle must be more recent than its centre. Certain differences in colour and density are visible to the naked eye; and with the microscope we can readily discern that every tubercle of tolerable size presents a series of different appearances, as it shades off in irregular zones, from complete tubercle to healthy lung. Knowing that any given tubercle, if progressive, would have continued to enlarge by the peripheric addition of tuberculous matter, we have a right to infer that the condition of that part of it which is nearest to healthy lung represents the earliest local morbid change—the first step in that local process which ends in the formation of tubercle. At all events, we approach as nearly to the facts as the nature of the circumstances will permit. It may be objected, that any given morbid condition around a tubercle may be the mere consequence of the morbid deposit already there, and so can indicate nothing respecting the phases of primary tubercular deposition. Even then we should learn the nature of the condition which favours increased deposit, after tuberculization has once commenced. But the objection can be disarmed of much of its force, for it is only valid provided the presence of inflammation, or that of any kind of heterologous formation indifferently, is adequate to produce a similar morbid condition: if not, then tubercle coincides with this condition, not because it is a mere morbid deposit in the lung, nor because of the inflam-

mation which often accompanies it, but because it is tubercle. Now, we can show that mere inflammatory exudation, in a lung without tubercle, is not surrounded by a similar morbid zone intervening between the solidified and the healthy portion of lung; whilst tubercle which has not yet any inflammation around it, is. Then, again, we can show that a morbid deposit which is neither inflammatory nor tuberculous—as, for example, the entozoic disease in the lungs of sheep—has no such morbid zone. On the other hand, in connexion with tubercle, we find the same morbid condition in some parts of a tuberculous lung which are not yet the seat either of tubercle or of inflammation; in the portion which forms the immediate margin of the tubercle; and, in the form of relic, in the mature tubercle itself.

Thus, one given morbid condition is found where tubercle does not yet exist, but where it would probably have ensued; and around existing tubercle where we know that more tubercle would have been formed; and vestiges of the same are met with amongst the elements of completed tubercle. All this suffices to prove, at least, an intimate connexion of some sort between the condition in question and tubercle.

Development of Tubercle.—Taking for examination any tolerably large distinct tubercle, we see with the naked eye that there is no abrupt line of demarcation between the healthy lung and the margin of the tubercle. A simple lens discloses further, that the tubercle has not any distinct and even margin at all, but has a jagged, irregular outline, from processes of

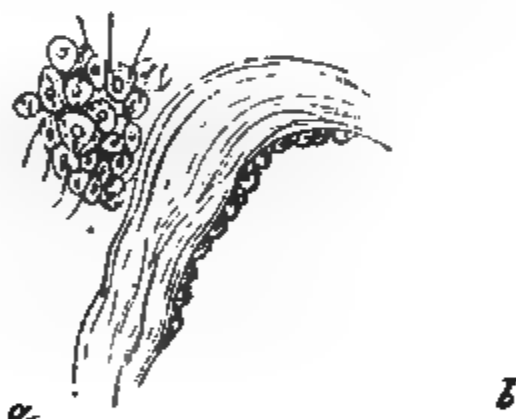


Fig. 1. Preliminary stages of tubercle. From soft, grey, uninfamed lung, bordering a yellow unsoftened tubercle. + 250.

a. Epithelium, scarcely altered.

c. Epithelium, more fatty and enlarged.

b. Epithelium, fatty.

d. Compound cells.

tubercle jutting out into the surrounding lung. By means of the microscope we perceive that the edge of one of these jutting processes of tubercle is not the limit of the morbid change; but that what, on cursory inspection, appears to be not unhealthy lung, immediately bounding the real tubercle, is in reality diseased, though not tuberculized.

Tracing onwards from lung which has no abnormal appearance to the centre of a large crude tubercle, we find as follows:

1. The pavement-epithelium of the air-vesicles is more nebulous.
2. Each epithelial cell becomes enlarged, more cloudy, more prominent when seen in profile, and is studded here and there with oil-dots.
3. The epithelial cells become still larger, and more fatty. In many of them no distinct nucleus can now be made out, but large dots of oil occupy its place. Some of the cells are detached, leaving the wall of the air-vesicle in one part bare, in another coated with compound tubercle cells. These are the preliminary stages of tubercle.
4. We now arrive at the completed tubercle, which consists of compound tubercle cells, small free nuclei in abundance, and granules; and occasionally, in addition, of a few fatty epithelial cells in various stages of disintegration; all being held together by a tough matrix.
5. So far, the deposit has been confined to the interior of the air-vesicles. It now invades the intercellular tissue of the lungs. For the first time, we find tubercle-corpuscles amongst the fine fibres which form the frame-work of the walls and septa of the air-vesicles. These fibres are here and there seen to be broken into lengths, and the entire tissue of the affected lung has become the seat of tubercle. In the first stages, tubercle is only intra-vesicular; at last, it is both intra-vesicular and interstitial. But it is to be remarked, that distinctly cemented in amongst the pulmonic fibres, we never find any of the compound tubercle cells, but only the free nuclei and granules.

The steps of the local morbid process, then, appear to be these:—Fatty degeneration of previously normal epithelium; shedding of this; its replacement by fresh epithelium, degenerate from the first, and rapidly becoming fatty; shedding of this; its replacement by large cells containing several nuclei; shedding of these; their replacement by free nuclei and granules, embedded in a structureless matrix. Up to this stage the tubercle is intra-vesicular only. The pulmonic fibres are next enclosed and separated by the morbid exudation, and free nuclei and granules are formed between and amongst them. The tubercle is now complete.

Tuberculization of the lungs thus commences as a degeneration of a normal tissue, proceeds as a production of this tissue in a depraved form; next, as an exudation capable of following only the lowliest process of organization up to maturity. Its ulterior changes from maturity, also, are those of the degeneration of a lowly-organized product; but concerning these I have dealt elsewhere.*

The structural representatives of these several stages, starting from normal epithelium, are the following, and may be considered in their natural order. They may not all be detectible at once in every mature

In the Annual Address in Medicine for 1853, delivered before the members of the Provincial Medical and Surgical Association.

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Fig. 2. Diagram of the stages of chronic tuberculation of the lungs.

1. Air-cell, with its epithelium healthy and adherent.
2. Ditto, with epithelium fatty and enlarged, still adherent.
3. Ditto, with fatty epithelium detached in places; compound tubercle cells upon wall of air-vesicle in places; wall bared in places.
4. Ditto, ditto, with exudation of tubercle-blastema into air-vesicle.
5. Complete tubercle, bounded by irregular line of black pigment consists of air-vesicles and their septa filled and infiltrated with blastema, with compound tubercle cells and fatty epithelial cells, as well as free tubercle-nuclei and granules in the air-vesicles, but only the free nuclei, granules, and black pigment interstitially amongst the pulmonary fibres.

tubercle, particularly if softening be commencing; but they are to be found in various proportions in the great majority of tubercles, provided we include along with the tubercle a certain portion of the pulmonary tissue around it.

Constant constituents of mature pulmonary tubercle at some period of its course:

1. Normal epithelial cells becoming fatty.
2. Fatty epithelial cells.
3. Many-nucleated cells.
4. Free nuclei.
5. Granules.
6. Matrix.

Frequent constituents:

7. Small bloodvessels in a state of fatty degeneration.
8. Red blood-corpuscles, and orange-brown pigment.
9. Black pigment.
10. Granule cells, and glomeruli.
11. Induration-matter.

1. *Normal Epithelium becoming Fatty*.—The existence of an epithelium at all in the air-vesicles has been questioned by physiologists of high repute, but is now very generally admitted. Blood-corpuscles seen through the walls of the capillaries were indicated by Mr. Rainey as having possibly been mistaken for epithelial cells. Such an error cannot arise when the lung of a bird, or of an amphibian, is examined. The even oval outline of the large and clearly-defined blood-corpuscle is too distinctive. In the frog, the epithelial cells are coarser, dimmer, and more separated from each other, than in the bird or mammal; having reference, perhaps, to the greater expansibility of the lung-sac. In the bird, the cells run one into another at their margins, showing only a faint outlining as their mark of division. In man also, the outlines are less sharply defined than in most other varieties of pavement-epithelium; but the flat cells are bounded by a dim line of limitation. In appearance they are thin, almost transparent, and have a slightly nebulous, somewhat ill-defined nucleus, very different from the bright sharply-cut nucleus of the pavement-epithelium of the mouth, for instance. A nucleolus is not distinctly to be made out. In size and shape the cells vary greatly; smaller and rounder when young, they become rather longer, flatter, and more angular with age. Pentagonal, hexagonal, or polygonal, with angles more or less acute or round, according to their mutual fitting into each other; their general character is, that they constitute a fine, but dimly-defined, pavement-epithelium of a single layer. Whether this internal cuticle undergoes any regular process of desquamation and reproduction in health, is unknown. In all probability, it behaves very much like the pavement-epithelium of serous membranes, in being for the most part persistent, and only cast off and renewed when accidentally incapacitated. Like serous epithelium, too, it permits the transudation of a thin watery halitus (with the especial addition, in its own case, of the gases of respiration), whilst it prevents the passage of complete blood-plasma, acting in this respect as a natural defensive coat of elastic cement.

When becoming fatty, the flat epithelial cells first appear better defined at their edge, and more nebulous at their centre. Next, they are larger, plumper, and more distinctly separate one from another, though still adherent. Oil-drops, of different sizes, spot the whole of the nucleus, which either stands out in relief as full of oil-dots, whilst the surrounding

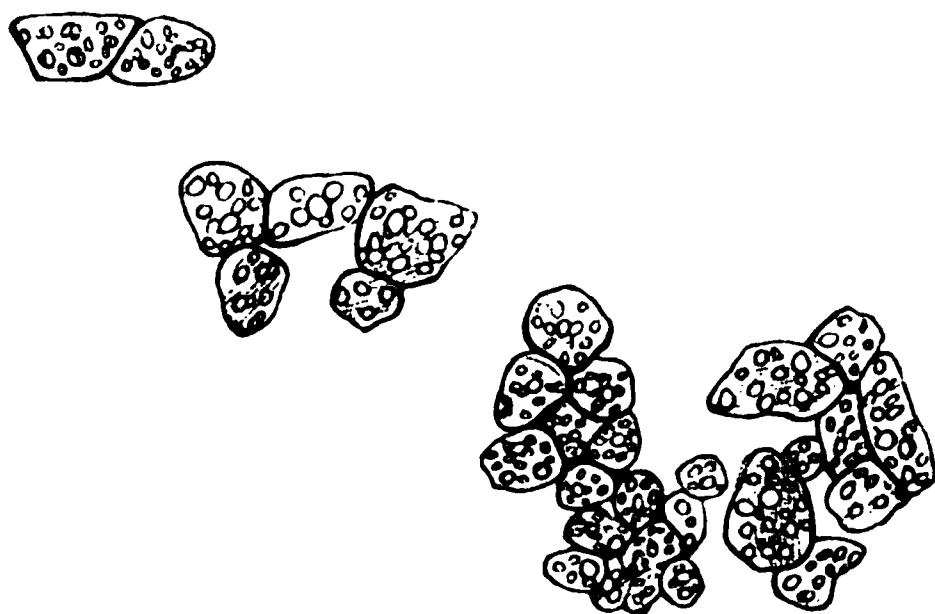


Fig. 3. Flakes of fatty epithelium from lung adjoining tubercle, showing the various sizes of epithelium cell when becoming fatty.

portion of cell is only nebulous; or it is apparently converted into one large oil-dot; or, it is obscured or lost by general oil-dotting of the entire centre of the cell, the marginal portion only remaining clear.

2. *Fatty Epithelium degenerate from the first.*

—Nearer to the tubercle than those air-vesicles which are lined with normal epithelium, which is

becoming fatty, we find other air-vesicles, containing a quantity of detached epithelium, yet still having an epithelial coat on their walls, presenting cells in various stages of fatty degeneration. Some of these are small and round, evidently young, yet fatty. In many, the nucleus is not fatty, whilst the cell is. In others, the whole is fatty, both cell and nucleus; the situation of the latter being still traceable by the clustering of oil-dots there. Lastly, the fatty spotting is so universal, that nothing remains to indicate where the nucleus had originally been. Although we have no right to infer that all these forms may not be presented by the original epithelium in different stages of fatty degeneration, it is certain that there are successive generations of epithelial cells which, almost from the period of their formation, begin to fattily degenerate, indicating a proneness from the first to fall into this kind of atrophy. In the first instance, the normal epithelium has existed indefinitely before it becomes fatty and is shed. In the last, the newly-formed epithelium is unable to maintain a normal life for more than a brief period of uncertain duration; it early becomes fatty, even whilst its growth is yet incomplete, and doubtless is rapidly shed, to make way for a fresh tribe of increasingly degenerate epithelial cells.

I suspect that those cells in which we find the nucleus alone fatty, are the degenerated original normal cells; and that those in which we see the surrounding cells largely fatty, whilst the nucleus is less or not at all so, as well as those in which all is alike affected with fatty degeneration, represent the subsequent tribes of degenerate epithelial cells. As countenancing such an opinion, I may state that I have found the earliest evidence of morbus Brightii in renal epithelial cells discharged in the urine, in which the nuclei alone were fattily degenerated.

Passing by this supposed distinction, the fatty epithelial cells, taken indiscriminately, present considerable variety in appearance and size. The following may be given as examples (fig. 4):

a. A middle-sized, sub-angular, well-defined plate, having a large oil-globule in place of nucleus; the rest being merely nebulous near this oil-globule, and clear near the circumference.

b. A rounder cell, either small or middle-sized, having a large oil-globule in place of nucleus, and remainder of cell dotted with oil-dots of different sizes.

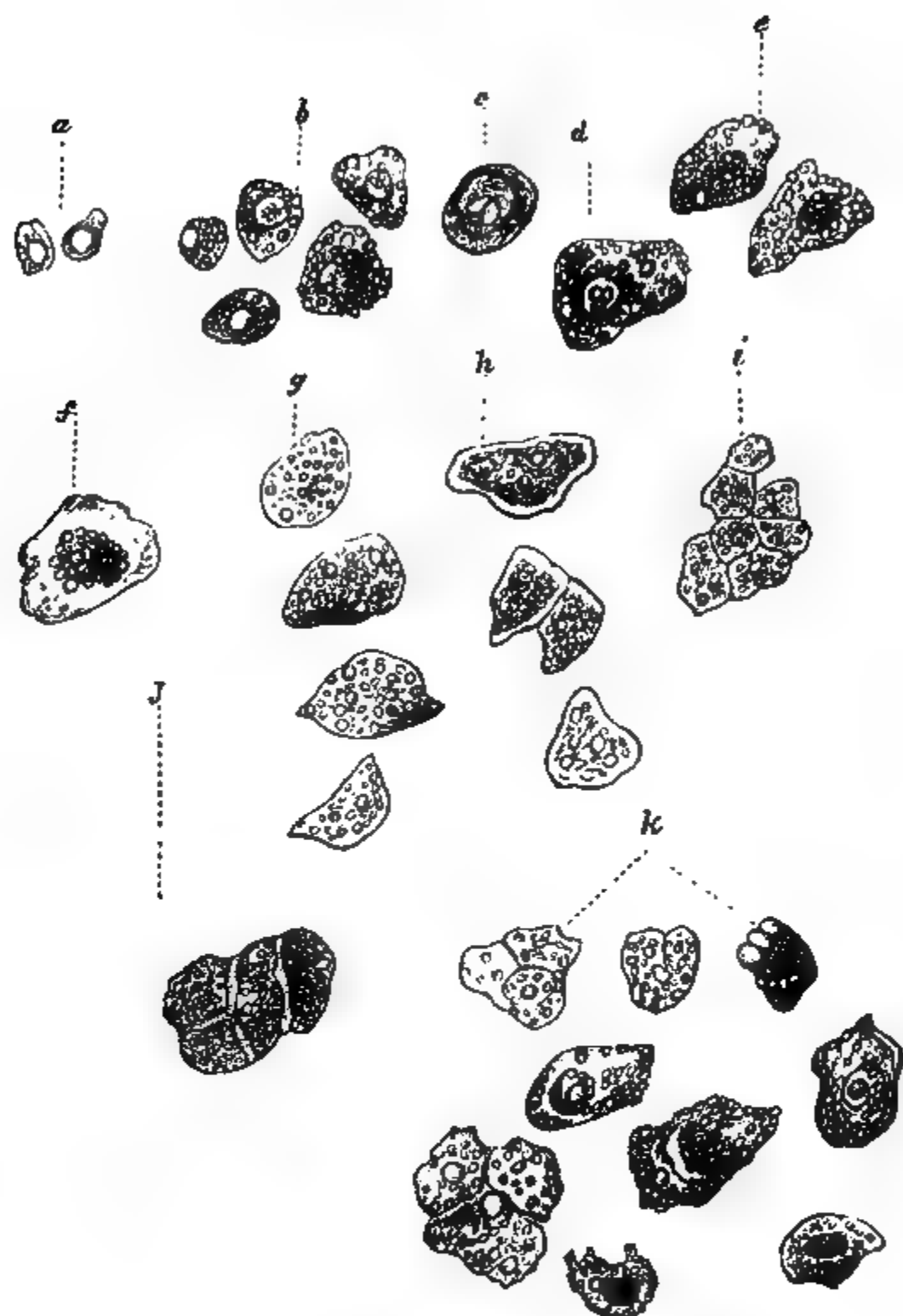


Fig. 4. Various forms of fatty epithelial cells, single and in flakes, as described in the text.

c. A flat cell, not generally fatty, containing two or three oil-globules in its centre.

d. Flat-cells of very irregular outline, having a large nucleus with central oil-dots; rest of nucleus clear; cell around nucleus full of oil-dots of varying size.

e. The same as the last, but whole of nucleus fatty, though still discernible.

f. An irregular cell, not at all fatty, but having very large nucleus dotted all over with fat dots.

g. Flat cells, of various but irregular outline, wholly fatty; nothing in place of nucleus detectible. Oil-dots very variable in size.

h. The same as the last, but with a margin of cell all round, not fatty, but dimly clear.

i. Flakes of detached epithelium, showing the cells altogether fatty, having a sub-angular outline, still adherent at their edges; very varied in size and shape, but fitted one into another without any interval of separation.

j. Similar detached flakes, but the fatty epithelial cells more swollen, and, though still coherent, showing a linear interval of separation from each other, resembling in this particular the lung of the batrachian.

k. Fatty epithelial cells, apparently disintegrating.

In size, the fatty epithelial cells vary so considerably, that no standard can be given as the average. A very few may be found smaller than a medium-sized normal lung-epithelial cell: by far the majority are larger, and some much larger. The smallest and the largest are generally the least angular in outline; the one perhaps from youth, the other from distention. Still, singularly-shaped cells, with abruptly cut margins, seeming to indicate that the cell has been partially disintegrated, are seen of all sizes.



Fig. 5. Bronchial columnar epithelium, from lung adjoining yellow tubercle. + 450.

Bronchial columnar epithelium is found freely in the several conditions of withered but not fatty; fatty and swollen; partly fatty and partly withered. In some of these detached bronchial epithelial cells the nucleus alone is fatty; or the columnar part alone is fatty, the nucleus being merely granular; or the whole is made up of oil-globules of different sizes, no nucleus being left.

Of course, more might be observed on the minute differences presented, but the above may suffice to establish the fact that what is thus described really is epithelium in a state of fatty degeneration, and not any of the morbid cell-forms which result from inflammation, and are generated in inflammatory exudation. In answer to the first question which is usually put, How are these cells to be distinguished from the granule cells of inflammation? it is to be remarked, that the easiness of making the distinction depends entirely upon the individual cells selected. A *young* gland-cell in one viscus is very much the same in appearance as a *young* gland-cell in another, however clear may be their ultimate dis-

tinctiveness at maturity. So, also, of pathological cells: one may find some specimens in every diseased product not distinguishable from others belonging to a very different disease. We are ruled by the form of mature cell which preponderates, and take that for the type. So here, by the same rule, we find certain cells which are so manifestly epithelial cells more or less fattily degenerated, and we can trace such marked gradations in them, that we need feel no hesitation about our conclusion because a few of the cells, if taken alone, could not with certainty be placed in the same class. There is no room for doubting, for instance, the nature of a flake of pavement-epithelium in which the cells still cohere and fit in by their edges, whilst they present various degrees of fatty degeneration. Neither can any doubt exist when we are viewing fatty columnar epithelium from the adjacent bronchi.

3. *The compound Corpuscles of Tubercle, or many-nucleated Cells.*—These are large cells, which contain several separate nuclei. We do not find many of them in the mass of mature tubercle, and what there are lie here and there, and not in aggregated heaps. Neither do we find them in those air-vesicles which are nearest to healthy lung. Here the epithelium is only fatty. But between the air-vesicles which contain the fatty epithelium and those which are crammed with complete tubercle, these cells are numerous.

They were first described by Virchow in 1851, and by Van der Kolk in 1852. The former mentions “cells with five large, oval, granulated, nucleolated nuclei.” The latter states that the cells nearest the wall of the air-vesicle are the smallest, contain only one nucleus each, but when cast off increase greatly in size by the imbibition of fluid, and are filled for the most part with numerous nuclei. “The cells which are placed in the middle of the air-vesicle are thus the oldest—i.e., they are farthest removed from its walls, longest exposed to the influence of the surrounding fluid, and thus, also, the largest.” By both observers these cells are considered to be morbid epithelium; and by both the contained nuclei are believed to be set free by dissolution of the enveloping cell, and then to constitute the small cells described by Lebert and others under the name of tubercle-corpuscles. For the further interpretations, somewhat differing, of the two authorities, I must refer to the only sources of my own acquaintance with them—the accounts severally given by Dr. Jenner* and Mr. Paget.†

My own observations lead me to conclude—(1.) That the many-nucleated cell may be found of the largest size, and containing its largest number of nuclei, whilst close and adherent to the wall of the air-vesicle; (2.) That no successive strata of cells, becoming more and more nucleated as they advance free from the wall towards the centre of the air-vesicle, can commonly be made out; (3.) That no distinct lamination of cells of any kind upon the wall of the air-vesicle, as if they had been thrown off in successive and distinct layers, is observable; (4.) That cells equally large, equally centric as regards the air-vesicle, are numerous, in which either only one nucleus, or none at all, is discernible, the whole being in some stage of fatty degeneration; (5.) That a rather small cell may contain several

* British and Foreign Medico-Chirurgical Review, vol. xi. p. 186. 1853.

† Ibid., vol. xii. p. 196; and Lectures on Surgical Pathology, vol. ii. p. 595.

nuclei, whilst one much larger, as just remarked, may have only one; (6.) That whether or not the nuclei of the many-nucleated cells, if set free, would be identical with the free tubercle-corpuscles, they are not the principal, still less the only, source in which these originate.

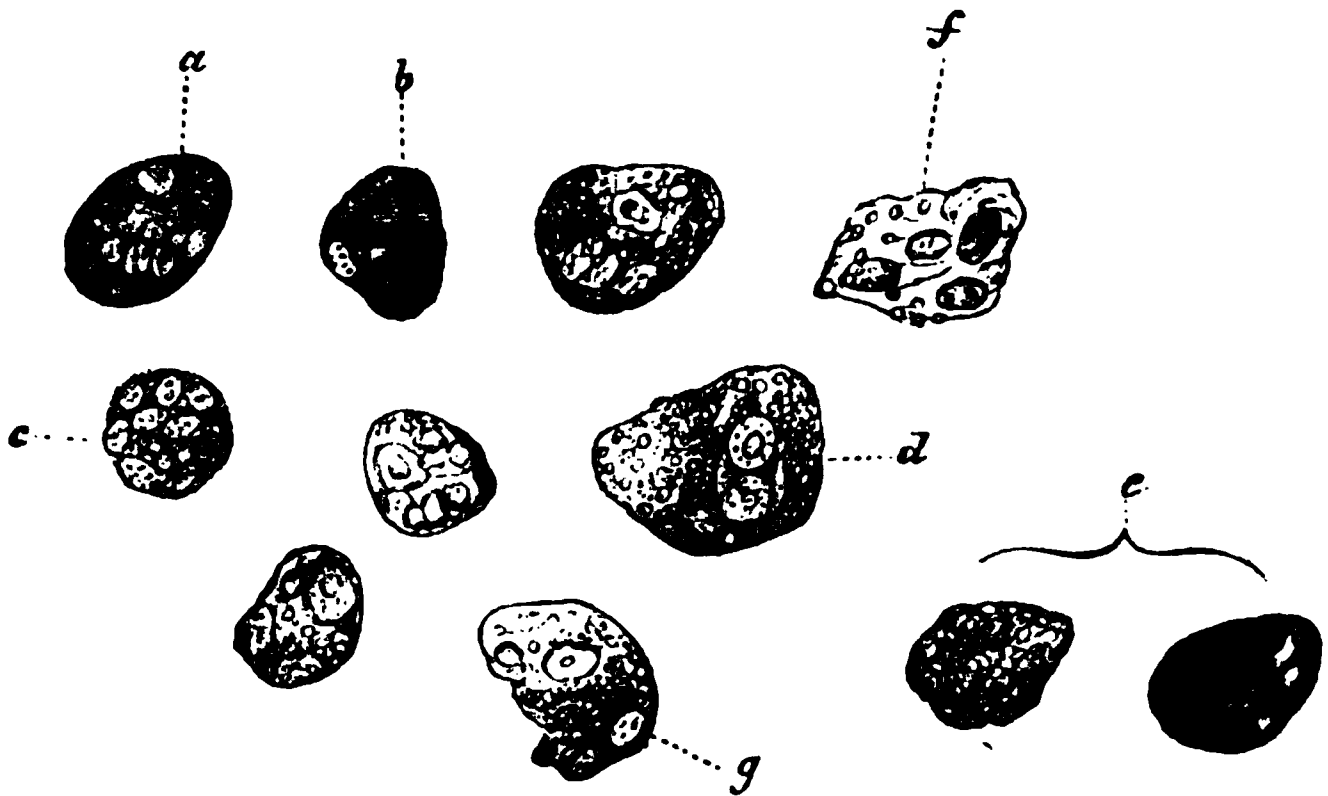


Fig. 6. The many-nucleated tubercle cells. + 450.

- a. A plump cell, nuclei rather dim.
- b. Another with one lateral nucleus, very distinct, and closely resembling a free tubercle-corpuscle.
- c. An aggregation of coherent nuclei, but no distinct parent-cell-wall around them.
- d. A large cell, having a second cell enclosed with two nucleated nuclei, taken from a highly organized tubercle—the grey miliary, with fibrillated matrix.
- e. The same cell at different foci.
- f. An apparently shrivelled compound cell.
- g. A compound cell, partially disintegrated.

The many-nucleated cells, or compound corpuscles, are darker than any other of the cell-forms seen in examining tubercle. They have often a clouded, yellowish-brown aspect, and at first glimpse are readily mistaken for large glomeruli. They are usually plump and roundish, ovoid, or pyriform, and strikingly differ in appearance from equally large, but flatter and more or less angular, epithelial cells which are only fatty. Their nuclei lie at different depths, and cannot all be seen distinctly at once. Distinct and in sharp relief, indeed, it is only by chance that any of them can be seen, owing to the dense nebulousness of the cell in which they are contained. So far as can be distinguished, these nuclei, whilst within the cell, are ordinarily larger, plumper, and more regular in outline than free tubercle-corpuscles; they look less hard and compact, and their granules less distinct. Occasionally, however, we catch sight of one close to the wall of the containing cell, which appears to resemble closely an ordinary free tubercle-corpuscle. And, in examining large miliary tubercles, grey throughout, I have sometimes seen a faded, shrivelled, flat, semi-transparent cell, no longer granulous, in which lay three distinct very characteristic tubercle-corpuscles. (Fig. 6, f.) This I took to be a compound cell, on the eve of dissolving and setting free its nuclei. It is not unfrequent for one of the nuclei in a compound cell to be larger than the rest, and to possess a nucleolus. There may even be two nucleolated nuclei side by side in an oval cell, contained within a large com-

pound cell having the ordinary non-nucleated nuclei. This form is rare. I have only found it in the highest type of tubercle—viz., the grey milary, with fibrillated matrix. (Fig. 6, *d*.) Usually, there is only one nucleolated nucleus in a compound corpuscle, however numerous the fellow nuclei may be. This nucleolated nucleus may be supposed to represent the original nucleus of the epithelium cell; the remaining non-nucleated nuclei being formed secondarily, as the cell increases in size, from mere corpusculatation of plasma imbibed whilst the cell is still adherent to the wall of the air-vesicle. Whether such growth of cell, and multiplication of nuclei within it, also go on after detachment from the wall of the air-vesicle, cannot, I believe, be decided either way by anything we can find on inspection. If the cell can grow after severance from the place of its birth, or if it be, from the time of its detachment, large enough to swell out by mere imbibition, there is no reason why the imbibed plasma should not corpusculate as readily on the inside as we find it does in the matrix on the outside of the cell. There is no ground for supposing, in any case, that the several nuclei result from fission of the primary nucleus, since that, as already noted, is very often still present and increased in size. I suspect that most of the compound cells complete their development whilst still adherent, and only increase in growth subsequently.

This compound cellulation is by no means peculiar to tubercle. In a more marked form, it is common in cancer; but, in very similar form, it may be met with in the cheesy secretion of the tonsils—in that of a sebaceous follicle—in healthy granulations, both in man and animals (on a horse's broken knee, for instance)—and in the plastic exudation surrounding the entozoa so constantly present in the lungs of sheep. Neither is it essential to tubercle. In a thoroughly tuberculous subject, in the midst of a thick adhesion, which connected the base of the left lung in front to the diaphragm, I found three triangular portions of true adipose tissue; and in the centre of each a distinct crude tubercle. In each

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Fig. 7. Three tubercles in an adhesion between lung and diaphragm. Natural size.
a. a. Lung. *b.* Tough, firm, pinkish, gelatinous adhesion-matter.
c. Fat. *d.* Tubercle. *e.* Diaphragm.

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Fig. 8. Section through adhesion and fat-wall into one of the tubercles. + 250.

a. Adhesion-matter

3. Adipose tissue.

c. Tubercle, containing fat globules, free tubercle nuclei, no compound cells, and but few granules.

of these tubercles, characteristic free corpuscles and granules set in their matrix, formed the entire mass. There was not a compound corpuscle in any one of the three tubercles. Evidently, the course of events had been this:—The lung, having been made an abnormal fixed point, by adhesions and tuberculous consolidation occurring at the last stage of phthisis, the diaphragm had had a tendency to drag asunder the newly-formed adhesion-matter. As this opened out into areolar tissue, fat was deposited; and into this loose fatty areolar tissue, plasma subsequently found admission, and corpusculated *more suo* into yellow tubercle. There was no epithelial surface engaged, and there were consequently no compound cells. So, likewise, when pulmonary tubercle has become interstitial, we do not find the compound tubercle-corpuscle amongst the lung-fibres. Neither do we find it in sub-peritoneal tubercles. As Virchow (and

also, I believe, an English pathologist of equal eminence) has found the many-nucleated cells constantly in tuberculous lymphatic glands,* we may perhaps infer that, although not essential to tubercle, and consequently not the crucial fact of its occurrence, this compound cellulation is still an habitual feature of tuberculization at some stage *when it attacks an epithelial surface*, such as that of the lungs and of the lymphatic glands. That this circumstance is due rather to the accident of place, than to the kind of morbid crisis on which the disease depends, is deducible again from the fact, that the typhus-matter in Peyer's glands occasionally presents a similar phenomenon, amongst its several other points of resemblance in microscopical appearance, to the lower forms of tubercle.

It might be a question whether all of the compound corpuscles of tubercle do originate as described, acting as parent cells to endogenous nuclei. Whether in some of filmy appearance, and which are devoid of any nucleolated nucleus, the free nuclei were not the first in the field, becoming secondarily enveloped in a film of plasma whilst in contact with the wall of the air-vesicle: just as we see shrivelling blood-corpuscles become thus encased on their way to form pigment cells.

We may fairly assume that the formation of the many-nucleated cells, whether it take place in one, or other, or both these ways, implies the exercise of more organizing power, and therefore the presence of greater vitality, in the parts, than does the production of free nuclei only.

4. *Free Nuclei, or Tubercle-Corpuscles Proper.*—These are the small single cells which make up the bulk of every variety of mature tubercle. In appearance they are irregularly round, oval, or oblong; or bean-shaped, or polyhedral, or altogether irregular; but always without sharp corners; their shape varying apparently according to their age, the kind and condition of tubercle, and the degree of pressure from close packing to which they have been subjected. They are more regular in figure in the jelly-like tubercle occasionally found in the most acute forms of phthisis, in grey miliary tubercle, and in yellow tubercle just beginning to soften; and most rounded and plump when softening has fairly set in. Least regular, in friable yellow tubercle, when it is commencing the process of dry obsolescence. They have a sharp, compact outline, are semi-opaque, and contain several very distinct dispersed granules, but, commonly, no separate and definite nucleolus. In size, they vary very considerably, from that of a very minute cell, specked with molecules, up to that of a pus-corpuscle. The great majority are rather smaller, regard being had to their constant difference in figure, than a red blood-corpuscle. They are consequently the smallest of the typical pathological cells;—exudation granule cells, pus cells, and cancer cells, *when fully developed*, being usually much larger than these tubercle-corpuscles. Those are also, all of them, nucleated cells; the unnucleated specimens being the exceptions. Tubercle-corpuscles, on the contrary, as a rule, are not nucleated (or nucleolated, if we name the corpuscles themselves nuclei); those which are so, are the exceptions. As they have not the size, neither have they the plump rotundity of most of the other morbid cells in their perfect state. Still, the tubercle-corpuscle is not a shrivelled, withered-looking cell; but gives one the impression of its being a small, tough cell, which has grown up to its present size and figure, and could not grow beyond it; and not of a cell which

* My own observations have been almost limited to tuberculized bronchial and mesenteric glands. In these, I have always found some compound cells.

has passed through a higher stage of development, and is now going down by some kind of degenerative process. In this respect, it differs entirely from shrivelled-up inflammatory granule cells, or old pus cells. These may have assumed the small size of the tubercle-corpuscles, and have lost their roundness; but then they look shrivelled, and have an uneven irregularity of outline; whereas the tubercle-corpuscle, whatever its irregularity of outline, is even, and shows distinctly the few rather large granules within it, and never looks muddy or clouded with minute molecules. Moreover, after swelling them out with water, and using diluted acetic acid, a nucleus can ordinarily be made out in the other cells, however shrunken they may be; it cannot, for the most part, in tubercle-corpuscles.

The granules contained in a tubercle-corpuscle, prior to the period of softening, cannot be made to move about within the corpuscle by any mode of manipulation. It is hence inferred that the corpuscle is filled, not with fluid, but with solid matter, which is identical, in all probability, with the external matrix.

Tubercle-corpuscles may be defined as—small, irregular-shaped, well-defined, semi-opaque, motionless-granule-bearing, unshrivelled, non-nucleated cells. Only one other kind of morbid cell could be included under this definition, and that is the cell more sparingly found in typhus-deposit.

Nucleated Cells.—Cells which contain a distinct nucleus are found more or less abundantly in many tubercles. The following varieties may be noticed:

a. A small roundish or oval cell, neither granulous nor fatty, having a bright oval vesicle in its centre.

b. Cells containing distinct granules, and only distinguished from the common tubercle-corpuscles in that they are longer and more regular in shape, and present a round or oval nucleolated nucleus.

c. Smaller cells than the last, bearing distinct granules, and in every respect like the free tubercle-corpuscles, excepting that a bright vesicle appears within them, either at the centre or nearer one end. (Fig. 9.)

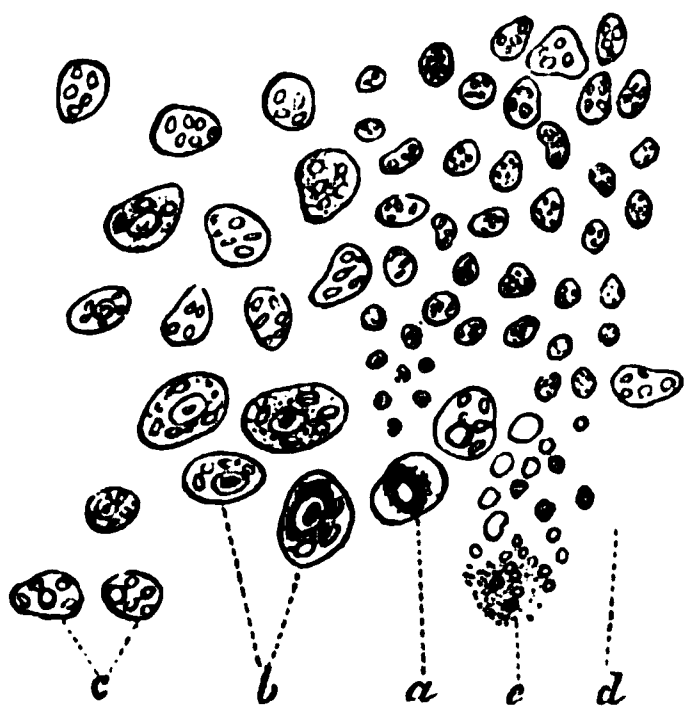


Fig. 9. Free tubercle-corpuscles. + 450.

a. b. c. Varieties of nucleated cell, as described in the text.
d. Non-nucleolated nuclei.
e. Free molecules.

Are these and such-like nucleated cells different stages of morbid epithelial cell, or of the tubercle cell, or of some superadded inflammatory granulous cell, or of modified pus?

That they are neither inflammatory globule nor pus, is clear from their presence in unsoftened tubercle, and most abundantly in grey miliary tubercle, which has no inflamed lung around it. Before stating what they probably are, we must inquire into the mode of origin of the free unnucleated tubercle-corpuscles. These are referred by Virchow and Van der Kolk to the disintegration of the many-nucleated cells having set free their nuclei, of which they are consequently the parent or brood-cells.

Some of the free corpuscles may originate in this manner, and may hence, with propriety, be designated free nuclei; but all, and probably the greater number, can scarcely so originate; for, we find the characteristic tubercle-corpuscles in masses in which no many-nucleated cells exist, and, as far as can be ascertained, never did exist—e.g., in the instance of tubercle in adipose tissue, already mentioned (figs. 7 and 8), and in the interstitial part of the deposit in pulmonary tubercle (fig. 2), and in sub-peritoneal tubercle;—and we also find miniature specimens in all degrees of the free corpuscle, smaller than we can ever make out amongst the nuclei contained in the large compound cells. These are not to be looked upon as young cells which would afterwards have developed into larger free nuclei, but as such nuclei made very small at first.

I would suggest the following explanation. The vitality of exuded plasma is partly inherent, partly dependent upon the adjacent living structure. When tuberculous plasma is exuded upon a surface whose normal office is that of forming epithelium, its subsequent cellulation follows, as far as its own defective capability permits, the type of epithelial cell-formation; presenting, as the result, numerous aberrant forms of nucleolated-nucleated cell, or merely nucleated cell. When such a cell is small, and contains a few granules, it constitutes what has been described as “a nucleated tubercle-corpuscle.” But when tuberculous plasma cellulates at a distance from the wall of the air-vesicle, it forms only that kind of lowly cell which its own unassisted capacity permits,—and that is, the unnucleated tubercle-corpuscle. And when, under the progress of disease, the pulmonary tissue has had its vital force too much impaired to afford anything towards organizing tuberculous plasma into its higher corpusculate forms, then, also, the exudation, although close to lung-tissue, may generate only the unnucleated corpuscles. Moreover, as all plasma will corpusculate when exuded, and as the resultant corpuscles will tend towards nucleation, in proportion to the goodness of the plasma, it is possible that some portion of the plasma poured forth in a phthisical subject may have the capacity within itself of forming small nucleated cells amongst the elements of tubercle, irrespective of any influence of the pulmonary tissue. In either case, the presence of nucleation may be taken to indicate a higher measure of vitality, or organizing force, in the tubercle cells which possess it than in the rest.

Such of the nucleated corpuscles as may originate in the way we assume, are not retrograde, nor degenerating, nor even tubercular epithelial cells, strictly speaking, because they have never been developed higher than we now see them. But at their origin, they may be said to have inherited a tendency towards the type of epithelium, although they never fulfil it.

Other tubercle cells may be nucleated in virtue of inherent capacity of plasma; these, probably, are the rarest. Some, again, are probably nothing more than the nucleolated nuclei of former compound corpuscles set free by disintegration. The diversity in appearance presented by the different forms of nucleated cell in tubercle, quite countenances the supposition that there may be several modes of origin for them.

The nucleated cell is not essential, or even in any sense characteristic of tubercle, for it is not always present; and seldom abundantly so, except

in certain specimens of fibrillated grey tubercle. But it does not follow that when present, it is not equally one of the elements proper to tubercle as any other.

The small non-nucleated cells, free nuclei, abortive cytoblasts, or tubercle cells proper, may be looked upon, with Lebert, as the especial characteristics of tubercle. In variable proportion, they are to be found in every tubercle; and if they were not also met with in typhus-deposit, might be considered as the pathological element which was peculiar to tubercle. In typhus-deposit, however, these corpuscles do not constitute the bulk of the exudation; molecular detritus in abundance, a few compound cells, and a few of these free nuclei, are mixed up together in a less firm matrix. Whereas in many tubercles, these corpuscles, set in a tenacious matrix, are the predominating elements.

Of these tubercle-corpuscles, some are probably nuclei which were formerly contained in a brood-cell now dissolved away; but the majority have been self-originated in an exudation of tuberculous plasma. Excepting tuberculous plasma, no other kind has the tendency to generate in abundance free unnucleolated nuclei like these. In common inflammatory exudation, we may find a few young cells not nucleated, but they are either round, or oval, or shrivelled; and in degenerated inflammatory cells, as already remarked, a nucleus may generally be detected. In cancer, here and there, a free nucleus might be undistinguishable from a tubercle-corpuscle, but there are always many other cell-forms in greater quantity. Any deposit, of which the *greater portion* consists of free nuclei answering to the description given, may be justly considered to be tubercle.

Whatever may be the resemblance between various other morbid deposits, when in a state of degenerative change, and tubercle; in their typical condition, all of them can be distinguished from tubercle, and *vice versa*. There is, therefore, no evidence whatever, that any exudation which was not stamped as tuberculous at the first, can ever change into true tubercle subsequently. It may certainly soften and lead to ulceration, and pursue, in other respects, much of the course of tubercle, and end in the same way; but all that does not establish identity of nature.

5. *Granules*.—The free granules are too minute for any structure to be discerned in them. They are irregular in shape and size, but always exceedingly small, and are probably nothing more than aggregations of atoms, without any definite arrangement. From their reactions, it is concluded that some are fatty, others albuminous in composition. They are comparatively few in grey miliary tubercle; in the ash-coloured, jelly-like tubercles of some cases of acute phthisis; abundant in the yellow tubercle of acute phthisis; in cheesy tubercle generally; and in destructive softening in all stages.

Are these free granules the detritus of pre-existent corpuscles, or primordial elements of tubercle? Probably both. As they are less abundant in young, and more abundant in old, degenerating tubercles, there is some reason for viewing them as in part the remains of corpuscular elements which have undergone disintegration. On the other hand, they abound in one degenerate form of tubercle at its earliest stage—viz., in the small yellow deposits of acute phthisis, where there has not

been time for corpuscular disintegration to have taken place; and where we must consider the free molecules as primordial. We find, moreover, in this case, both the albuminous and the fatty molecules, and cannot therefore view the one variety, rather than the other, as the result of disintegration. So much of the plasma as can neither corpusculate into cells, nor coagulate into mere matrix, nor be absorbed, probably granulates into these minute molecules.

6. *Matrix*.—If we move about the tubercle-corpuscles under the microscope, we see that they are held together by something which is transparent, homogeneous, structureless; which, in the yellow tubercle presents no appearance of fibres or of fibrillation, and has no adherent nuclei; but, in the well-defined grey semi-transparent tubercle, is finely fibrillated. In the mature tubercle, this matrix is firm and solid; in the small grey miliary tubercle, it is as resistant as foetal cartilage; in the recently-formed tubercle of certain cases of acute phthisis, it is less firm, and is sometimes found of the consistency of tough jelly; and in softening tubercle, the matrix gradually loses its tough consistency, and finally becomes liquid.

Fig. 10. Fibrillated matrix, containing nucleated and non-nucleated tubercle-corpuscles, from grey miliary tubercle.

7. *Small Bloodvessels in a State of Fatty Degeneration*.—I have hitherto found these once only in large grey semi-transparent fibrillated miliary tubercles, which lay as distinct nodules in uninflamed lung.* Near to the portions of fattily-changed vessels, shrivelled red corpuscles, and orange, as well as black, pigment, lay amongst the tubercle corpuscles in the tenacious fibrillated matrix. The tubercles were of larger dimensions than miliary tubercles ordinarily are, averaging the size of duck-shot, and including, of course, within each many air-vesicles. The fatty bloodvessels were unmistakable, presenting a clear, defined outline, dividing into branches of considerable length, and having no openings or connexions with the adjoining elements. The centre of the vessel was bright and translucent, and the whole was studded with dim granules and oil-dots of various sizes. The patient had suffered from copious and repeated hæmoptysis two years before his death.

Considering how many cases of phthisis spring into noticeable activity coincidentally with an attack of hæmoptysis, the patient so commonly declaring that his chest was strong and his health good until suddenly he "broke a bloodvessel," which led to consumption; this fact of fatty degeneration of small bloodvessels, where no inflammation exists, at the earliest stage of the highest form of tubercle, whilst as yet it occasions neither pain nor organic irritation of the lung, is valuable and explanatory. No doubt such a degenerated vessel, under some temporary muscular exertion or mental excitement, does literally "break," and the flow of blood is then a veritable hæmorrhage from a patulous vessel, and not a mere transudation, like that through the mucous membrane of the stomach in coffee-ground vomit. If so, the popular phraseology is correct.

I have never found bloodvessels in a yellow tubercle; and these fattily

* I have also found fattily-degenerate bloodvessels in the walls or large cavities; and occasionally shreds of the same amongst the contents of closed cavities.

Fig. 11. Small bloodvessel in the state of fatty degeneration, from large grey military tubercle, within which the walls of the air-vesicles were still detectible.

degenerate vestiges in certain grey tubercles evidently represent the blood-vessels which originally belonged to the lung, and not vessels newly formed because of, or in any sense belonging to, the tubercle.

8. *Blood Corpuscles, and Orange and Brown Pigment.*—Red blood-corpuscles unchanged are not common in tubercle; but a few shrivelled ones enclosed singly, or in small clusters of three or four, in a filmy envelop, are usually to be found. Yellow, orange, and brown cells may be traced to consist of such cellulated blood-corpuscles undergoing solution, and changing colour as the disintegration proceeds.

9. *Black Pigment* in granules, either free or else contained within a nucleated cell, is common both in and around tuberculous deposits, and abounds in direct proportion to their chronicity. Seeing the gradual alteration in the appearance of the enveloped red-globules last mentioned, and noting that black pigment is normally found on vascular surfaces, where there appears to be a special provision for rendering the circulation sluggish (e.g., the black pigment on the choroid, with its *vasa vorticosa*); and, abnormally also, chiefly in parts which are very vascular, and in which there is a tendency to obstructed circulation (as in tuberculized lungs, infarcted bronchial glands, and soft cancer which does not discharge its blood by external hæmorrhage); it is difficult to avoid the conjecture that altered red-globules constitute the material out of which the black granules are formed by a process of disintegration, and subsequent molecular attraction, after their enclosure in a cell. This pigmental degeneration may be taken to indicate chronicity in the disease, and tendency to local stagnation of blood.

10. *Granule Cells.*—The large round or oval cells, full of small similar-sized granules, so constant as a consequence of inflammation, are very common in and around tubercle. They abound in exact proportion to the extent of inflammation, of which, indeed, they are a principal measure. The only form of tubercle-cell with which, when very large, they may readily be confounded, is a compound corpuscle, too granulous for its several contained nuclei to be easily distinguished.

11. About and around the seat of tubercle, the lung-tissue is generally more or less consolidated by induration-matter. Tough, glazy, and firm, it presents, variously amalgamated, the nuclei, nucleated cells, and fibres of common plastic induration-matter, together with the characteristic free nuclei of tubercle.

Other constituents of tubercle and its boundaries are met with, but they belong to the ulterior changes which constitute the conservative and destructive courses of tubercle, and are elsewhere more fitly noticed under those heads.

Sputa.—If such be the real course pursued by the local disease, we shall naturally expect to find some evidence of fatty epithelial cells being cast off from the air-vesicles in the expectoration at the commencement of phthisis. This corroboration is not wanting. I have made careful examinations of the sputa in cases in which the deposition of tubercle appeared threatening, but in which no auscultatory proof was detectible; in cases of phthisis in its earliest confirmed stage, and in others where the disease was of old duration, and in its third stage. The following is the sum of these observations on the sputa in impending, incipient, and established phthisis.

Common to all.—Flat pavement epithelium, with sharp clear nucleolated nucleus, entire or partially dissolved, from mouth or pharynx. Bronchial columnar epithelium. Filmey cells, containing two or three dim granulous nuclei from tonsils. Pigment cells.

Impending Tuberculization of Lungs.—Expectoration grey, glairy, adhesive; brought up chiefly when dressing in the morning, and scarcely noticed or thought of by the patient. If innocent (which of course it generally is), it consists merely of round nebulous mucous corpuscles, in transparent adhesive mucus. If threatening, it contains, in addition, single plates, or small flakes of flat epithelium from the air-vesicles, fattily degenerating; and bronchial columnar epithelium, also presenting various degrees of fatty degeneration.

Incipient Tuberculization.—The same as the above; and, in addition, red blood-globules, more or less shrivelled and faded, enveloped in a filmy cell; a few large many-nucleated cells; granules; and frequently small casts of air-vesicles and the ultimate bronchi, in which are plainly visible epithelial cells in various sizes and various stages of fatty degeneration. So constantly have I met with enveloped blood-corpuscles in sputum which betrayed no appearance of blood to the naked eye, that I suspect that this microscopic hæmoptysis is seldom, perhaps never, altogether absent in those cases of commencing phthisis in which the more obvious expectoration of blood is wanting.

Established Phthisis.—The same as the last, largely mixed up with corpuscles of pus and mucus. Free tubercle-nuclei occasionally in small quantity.

Large Suppurating Cavities.—Absence of specific tubercle-cells, excepting portions of opaque tubercle itself happen to be expectorated. Habitually, little else than pus and mucus, and large granulous cells, unless confervoid vegetations be superadded, which is not unfrequent.

The microscopic appearances presented by the expectoration are most special and characteristic in the early stage, when other physical evidence



Fig. 12. Expectoration in phthisis.

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|---|--|
| 1. Oral epithelium. | 2. Filmy cells from tonsils. |
| 3. 3. Bronchial epithelium, some fatty. | 4. Mucous and pus cells. |
| 5. Pigment cells. | 6. Many-nucleated tubercle cells. |
| 7. Fatty epithelium from lungs. | 8. 8. Filmy casts. |
| 9. Free tubercle nuclei. | 10. Red blood-globules, enclosed in filmy envelopes. |

is least marked; and may be re-enumerated as consisting of fatty epithelial cells, single or in patches, fatty bronchial cylinder epithelium, casts of air-vesicles, lined with fatty epithelium, and microscopic hæmoptysis—i.e., enveloped red blood-globules. These are enough to indicate that consumption is threatened. When the many-nucleated cells and special free nuclei of tubercle are superadded, no doubt can remain. All these I have repeatedly found in cases where the patient was not apprehensive of phthisis, but complained only of general mal-aise or dyspepsia, as well as in other cases in which consumption was feared, but was not supposed to be yet in active existence.

Theory of the Local Part of the Disease.—The natural defensive epithelial coat of the air-vesicles is unable to maintain its own nutrition, and slowly falls into a state of fatty degeneration. It is then thrown off, and is succeeded by fresh epithelium, which is badly organized from the first, and quickly becomes degenerate and shed. This process of fatty degeneration and shedding may, perhaps, go on for some considerable time without further local disease; and we do not know but that it may happen in occasional instances, where the persons affected have their general nutrition by some means so much improved at this juncture, that good epithelium is once more laid down. This is quite conjectural; but if it ever do occur, such persons have been on the eve of becoming tuberculized, but have escaped. Ordinarily, after an uncertain number of desquamations of fatty epithelial cells, the surface of the air-vesicle forms cells which contain separate granule-bearing nuclei. In due course these also are shed, and probably some may grow larger after their shedding. A variable number of crops of these compound corpuscles may succeed each other, according as the wall of the air-vesicle maintains, for a longer or shorter period, sufficient organizing power to make them. The wall eventually becomes bare in places, and the albuminous part of the *liquor sanguinis* has no longer anything to hinder it from escaping into the air-vesicle. It does so escape; coagulates and corpusculates into such small, compact, and for the most part non-nucleated, cells, as its feeble organic force permits; and these are the free nuclei, or tubercle-corpuscles proper. That portion of exuded plasma which is unable to perform even this feeble act of development, falls into amorphous granules. The adjoining air-vesicles, under a continuance of this process, are crammed with desquamated cells, and with tuberculous exudation. By the resultant pressure, the remnant of vitality which the enclosed intercellular pulmonary tissue (viz., the septa and walls of the air-vesicles) retained, is still more impaired; exudation infiltrates amongst the pulmonic fibres, and corpusculates in their interstices into the free nuclei, but not into the many-nucleated cells. The tubercle is now complete. Up to this time it may have been unattended by inflammation, or inflammation may have attended its course throughout.

The relation of inflammation to tubercle is far too important to be

cursorily dismissed, and is discussed at length elsewhere.* I will merely state dogmatically here that tubercle *may* pass through all its stages *except the destructive ones*, without the coincidence of anything which we can justly term inflammation. But, on the other hand, it seldom does. Tubercle may commence with inflammation, or be quickly followed by it. Inflammation, in short, may step in at any stage, and almost invariably does step in at some stage, and is a necessary antecedent to the fatal issue of softened tubercle. Inflammation practically, therefore, exercises the greatest influence upon the course and termination of tubercle.

If we choose to say that when pulmonary tubercle commences with pneumonia it has an inflammatory origin, there is no refutation at hand. But as tubercle is not *always* of inflammatory origin, it cannot be essentially an inflammation. It is rather always a degeneration, which has a very habitual relationship to inflammation as a concomitant.

The exudation poured out by inflamed blood into inflamed tissue either fibrillates into induration-matter, or corpusculates into cells, for the most part round and nucleated, which become ultimately organized into tissue, or dissolved and absorbed, or changed into pus cells, according to circumstances; or else, the inflammatory blastema dies rapidly and at once. Tuberculous blastema does none of these after precisely the same type. And when tuberculous exudation is attended with inflammation, we find side by side the normal products of inflamed blood, and the specific products of tuberculous blood. The two are not mutually convertible. It follows, that either that part of the blood-plasma which forms tubercle *cannot* undergo the changes of inflammation; or else, that when some of the blood-plasma is inflamed another portion escapes the process, and is exuded in company with the first, to pursue afterwards a distinct course. In either view, the distinction between tubercle and all the known products of inflammation is obvious. But it may be said, that tuberculous inflammation may be of a specific kind, leading to the specific product of tubercle. Not so: for we find the inflammation which attends tubercle is just like common inflammation in its course, and in its products. If, again, it be urged that part of the process may be common inflammation, and part specific inflammation, proceeding side by side in the same portion of tissue, this seems to be almost a *reductio ad absurdum*, since upon such terms every known disease might be called an inflammation.

Natural Alliances of Pulmonary Tubercle.—Viewed in its twofold aspect, as primarily a degeneration of existing structure, and secondarily an exudation which corpusculates into very lowly-organized cells, tuberculization of the lungs has a generic relation with similar morbid affections of many other organs. And such allied affection of any other viscus, when it happens to coincide with pulmonary tubercle, may be looked upon as equally con-generic, whether it stop short at the stage of fatty degeneration, or pass on to that of tubercular formation. Hence the frequent association of phthisis with fatty liver and fatty heart, and its occasional association with the fatty variety of Bright's disease. The occasional occurrence of this affection of the kidney, the more rare occurrence of apoplexy from fatty degeneration of the small bloodvessels of the brain in persons of phthisical habit, but who are not the subjects of developed phthisis; and also the antecedence of general fatness of integument, the fat being less firm than that which indicates mere

* Loc. cit.

excess of good nutrition, in some persons who afterwards become phthisical, may point in the same direction. And several of the not unusual complications of phthisis, as well as some of its customary features, are possibly due to a similar kind of degenerative tendency. Thus, the thinning of the coats of the stomach which occasions the enlargement of that viscus in phthisis, though chiefly owing to simple atrophy of the muscular coats, may be in part also due to fatty degeneration. The hooked and clubbed nails, to fatty degeneration with enlargement of one portion of the bed of the nail. Scrofulous lymphatic glands, to enlargement, fatty degeneration, and accumulation of lymph-cells; or to positive tubercle; or to both, in different instances; inflammation being either coincident or absent. In like manner, the falling off of the hair, so common in females who are phthisical (as well as in many others whose mal-nutrition arises from more innocent causes), may greatly depend on fatty degeneration of the cells secreted by the hair-bulbs unfitting them for development after the existing hair is shed.

Upon these latter points (with the exception of the lymphatic glands), I have no certain knowledge; but reverting to the three instances in which the alliance is clear and sufficiently common—viz., fatty degeneration of heart, liver, and kidney, there is more to be said.

Muscle is not liable to tubercle. We have, therefore, no difficulty in admitting that fatty atrophy of the heart, when it occurs in phthisis, may be directly related to the tuberculous disease, although tubercle itself is not found in the organ. The degree in which the liver and the kidney are prone to become the seat of tubercle is not quite established. All that can be affirmed is, that whilst they are occasionally tuberculized, they are much more liable to be merely affected with fatty degeneration. When so affected in conjunction with tuberculization of the lungs, we need not hesitate to admit a direct relationship between the several organs diseased, in regard to the very essence of the disease. In the fatty form of Bright's disease, the similarity to the primary steps of chronic tuberculization of the lungs is particularly close, keeping in view the normal peculiarities of the two great excretory organs involved.

The lung is a chimney, which requires mere ventilation for carrying out its vaporous excretions. The kidney is a main sewer, always draining and periodically flushed. The internal surface of the lung keeps in all the blood except that portion which evaporates during its passage through the pulmonary capillaries. When, therefore, this surface has lost its natural defence, it does not instantly give exit to complete blood-plasma in excretable quantity. If it does so, there is often a large pouring forth, the blood escapes entire, red globules as well as plasma, and we have hæmoptysis. But in the kidney the internal surface is normally accustomed to let out the watery part of the blood, and so, when bared of epithelium in places, it readily gives passage to a portion of fluid plasma, with or without some red corpuscles, and we have albuminous urine, with or without hæmaturia. In the lung, tubercle follows; as a rule, it does not in the kidney, perhaps for these reasons:—So long as the plasma is periodically washed away with the urine, there is no material to corpusculate left behind; and when checked elimination of urine at length ensues, its consequences generally put an end to life too speedily to permit the question to be tested, not to mention the altered blood-crisis then induced. Moreover, when tubercle

is laid down in the kidney, its seat of election is the cortical parenchyma, and not the interior of the uriniferous tubes. And although the exact relation of tuberculous deposit to the minute structure of the cortex is not clearly ascertained at present,* it is quite possible that the free discharge of albumen from one portion of the gland may oppose the formation of tubercle in the other.

As in chronic phthisis the earliest local process consists in the enlargement and fatty degeneration of epithelial cells, and more or less accumulation of these in the air-vesicles; so also, in some cases in which, had the patient lived, he would probably have shown the customary signs of the fatty variety of morbus Brightii, do we find the uriniferous tubes so choked up with detached granulous and fatty epithelium, that the urine only filtrates through, and carries no albumen with it. Such a condition we shall generally find after death, when, in the last stage of slow phthisis, there is œdema of the lower extremities (irremovable by remedies), without any albumen in the urine, but with a copious sediment of phosphates.

In practice we notice, in respect to this subject, three kinds of case: the one just mentioned; phthisis with albuminous urine; and phthisis arrested, in which the patient dies at some future time of Bright's disease.

Is, then, in conclusion, tuberculization merely an advanced stage of fatty degeneration when this affects certain textures? Certainly not, since fatty degeneration is related to many other morbid conditions besides tubercle, and having little in common with it. But it may be, that it is a constant attendant upon tubercle; in which case we have a right to consider tuberculization as fatty degeneration *plus*—something else. If so, all that is known respecting fatty degeneration has a certain reference to the etiology and treatment of phthisis.

Not to occupy space by compiling information easily obtained from the writings of Mr. Bowman, Dr. Quain, Mr. Paget, and Dr. Handfield Jones, I will merely state here that I have succeeded on a large scale in inducing fatty degeneration of various viscera by keeping animals under such conditions as entailed mere inactive discharge of the functions of these viscera.

Neither is tubercle in the lungs the necessary consequence of shedding of the epithelial cells in the air-vesicles, and of subsequent exudation of plasma; for this occurs to some extent in simple pneumonia, and to a much larger extent in the intensely-congested lungs after section or ligation of both paria vasa at the same time. Here the deep, forcible, and very slow and infrequent inspirations, and the congestion of the pulmonic bloodvessels, acting as pressure alternating in opposite directions, cause the serum to transude and wash off the epithelium of the air-vesicles. The serum is churned into a spumous froth; hæmorrhage follows in a degree which varies from mere reddening of the foam to pulmonary apoplexy. Inflammation and suppuration may afterwards ensue, but no tubercle. As the animal seldom lives long, how can we know this? If a few days be allowed to intervene between the injury of the two nerves, the animal will sometimes live. Birds will generally survive under any circumstances. Killed some months afterwards, we are able to trace in these

* See, however, an excellent essay by Dr. Duncan, of Colchester, in the Transactions of the Provincial Medical and Surgical Association, vol. xvii. p. 113. 1850.

animals the morbid actions which ensued in the lungs, and we find no trace of tubercle.*

Neither fatty degeneration alone, nor exuviation of epithelium alone, nor mere exudation alone, is enough to constitute tubercle. The exudation must ever be at the first one of tuberculous blood-plasma.

But, in point of fact, as regards the lungs, are these three morbid processes ever conjoined in sequence, unless the blood be tuberculous? Does it ever happen that the epithelium of the air-vesicles degenerates fattily in considerable quantity, and is shed; and that this is succeeded by exudation into the air-vesicles, except in cases of tubercular phthisis? To this question, I am not at present prepared to furnish a positive reply.

(To be continued.)

ART. II.

On the Means of Diagnosing the Internal Diseases of the Eye. By C. BADER, M.D., and BRANSBY ROBERTS, Esq., Resident Medical Officer Royal London Ophthalmic Hospital, Moorfields. August, 1854.

WITH the publication of the following researches we bring before the notice of the profession an improved means of diagnosis in internal diseases of the eye—viz., a simplified ophthalmoscope. The instrument is not new by name; but those already in use are, generally speaking, so complicated and difficult to adjust, that they are but seldom employed.†

Believing that the diseases of the interior of the eye required cultivation, we have jotted down shortly a little advice respecting the management of the mirror, and some portion of the slight experience which we have acquired. It is exclusively to the invariable kindness of the physicians and surgeons of the Royal Ophthalmic Hospital, Moorfields, that the profession is indebted for enabling us to procure, in such a short time, so large a number of cases for examination, and thus fitting us in some way to give assistance to others.

For the different organs in the eye which we are about to examine, we use either a plane or a concave mirror for reflecting the light thrown from a lamp into the eye of the patient. That the eye of the observer may be in the centre of the rays of light thus reflected, a small circular aperture is made in the centre of the glass mirror, either by simply cutting a small round hole in the metallic back to the mirror—scratching off the

* These observations are founded upon experiments made, in 1847, with the assistance of Mr. Russell Hall (Cambridge) and Mr. Cotton, on forty-eight animals of different kinds.

† I saw the present simplified mirror first used at the Clinique of Dr. Desmarres, at Paris, where it was introduced by Dr. Anagnostakis, of Athens, who has published a dissertation, in which he speaks of the mirror as his own invention; I have felt it my duty, therefore, as a German, to offer some explanation of this. Dr. Anagnostakis speaks in his dissertation, from the book of Prof. Coccius, at Leipsic, about the ophthalmoscope as an *opuscule remarquable*, therefore, he must have known it. I may mention here the words of a letter from Prof. Coccius, which I received a few weeks ago: "In the year 1853 I drew a parallel between the advantages resulting from the use of a slightly concave and perfectly plane mirror, joined with a convex lens (I preferred the latter), and have shown (p. 81 in a book upon the ophthalmoscope, *opuscule remarquable*) that, optically, both forms of mirror are alike, but that the plane mirror, joined with a convex lens, furnishes more advantages in an examination near the eye." The curious may read the dissertation of Dr. Anagnostakis, for the purpose of seeing how much he says concerning that of Prof. Coccius; for my own part, I am quite unable to detect any difference between the concave mirror of Dr. Anagnostakis and that of Prof. Coccius. But Professor Coccius, at another time, will undertake to put Dr. Anagnostakis in mind of the concave mirror which he saw used at Berlin, at the Clinique of Dr. Graefe.—Dr. C. BADER.

silvering, but leaving the glass entire—or boring the aperture through the glass also. (The latter has been generally employed, but Mr. Hawes, of 79, Leadenhall-street, is now making some of the former, in which, also, the diameter of the aperture has been reduced, and which we think will be found of some slight advantage.) Through this aperture the observer looks. The concave mirror—which must be well made, and whose surface must reflect quite equably—is round, has a diameter of about two and a half inches, and a focal distance of about six inches: in its centre, a round aperture of a diameter of two lines,—the whole being fixed to a handle for convenience in using. The diameter, the opening in the centre, &c., are of the same size, both in the plane and the concave mirror. The light reflected by the former being more feeble, is better adapted for examining the crystalline lens, to detect opacity of which, as well as of its capsule, it will be found quite sufficient.

In the examination of all eyes, excepting those of short-sighted people, we were obliged, for the sake of seeing clearly, to use a bi-convex lens of two and a half inch focus, our sight being tolerably natural. Short-sighted people have the advantage of being able to see clearly without the lens, and will only require it for the purpose of concentrating on a smaller surface the objects which, seen through the crystalline lens, appear highly magnified. The usual course we have adopted in the examination of any case is the following:—The patient is seated in a dark room (his pupils having been well dilated by a solution of atropine—gr. j. or gr ij. ad. ʒj.); opposite him is seated the examiner—(a music stool is the most convenient seat for the observer, as it can be raised or lowered as required, to suit the height of your patient.) As near as possible to the patient's head is placed a steady-burning lamp, giving a good light, the eye of the examiner, that of his patient, and the flame of the lamp, being on the same level. The observer then takes the mirror and turns the reflecting surface towards the lamp, and endeavours to throw the light upon the patient's eye. The mirror having been taken, and the eye adapted to the central opening (where it must be steadily retained, the head, hand, and mirror being slightly moved, as required, together, either from side to side or slightly round, so as to throw the light fully upon the eye), he must look, having both eyes open, for the round circle of light about as large as the diameter of the mirror: the patient keeping his eye well open, quiet, and fixed; if the left eye is being examined, looking slightly towards the left side of the head of the examiner—the reverse when the right eye is under examination. To return: when the reflected image upon the examined eye is about the same size as the diameter of the mirror—the eye of the examiner and that of the patient, and the flame of the lamp, being on the same level, he looks through the central opening (to make sure of this he shuts for a moment the other eye), and being in the centre of the rays of light, he sees behind the pupil an illuminated red space. Looking into that space from different sides (the patient keeping his eye quite quiet), he will see inwards, and a little downwards, instead of the general red surface, a white, brilliant shining: immediately upon seeing this he brings down the convex lens (which he holds ready in the other hand, just above the examined eye) in front of the eye, so that the light from the mirror may pass through the convex

lens as it enters the eye: and now he sees a most brilliant sight:—the large white surface in the eye, which he previously saw shrinking together, forms a sharply-marked brilliant white circle, of a diameter of about three lines (the entrance of the optic nerve), from whose centre are seen passing a vein and an artery upwards, and a vein and an artery downwards, over the red concavity of the eyeball. This white surface is seen to be surrounded by the beautiful red choroid. Sometimes, before you catch the white surface of the entrance of the optic nerve, upon the red bottom of the eye are seen running darker streaks (the vessels coming from the entrance of the optic nerve), by following which in their backward course, as they become larger—that is, towards the internal and inferior part of the eye—for which purpose the head must be moved in the opposite direction, he will invariably be led to the white surface of the optic nerve.

During the whole examination, the left free eye (supposing the right to be used) must watch the movements of the globe which is being examined, and the reflection of the mirror; which reflection, in all movements with the mirror, must rest upon the eye. Having found the entrance of the optic nerve, and brought the convex lens in front of the eye, if he does not see quite clearly he must carefully endeavour to reach the convenient and proper distance by moving the head, gently and slowly, either forwards or backwards, as may be required, always keeping the mirror close to his own eye. After the examination of a few cases every one is able readily to find for himself the entrance of the optic nerve vessels, &c.

For rendering the examination easier, he may remember the properties of the convex lens—viz., that he will be able, by moving it towards any side, to bring other parts into the field of vision which had been previously hidden; for, supposing the eye to be quiet, we have a field of vision corresponding to the dilatation of the pupil, the objects in that field being immovable: if you now bring the convex lens in front of the eye, the lens is so far from the bottom of the eye that you are unable to see through it the whole surface, limited as it is by the pupil, but only a small part—as, for instance, a portion of a vessel with some surrounding choroid, or half the optic nerve with some choroid. For examining the whole surface, therefore, you are compelled to move the convex lens in a direction opposite to that in which the objects you wish to see are contained. For instance, if he sees only the lower half of the entrance of the optic nerve, and the iris appears to prevent his seeing the other half, by moving the convex lens upwards he sees the image of the optic nerve descend downwards till the whole surface is exposed to view. It must be always borne in mind that the iris limits the field of vision, and therefore, in order to see the whole surface through the convex lens, it (the lens) must either be moved about, or the examiner must move his own head with the mirror, or worst of all, for the patient cannot command his eye sufficiently, he (the patient) must be directed to move his eye slowly in every direction, till the whole has been brought into view. The transparent media of the eye magnify so much that, for overlooking a larger field of vision, for diminishing the size of the objects, and for bringing them nearer together, we are compelled to use the convex lens. As soon as the entrance of the optic nerve is found, you are assured that you have

arrived at the bottom of the eye, having seen through all the transparent media. This spot is the first and the most important that we have to look for, and the examination of other parts should not be prosecuted till everything that belongs to the entrance of the optic nerve has been thoroughly examined. If only a small portion of the crystalline lens remains clear, the entrance of the optic nerve must first be looked for. By observing this white surface, not only do we see the vessels entering and taking their course therefrom, but we see likewise the changes, if any, of and about its surface, as well as in the space in front of it.

The further examination of the eye is very easy. Whenever the entrance of the optic nerve can be readily found by the observer, he can easily learn by his own experience how to go on; the following limited remarks, however, may serve to facilitate the inquiry.

After the examination of the entrance of the optic nerve, we should notice the vessels entering in its centre (always remembering their normal state); then their course over the white surface; afterwards, their relations; and finally, their passage upon, and course over, the red surface, &c. After the vessels, the subjacent retina had better be noticed—its transparency, and its relation to the choroid. Following this, the yellow spot (making the patient look in the centre of the mirror); subsequently, the layer of pigment over the choroid, and the choroid itself; which, after several examinations (for which short-sighted people are the best subjects), being well understood, in its relations to the entrance of the optic nerve, to the vessels and transparent media, we come to the lens, for which, provided there are no muscæ in the vitreous humour (*vide* vitreous humour), we may use the plane or concave mirror, with or without convex lens, and must examine at every distance and from all sides.

The lamp being placed at the side of the patient, allows the eye of the examiner to go as close as possible to the crystalline lens, without losing the light on the eye. The free eye, as said before, should watch the image of the mirror as well as the movements of the globe.

Opacities of the lens are generally stellated in form, and, in consequence of their covering other objects lying deeper than themselves, they are readily seen.

If some time has been spent in strictly following out the course here described, first looking for the entrance of the optic nerve, travelling from behind forwards, and in this way carefully studying as it were the geography of the internal parts of the globe, then the examination of cases where the lens is absent, or the retina detached, or tumours are resting on one or other side of the eyeball, is very easy.

For common examinations the concave mirror is to be selected; but when a very feeble light is required, and you want to examine the anterior parts of the vitreous and the lens, then the plane mirror is to be preferred. If a very strong light is desired, let a tolerably strong convex lens be held between the lamp and the mirror, so that the light may be concentrated upon the mirror. For holding between the mirror and the eye, a good bi-convex lens of two and a half inch focus is all that is required.

Every examination ought to be begun without using the lens.

The following is a short account of the changes which are to be observed in the internal tissues of the globe:

Entrance of the Optic Nerve.—As soon as the light is thrown into the eye from the mirror, the patient looking straight forward, the observer, if he looks a little from the outer side, sees, instead of the red fundus of the globe, a glittering white surface; on approaching this he will find it to be circular, of a brilliant white colour, having a radius of about two lines; in the centre are seen vessels entering arteries and veins, which run upwards and downwards along the concave surface of the globe. This, the entrance of the optic nerve, the fixed point at the bottom of the eye, which must be first seen to make sure that you see through all the media of the eye in front of it, is of considerable pathological importance, not only on its own account, but likewise for the purpose of deciding what belongs to the choroid and what to the retina. In all objects lying behind the lens you must remember that you are looking through that body, and that, consequently, whatever you see is highly magnified. In a later part we shall speak of the vessels coming from the centre of the optic nerve, but at present we will confine our attention to the pathological changes which are most frequently met with on the surface and at its periphery. Any change of or upon the surface, or at the periphery, seems to have a marked influence upon the vision, and up to the present time we were unable to recognise any distinctive character in that portion of the retina which lies to the outer side, and is, as it were, cut off by some pathological change in or about the entrance of the optic nerve, from the other parts of the retina which are not so affected, so as to show that the part of the retina corresponding to the change at the periphery, or on the surface of the entrance of the optic nerve, was distinctly altered in its functions. The normal diameter of the entrance of the optic nerve, which is easily borne in mind, exhibits slight differences in its size at a first glance; but of more than 600 eyes which, up to the present time, have been examined, only one was found in which the surface of the entrance of the optic nerve was remarkably large, in which case the diameter of the circle was about seven lines, the eye perfectly healthy with this exception. The patient, who was a vigorous man, without any known cause had lost all sensibility to light. Where white flakes are seen in the choroid close to the periphery, care must be taken not to look upon this as an enlargement of the surface of the entrance of the optic nerve. In two cases only was the surface of the entrance seen to be diminished. Here the white surface appeared to be drawn backwards, giving it the appearance of a dimple. There are frequently cases in which the entrance of the optic nerve *appears* to be diminished, owing to an infiltrated or detached choroid or retina overhanging it. A little care will remove this source of error.

A brilliant white surface, or a white surface covered with a slight greyish gauze, is the normal appearance of the entrance of the optic nerve, but the limits between normal and diseased parts are so minute, and our knowledge of the degree of organic change necessary to produce impairment of vision so insufficient, that we are obliged to confine ourselves to relating simply what we have seen. We would only remark, however, that in judging respecting an examined case, having compared all the pathological

changes, we should afterwards examine the greater influence of one or the other.

In several cases the white surface of the entrance of the optic nerve was remarkably brilliant, and of a slight greyish hue; where, at the same time, there existed an effusion of serum behind the choroid, which was pushed forwards.

The greyish gauze upon the white surface generally consists of very fine points thickly standing together; these granules of pigment appear diffused through all the layers of the retina. In some cases they are in layers one over the other, in others they form a fine greyish ring upon the white surface; very often they are confined to the entrance of the optic nerve; in other cases they pass over upon the retina; occasionally large brown spots were seen resting upon the boundary of the entrance of the optic nerve and the choroid; while, at the same time, the choroid was seen to be covered with them, so that their situation was with difficulty to be accounted for, unless by an emigration, as it were, from the choroid. In a few cases, but very few, the spots were seen lying over the vessels. White flakes in the choroid are the best indications for judging whether the deposits of pigment are confined to the entrance of the optic nerve, or go over upon the retina.

The same care must be used with regard to the red gauze which is sometimes seen lying over the entrance of the optic nerve. The observer should keep well in mind this reddish gauze, and the layer of pigment which lies over it (or the granules of pigment imbedded in it), modifying its colour and its circular form. He should also mark well the ring of pigment which frequently is situated around the periphery of the optic nerve. For, unless these circumstances are kept continually in mind, the observer will be perpetually thinking the entrance of the optic nerve larger or smaller, in consequence of changes which have taken place around it in the choroid.

On several occasions effusions of blood were seen in the entrance of the optic nerve, either without any bloodvessel about them, or more frequently surrounded by a red gauze, which seemed to be situated below the vessels coming from the midst of the entrance of the optic nerve; the vessels composing the red gauze above-mentioned cannot always be seen, or even the trunks from which they originate, but in some cases very fine branches are observable leaving the vessels at the entrance of the optic nerve, and forming a red gauze or network over its surface; but in these cases two layers are frequently to be seen—a superficial one, formed by fine vessels coming from the entrance of the optic nerve, and a deep one which can only be seen as a gauze, and is generally confined to the surface, whereas the former passes over upon the retina.

The red gauze confined to the entrance of the optic nerve is often found, combined with an anæmic choroid and small vessels, coming from the centre of the optic nerve. The double layer is generally observed to be accompanied by a congested state of the choroid, the vessels of the retina, chiefly the veins, being very numerous. The appearance of the red gauze is sometimes simulated by the choroid hanging over the entrance of the optic nerve, and often exists distinctly, without any change visible in the other blood-carrying tissues of the eye. It was several times seen

in young women who had worked much with the eyes. Few veins were to be seen, but many small arteries coming from the entrance of the optic nerve. The choroid was not much congested. Sometimes the red gauze is the only distinguishing mark between the entrance of the optic nerve and the choroid.

We call the periphery of the optic nerve that circular border of the entrance of the optic nerve, as we see it in the eye, which is surrounded, in the healthy states, by the choroid. This border, or periphery, forms a fixed line between the choroid and entrance of the optic nerve, and is generally surrounded by a slight greyish ring of pigment, composed of fine greyish points, which appears to rest on the choroid. By means of the periphery of the optic nerve, we know what is on the same plane with the choroid, what lies above it in the retina, or is situated over both. In the examination of any pathological change, we first look for this boundary (the periphery), and the state of the vessels running over it; for it is but very seldom indeed altered in position (in two cases it was irregular, and thrown into folds backwards, owing to protrusion of the globe). Effusion of serum behind the choroid or retina detaches and pushes them forwards, causing them to form a wall around the entrance of the optic nerve, which overhangs the periphery, and hides it either partially or entirely.

Whenever the retina or the white surface of the entrance of the optic nerve exhibits an extraordinary brilliancy, partaking of a greenish or bluish glittering, there exists the probability of serous infiltration; and attention must be given to the parts around the periphery of the optic nerve. The periphery being a fixed point, prevents the further extension of the serous effusion, and the infiltrated or detached parts are raised around and hang over it, supposing the choroid is not pushed forwards, but only the retina; the wall hanging over the periphery has a slight reddish appearance, is transparent, and allows the choroid around the entrance of the optic nerve to be seen through it. All tissues over the entrance of the optic nerve appear to have a firmer connexion with it than with other parts; for instance, take the case of vessels coming from the centre of the optic nerve (the characteristic tests of serous infiltration, and protrusion of the parts lying around the periphery). In such an example, the retina can be partially detached, or is overloaded with serum, or both retina and choroid are pushed forwards by serum effused behind them, and so are raised to a higher level than the plane of the optic nerve, or even hang over it; in the former case, the vessels from the optic nerve run as far as the wall, and are then seen to ascend it, and continue their course over the retina; in the other case they run and disappear beneath the wall, again appear a little on one side in the same order, ascend the wall, and continue their course. It is necessary to notice the order in which arteries and veins go beneath the wall, for the purpose of ascertaining that those which are seen ascending are the same as those that went below it.

Up to the present time it was not possible to ascertain the amount of influence which the deposit of pigment in that part of the choroid immediately surrounding the entrance of the optic nerve, exercised upon the functions of the retina. The deposit, to a slight extent, is observed in

what appear to be in other respects healthy eyes, in a greater degree; considerable impairment of vision always accompanies it; continued observation will show whether the reason of the impairment is, that having been the product of inflammation, that part of the retina corresponding to it is injured. In several cases the deposit of pigment was, without doubt, combined with a finished process of inflammation, a cicatrix being visible in the choroid at the periphery of the optic nerve.

The Vessels coming in and going out from the midst of the Entrance of the Optic Nerve.—On the illuminated red fundus of the globe are seen running darker-coloured streaks; by following them backwards they are seen to become larger, and all converge towards the white surface of the entrance of the optic nerve, in the midst of which they disappear. On approaching to a proper distance for observing them distinctly, the streaks are seen to be really veins and arteries, the latter coming from, the former going to, the entrance of the optic nerve. The normal condition seems to be, a vein and artery going upwards, and another going downwards; sometimes the vein and artery have the same entrance, sometimes different entrances. Occasionally each vessel seems to have its own opening, but always the entrance is in the centre of the white surface of the optic nerve. Often the arteries are seen entering to the outer side of the veins, and in their course either crossing the veins, or being crossed by them. The arteries are easily distinguished from the veins by their straighter course, brighter colour, and lesser diameter. In pathological states, where the veins are in greater number, enlarged, and more tortuous, the difference is still more striking. Around the entrance of the vessels in the eye is seen occasionally a little bluish ring or black dimple. Few or no branches are generally given off to the white surface of the optic nerve, but the vessels pass over the surface and its periphery without showing the least deviation in their course, and run along the concavity of the choroid, exhibiting a strong contrast to it. The veins are slightly tortuous, both arteries and veins rarely ramifying; the red surface of the choroid makes it impossible to see the finer branches, and the iris (behind which they disappear) prevents us from following them to their termination. It is very remarkable that in all healthy eyes which have been examined, the arteries and veins have always been seen running upwards and downwards, dividing the concavity of the eyeball into equal halves; and that no visible vessel has ever been seen taking its course over the situation of the yellow spot. The veins and arteries take their way always running near each other; the vessels have transparent walls, and are in close connexion with the retina and hyaloid membrane, because, whenever one is detached, the part of the vessel corresponding to it is detached likewise. The blood in these detached vessels appears to be of a blackish red, and different colour to those which run in part of the membrane still in contact with the choroid.

The pathological conditions of the vessels are alterations in number, diameter, course, and contents; for instance, a vessel taking its course over the situation of the yellow spot would be called abnormal, likewise the tortuosity of the veins, in some places, with or without enlargement; these tortuous veins are seen upon the surface of the entrance of the optic nerve, upon the choroid, but chiefly at its highest concavity, being frequently surrounded by a mass of black and brownish spots.

Sometimes scarcely any arteries are seen entering the eye, in other cases they are very numerous, radiating on all sides, but ordinarily very slender.

In the majority of instances the cause of the tortuosity of the veins did not appear to lie in the eye itself (in one case the pressure upon the vessel behind the eye was quite clear); at the same time, the arteries are very small and anæmic, although sometimes numerous; the tortuosities of the veins are often confined to the entrance of the optic nerve; in some other cases, where the cause of increased number of veins and their enlargement, was in the eye itself, the veins appeared to be carrying very dark blood, were much enlarged and very numerous, converging from all sides towards the entrance of the optic nerve, but they were not tortuous. An augmentation of the number of the veins is not always accompanied by a congested choroid; for sometimes the choroid has been seen to be very anæmic, and yet the veins were enlarged and increased in number; for instance, in serous infiltration of the choroid and retina. The arteries, on the contrary, appeared to be diminished in number, very slender, and very deficient in blood; sometimes no arteries were to be seen at all. Augmentation of the arteries and veins was found to be very common in shortsighted people, as well as in young people who had tried their eyes much by their occupation; in the latter case, the veins were seen to be slightly enlarged, and running a straight course, the arteries generally very thin and numerous. Sometimes a white streak was seen, commencing at the first entrance of the artery, more frequently where the artery began to split into branches, giving the appearance of the blood running along the side of the vessel; whether the vessel is compressed in its centre, and the blood pushed towards the sides, or whether the white streak is a nerve, or is simply the division of the vessel into two finer branches, we are at present quite unable to say. In one case, the vessels coming from the entrance of the optic nerve were seen to form several layers over the red surface; on examining these from the side, they were seen to be separated by some transparent substance. In effusion of blood upon the choroid, the arteries were seen, immediately upon their entrance into the eye, to be enlarged for a short distance; sometimes they appeared to form a rich network over the retina, which contrasted beautifully with the choroid lying below. No certain and true conclusion, in all cases, respecting the state of the vessels in the interior of the eye, can be arrived at from external examination; alterations in the course of the vessels can be best seen over the periphery of the optic nerve, where they are met with most frequently.

Choroid.—The light having passed through the transparent media in front of the retina, passes through that organ, and falls upon a brilliant red surface, which is the first thing which strikes the observer in the examination of the eye; the light reflected from the surface in the transparent media gives this bright red appearance. The choroid (the red surface lining the posterior wall of the vitreous space) consists, judging from the usual appearance in healthy eyes, of bloodvessels lying as close together as possible, and having perfectly transparent walls; the whole being covered by a very tender, pointed, greyish-brown layer of pigment, giving the appearance of an uniform red surface (a comparison of many

healthy eyes of different ages is needed for the purpose of having a correct idea of the normal red of the choroid; likewise, one eye must be compared with the other before you can determine about the degree of congestion, anæmia, &c., present).

Over this red surface are seen running darker red vessels, well contrasted with it; if you follow the course of the vessel backwards, as it becomes larger, it leads to a circular, white, brilliant-looking place, in the centre of which that vessel, with others coming from all sides, disappears (this is the entrance of the optic nerve); and the vessels going out from its midst must first be seen before we can judge of the state of the choroid; as soon as these vessels are seen distinctly, you are certain also to be able to discern the choroid; the periphery of the optic nerve renders it possible to examine accurately the borders of the surrounding choroid; the vessels running over the red surface are easily used as marks, which limit any particular portion for observation, and show what lies upon the choroid, what upon the vessels next the vitreous humour, any inequality of the retina, and enable us to draw a parallel between its pathological changes and those of the choroid.

We are able to examine the choroid a little further than the highest concavity of the vitreous space. towards the ciliary processes, without, however, being able to reach them; the most direct light falling upon the choroid about the entrance of the optic nerve, renders that portion always clearer than the parts next the higher concavity of the globe, which ought to be borne in mind in judging about anæmic or congested choroids, &c. Having found the entrance of the optic nerve, we should always examine the place corresponding to the yellow spot, by making the patient look at the aperture in the centre of the mirror.

After the examination of both eyes, and having compared the result with his own experience of the normal condition, he may then look upon it as if it were independent of the layer of pigment placed in front of it; separating in his own mind, as it were, the influence of the pigment upon the red colour; and having thus a clear idea of the mass of blood contained in the choroid, he may compare it (the choroid) with the other pathological changes upon and around it.

The external aspect of the patient does not always enable us to draw any conclusion as to the state of the vessels in the interior; but the knowledge of the condition of the vessels in the eye will often be one of the utmost importance, as a means of diagnosis concerning the state of the vessels throughout the whole organism, as valuable in the treatment of the general as of the local affection.

Our attention must also be directed to the unequal distribution of blood often observed in the choroid (to confound effusion of blood and congestion is almost impossible, because the difference in colour is well marked, and the borders of the effusion well defined). The lighter coloured places may be normal or anæmic; at the same time we may compare the functions of the retina over these points with that at other parts. Sometimes one half of the eye is richer in blood than the other, which is remarkably the case in some squinting people. Some places may be perfectly bloodless, others of a feeble red colour, without the patient complaining of impaired vision. The congested state of the choroid does not always correspond with the exterior of the eye.

The observation of many cases of congestion gives the impression of an active and of a passive congestion; in the former, the choroid is of a bright red hue; the vessels, arteries, and veins upon the retina numerous, but slender, having a straight course, the veins not enlarged. Only a small quantity of pigment is seen upon the choroid, the transparent media in front of which are very brilliant; sometimes in the lens are seen undulating, transverse, and transparent streaks; this form is frequent in young plethoric people; in three cases it was accompanied by enlargement of the thyroid gland, in one by hypertrophy of the left ventricle; in no single case of active congestion were corpuscles seen floating in the vitreous humour.

In the passive form of congestion, the choroid is of a dark red colour, the veins running over it enlarged, numerous, and tortuous; but few arteries, and those of small diameter, visible; the choroid, covered by brownish-red spots of pigment, in some places placed more thickly together than in others. In the vitreous humour are seen floating bodies of the same colour as the pigment; similar deposits are also seen sometimes upon the posterior surface of the capsule of the lens. Till the present time, but little has been known, comparatively, of the state of the choroid in inflammations of the iris, sclerotica, &c. A little exercise in the management of the mirror makes a glance almost sufficient to determine the condition of the choroid. How far the feeble light of a candle, concentrated upon the retina, augments the inflammation, it has been hitherto impossible to know.

The degree of Congestion is not sufficient to account for the Impairment of Vision.—In congested eyes, the observer must notice the mass of pigment lying upon the choroid, its distribution and form; likewise the number, direction, and size of the vessels coming from the centre of the optic nerve; also the vitreous humour, lens, &c.; for the purpose of knowing the extent of the congestion, as well as for estimating how far the other tissues of the globe are affected. In some cases of active congestion, on approaching the eye so as to see not quite clearly the form of the vessels, a circular shadow will be seen upon the convexity of the lens, surrounded by a luminous ring, behind which appears the illuminated vitreous space, of a brighter colour, however, behind the luminous ring, than behind the greater convexity of the lens; whether the lens is pushed slightly forwards, or what is the cause of the shadow, we have not been able to determine. What confidence can we have in the means employed against congestion, when we see the vessels so distinctly?

The external appearance of the eye frequently agrees with the anæmic state of the choroid, but not always. The diagnosis of these conditions is easy so soon as you have a true and accurate idea of the normal red colour of the choroid; and these concomitant organic changes in the eye being strongly marked, can easily be distinguished from any other disease. The eye of the observer, accustomed to a red surface of a certain shade, sees instead of it (chiefly where the most light falls upon it, as about the entrance of the optic nerve) a reddish white or dirty white surface, covered with greyish, black, or brownish flakes of pigment, over which surface are generally seen running very slender veins, and only one or two arteries coming from the centre of the optic nerve. Not seldom the

vitreous is fluid, and *muscæ* are seen floating in it; in very many cases, cataract is seen commencing, and of it this anæmic state of the choroid frequently seems to be the forerunner. Cases of anæmia were most frequently seen between the ages of thirty-eight and fifty; many complained of a cloud before the sight, and of soon feeling fatigued upon using the eyes.

In all diseases of the eye, it is very interesting to observe the state of the layer of pigment upon the choroid; what is the normal condition is difficult to say; in about thirty healthy eyes which have been examined, it appeared as a thin, greyish-brown gauze, placed over the red in the form of points. The accumulation of pigment in masses seems to be one of the first visible symptoms of a diseased condition of the interior of the eye; the pigment was never seen in its normal state in any cases which have been examined where the other tissues of the globe were diseased. It was always accumulated in largest quantity where the choroid surrounded other tissues or pathological products in its own plane; as, for instance, at the periphery of the optic nerve, around bloodless places of the choroid, towards the ciliary processes, &c. In six cases of detached retina, which have been examined, no pigment at all was seen upon the choroid, which was of a feeble red colour; yet in these cases the vitreous humour was full of detached portions of pigment. After iritis of a severe form, rich accumulations of pigment have been always seen either deposited upon the capsule of the lens, on the choroid, or swimming in the vitreous humour. Very remarkable and almost characteristic of anæmia, is the mass of pigment which may be seen seated upon the choroid, around the entrance of the optic nerve, as a fine mass of points, of varying colour—greyish, greyish-brown, brown, and black—and, toward the highest concavity of the globe, as large surfaces collected together, forming small islands of pigment upon the anæmic choroid. Sometimes below the choroid, carefully examining between the masses of pigment, are seen bright red streaks, which uniting, form larger trunks, and take their course towards the entrance of the optic nerve. Why the pigment is seen better in one case than another?—why in some places it is accumulated together in large masses?—how it arrives in front of the retina in the vitreous space?—whether the so-called *muscæ* always make their way from the choroid into the vitreous space, or sometimes originate in the vitreous itself?—all these questions are capable of being resolved, we trust, by the use of the mirror.

No comparison can at present be founded between the amount of pigment and the degree of impairment of vision; we can only say, that the pigment is a valuable symptom which, united and compared with others, renders us capable of knowing and distinguishing different diseases.

The presence of quite white surfaces upon the choroid is curious. Hitherto we have only observed them in shortsighted people, where either in or behind the retina was a white layer, which prevented the light from arriving at the choroid, or, what is more probable, the vessels of the choroid itself at that place were obliterated or bloodless; in all cases these surfaces were close to the entrance of the optic nerve; sometimes a few flakes of pigment were situated over its surface; their border was surrounded by a thin line of pigment, and stretching across them was

seen a vessel coming from the entrance of the optic nerve. It is impossible to confound these places with spots in an anæmic choroid poorly supplied with blood, because their borders are distinctly marked by the surrounding choroid, which has a good red colour, and no other symptoms indicative of an anæmic condition can be detected. It perhaps might be mistaken for such a case as we once examined, in which the radial and temporal arteries were rigid and tortuous; in the congested choroid of this patient were seen quite white, irregular, sharply-marked spaces, in which were scattered about little diffused effusions of blood. These small apoplexies may be sometimes seen, after blows upon the eye, or an attack of apoplexy of the brain, as sharply-marked dark-red flakes, as a mass of points composed of blood, or oval patches, besides the vessels, with or without augmentation of the vessels of the retina.

At the situation of the yellow spot (that is, at the point which is opposite the mirror, when the patient looks at the aperture in the centre of it), in the normal as well as in most diseased eyes which we have at present examined, there is an uniform red space, covered by a thin greyish layer of pigment, over which not any of the vessels coming from the entrance of the optic nerve were seen running; in the few cases in which it was seen to be changed, the vision was considerably impaired; in one case a reddish surface existed, which was surrounded by a bluish-black ring; the other eye was healthy; this patient could only distinguish day from night. In another case, it (the place of the yellow spot) was occupied by an irregular brownish spot, as large as a pin's head, in the centre of this spot was a black point; the other eye healthy; on holding the palm of the hand before the patient, he could only distinguish the tops of the fingers, and of many other objects nothing but the outline.

The vitreous appears behind the iris as a perfectly transparent, quiet mass, placed in front of the choroid which surrounds it. Whether the vitreous in its centre or in its whole extent is fluid in the normal state, has not at present been determined; that it is fluid in a great number of cases of disease, is certain. Movement in the normal vitreous has never hitherto been proved; but when the eye is moved and looks upwards, the vitreous not only is moved passively with it, but after the globe has come to rest, owing to the shock in the first instance, it still remains in motion. We are enabled to see best through that part of the vitreous which lies between the greatest convexity of the lens and the entrance of the optic nerve. On looking from the side into the eye, for the purpose of examining the parts which lie toward the ciliary processes, in a healthy eye we can see nothing but a red glittering. The structure of the lens is not the only reason for this, for in cases in which the lens was quite transparent, and the vitreous humour fluid, we were enabled to follow vessels from their commencement at the entrance of the optic nerve till they disappeared behind the iris, or rather, till we could follow them no further, owing to the free border of the iris. The uniform transparency of the vitreous humour enables us to see the transparency of the retina very clearly. For measuring the distance of objects in the vitreous humour, and judging what lies near the retina, what near the lens, what is placed above and below, what is fixed, and what moves forwards or backwards,

we must employ the vessels which come from the entrance of the optic nerve and run over the retina in front of the red surface, taking for granted that the latter is not detached, these vessels are immovable, and being clearly seen, form marks by which we learn what is behind them or in the space in front. When the eye moves, they move with it, but immediately the globe comes to rest they become stationary, and so indicate the slightest further motion of any body in the vitreous space. Another means of measuring the distance is the periphery of the optic nerve; still another are the flakes of pigment sometimes seen on the posterior wall of the capsule of the lens, or even streaks in the lens itself, which have portions of lens, sometimes quite transparent, between them, and are, for the anterior part of the vitreous humour, very accurate marks for estimating the size of bodies, their movements, &c. Another means is the different distances at which we use the plane or concave mirror, with or without convex lens; the nearer you approach the vitreous humour, the more superficial must be the object you examine, and the better you are able to distinguish its colour; so that any small body which at eight inches' distance seemed to be black, will at two inches' distance prove to be white. The nearer you approach the vitreous humour, more especially when it is partially obscured, so much the more do the posterior parts serve to stop the light, and it becomes concentrated upon the smaller objects in the anterior portion of the vitreous space. The more light there is behind small objects in the vitreous humour, the darker they appear; but for this they need a certain circumference, for, under a strong illumination, considerable masses suspended in the humour, in the form of points, become quite invisible; and the best plan under such circumstances is to use a plane mirror, which casts a feebler reflection into the eye. Experience is the best instructor for each, as to when the convex lens ought to be used: it would be extending this paper to a great length and little profit if we were to discuss all the cases in which it might be used—it had better be tried in every case. The most frequent pathological conditions of the vitreous met with are its fluidity, in different degrees, and its being obscured by the so-called muscæ, or through effusions into it. Fluidity without any substance floating in it, we have never as yet met with; the degrees of fluidity are various and well-marked, and can be best judged of by the facility with which the bodies move about; they are seen sometimes flying up only to a certain height, and falling back again; often these movements are confined to the space which corresponds to the posterior convexity of the lens; often these bodies form greyish clouds which, in consequence of the shock given to the vitreous by any movement of the eye, take on a rotatory movement, which is confined to the circumference of the lens; often the bodies are so free that they may be seen moving in all directions—disappearing behind the iris—appearing again—never being quiet. The eye which is not employed in looking through the aperture in the mirror, should always be used in watching the movements of the examined globe, for the purpose of comparing the extent of the external movement with that of the bodies in the eye, for measuring the degree of fluidity, &c. Care should also be taken to fix one of the noticed stationary marks at the periphery of the vitreous, to judge concerning the movements in it. Supposing only a small portion of the

lens to be transparent, even then the vitreous will not escape a careful observation.

The cases of fluid vitreous which have been at present examined were in persons of middle and old age, whose attention had been in the first instance drawn to their eyes by the appearance of *muscæ*; in some cases the disease was distinctly referred to some antecedent syphilitic affection of the eye, in other cases the subjects of the malady were very strong and healthy-looking; in some, cataract existed in the other eye, the opacity being inconsiderable, the vitreous behind perfectly transparent, no *muscæ*, and the vision comparatively much more interfered with in the other eye, in which the vitreous was fluid and full of long black flakes; it is very seldom that the same degree of fluidity is observed in both; the worst eye is often the one which was affected last (as the patients say).

What is the influence which the different degrees of fluidity exercise upon the vision, is not easy to be determined, because generally the changes of nutrition and their derangements in the eye are so considerable, that no separation is possible.

The questions—What is the cause of the fluidity of the vitreous humour? where it begins? how it ends? what is the cause of the *muscæ*, and how do they become seated in the vitreous?—can only be answered by long and repeated observation. The determination of the colour of the *muscæ* is valuable, inasmuch as it enables us to compare those floating in the vitreous with the bodies observed on the choroid. After long-continued inflammation they are found to be black; sometimes, also, they are seen deposited upon the posterior capsule.

Numerous small *muscæ* appear to interfere more with vision than the larger flakes which hang together and are less numerous, and the movements of the latter are generally very limited—simply from one side to the other. Black *muscæ*, of the colour of the pigment deposited upon the choroid, do not seem to affect the sight so much as the greyish ones (due regard to other changes in the eye being taken into consideration). These black bodies seem so analogous, in many respects, to the pigment over the choroid, as to at once give rise to the idea that an emigration takes place in some way from the choroid to the vitreous. We are further led to think this by the obliteration of spots of pigment at the same time behind and over the vessels of the retina, so that one layer seems more advanced than the other: for this to be carried out we must suppose an atrophy of portions of the retina. If some of the *muscæ* are followed in their movements as they fall back behind the iris, they are seen either to sink and disappear in a troubled reddish and brilliant space, or else, after falling to a certain depth, remaining fixed in the humour. Besides these sinking *muscæ*, a fine greyish or black mass of points is often seen suspended through the whole vitreous; these appeared in some cases to be like the *débris* of larger bodies, in others quite uniform. The plane mirror should always be used in these cases, for sometimes the vitreous examined with the concave mirror appeared to be quite clear, whereas, upon employing the more feeble light of the plane mirror (placed very near the eye), the whole vitreous was seen to be permeated by a mass of very fine greyish points.

A cataract must be very dense not to allow the red space behind it to

be examined. If, after directing the patient to move his eye quickly from side to side, you tell him to look straightforward, and throw the light through one of the less opaque streaks in the lens, when the vitreous is fluid and bodies are floating in it, through the lens, feebly illuminated, small shadows (as it were) are seen flying about.

In examining effusions of blood into the vitreous, its colour must be noticed, for knowing how long it has been effused; its form, for judging concerning its diminution; and the surrounding parts must be searched for exudations about them, or for greyish membranes extending into the vitreous chamber. We believe that many cases of capsular cataract which have been torn through, and in which a good black opening has been formed—the patient not being much improved by the operation—if the eye were examined, many other opaque portions of membrane might have been found stretching through the vitreous in all directions.

An anæmic condition of the eye is, more frequently than any other organic change, found concomitant with fluidity of the vitreous.

Without observing any *muscæ* in the eye, without being able to detect any mass of points, the vitreous sometimes appears so obscured, that only the outlines of vessels can be seen, and the entrance of the optic nerve as a feeble white glimmer. This dulness has a dirty-brownish aspect; and in all these cases, the exterior of the eye has exhibited marks of chronic degeneration of all the internal tissues, the pupil has been dilated, irregular, and immovable.

The influence of the vitreous and other transparent media of the globe upon vision, we shall leave till further experience has enabled us to speak more practically than theoretically.

Excepting in those cases where the retina and hyaloid, having been detached from the choroid, are floating loose in the vitreous space, it is very difficult to say anything certain respecting the pathological state of the retina, being quite transparent; and we have never been able to prove, with certainty to our own mind, that either spots existed upon it, that it was atrophied, or not perfectly transparent; for when, between a certain point of a vessel coming from the entrance of the optic nerve and the choroid, a black spot or effusion of blood is seen, the retina may be pushed forwards in the transparent vitreous, without being in any way changed. The difficulties of examining perfectly transparent media can only be appreciated by those who have seen the distribution of the light in the vitreous space behind the lens.

How the pathological changes of the other tissues—as of the pigment upon the choroid, congestion of the vessels distributed over the retina—act as disturbing influences upon the latter, is at present impossible to say. It cannot be expected, in the present state of our knowledge, for us to show why one part of the retina is sensible in this way, another portion in a different manner—what may be the influence of this or that change upon its functions.

An œdema of the conjunctiva, with slight congestion of its vessels, has a yellowish-bright aspect; the same impression is conveyed to the eye when the choroid is congested, and the retina appears to be infiltrated with serum. When changes are seen at the situation of the yellow spot, which considerably impair the vision of the patient, we must not forget

(although that part of the retina which is opposite the highest convexity of the lens is impaired, and, therefore, the patient sees badly) to examine other parts of the retina also for the purpose of testing its sensibility.

The cases which have been hitherto examined, have led us to the conviction, that the mirror, to a certain degree, like the stethoscope, will enable us to determine the amount of impairment of vision in the patient's eye, without its being necessary to ask what he complains of; whether he is short or long sighted; whether he sees better with this or that portion of the retina, &c. The congestion and anæmia of the choroid, so frequently met with, always exhibit a train of symptoms peculiar to them, so that it is to be hoped that, by persevering study of the organic changes, some light will be at length thrown on the functions of its different parts. The most interesting disease, in an optical and anatomical respect, is the detachment of the retina and hyaloid with the vessels coming from the entrance of the optic nerve. In the small number of cases which have been yet seen, a large portion of, or the whole, retina appeared to be swimming in the fluid vitreous. The mode of examining these cases, and what is to be seen, is more given in detail in the history of each patient; but it may be remarked here, that when the whole retina appeared to be detached, the patient always retained a slight perception of light. These cases ought to be examined in every way, and at every distance; they are, optically, the most instructive. The red surface of the choroid has lost the brilliancy which it received from the closely superimposed transparent retina, and is of a feeble red colour, upon which the pigment is deposited, and over which the vessels are seen running. The entrance of the optic nerve appears as a small surface of the size of a pin-head, occupied by the vessels coming out from its centre (which appear to be chiefly veins) hanging down in the vitreous, and running like ribs over the whitish-grey membrane. The vitreous humour, often obscured by a nearly invisible granular mass of points—sometimes full of muscæ of different colours; in most cases the lens was quite transparent, and allowed the observer to trace vessels as far forwards in the space behind as was compatible with the presence of the iris. Four of these cases occurred in seamen who had made voyages to tropical climates, and were between the ages of forty and sixty.

Lens.—The examination of the lens with the mirror possesses a double interest:—First, from the certainty of the existence of changes in it which are not to be seen with the naked eye; and, secondly, the possibility of being able to examine, in many cases, the parts behind the opaque lens. Changes in the lens which can be seen with the naked eye are as well seen with it as with the mirror, and before you proceed to your examination with the mirror, the cornea must be carefully observed, that no opacity exists in it; for if such should be the case, the determination of the state of the lens becomes very difficult, not to mention the great likelihood of mistaking an opacity of the cornea for that in the lens. Striæ and slight opacities of the lens are easily known. Supposing these to be present, and you look for the entrance of the optic nerve, it will be found that the eye is prevented from seeing at once the whole surface by some dark mark. To know whether this mark is seated in the lens,

vitreous humour, or still further backwards, the reader must refer to what has been said respecting the movements of *muscæ* in the vitreous humour. Any opacity in or upon the lens has a movement corresponding to that of the whole globe, and no more, at the same time covering a portion of the background in proportion to the size of the opacity ; if you, however, look from the side into the eye, the part previously covered is at once seen.

In examining the lens, the plane and the concave mirror, with or without convex lens, should be employed ; and it should be tried at every distance, as well from the front as from the side. Very often the different direction of the rays of light is the only apparent reason for seeing or not seeing. For example: the lens, when looking from the front, may appear perfectly transparent, but on looking at it from the side it is seen to be traversed by light undulating lines, which detain the light in its passage through in a straight line, without, however, producing any opacity of the substance of the lens. These undulating lines are most frequently seen in eyes which are actively congested ; sometimes the mass of light exercises a vast influence upon the appearance or non-appearance of opacities.

In one case the changes in the lens, which were tolerably advanced, were only to be seen with the plane mirror. There was not the slightest opacity of the lens, but its transparent elements were so disturbed amongst themselves, that, from whatever side it was looked at, small glittering surfaces were seen, which, although quite transparent, reflected the light differently from the surrounding substance. In another case, this derangement of the fibres of the lens had a curious effect: the patient, having both eyes open, found his vision much impaired, and could only read large print ; upon shutting the right eye, he can see with the left the smallest type. In both lenses, bluish streaks, of equal number and development, were seen running from the periphery towards the centre ; in the right eye the transparent elements of the lens lying between these streaks were as much deranged in their equality as if they had been stirred about.

Sometimes flakes of pigment are deposited upon the posterior capsule, from which the patients say that they are floating about in the eye.

Anæmia of the choroid is the most frequent disease seen at the same time with opacity of the lens: the relation between them—which the cause and which the effect—in what manner the degree of one corresponds with that of the other—is easy to be decided. The amount of vision does not only depend upon the degree of opacity of the lens, but likewise upon whether the choroid is well supplied with blood or not, for the more blood there is in the choroid (taking it from the anæmic state upwards to its normal condition), the better the sight.

The brilliant surface given to the choroid by the transparent retina seems to play an important part in the formation of cataract ; and it would be interesting, in a certain number of cases, to examine the condition of the choroid both before and after the extraction of the lens ; for although the removing of the opaque lens is the first indication for the procuring of improved vision, yet the congestion produced in the eye by the operation, and the irritation caused by the light which now falls

without obstacle upon the retina (for a simple examination with the mirror causes a freer injection of the vessels), are certainly subjects of some moment, which deserve to be borne in mind. The observation of the influence which the choroid has upon the vision, has caused some doubts to arise concerning the propriety of the rules which are generally given respecting waiting in cases of extraction.

The following are some cases in which it was neither possible with the naked eye, nor with the aid of a convex lens for throwing more light in the eye, to make out any alteration in the transparent media of the lens:

1. On looking from the front, nothing abnormal to be seen in the lens; at ten inches distance, looking from the side into the eye, undulating lines were seen in the upper part of the lens.

2. It was only possible, with the plane mirror, to see some light striæ going from the periphery towards the centre of the lens.

3. From the centre of the lens, towards the periphery, are going six greyish-white streaks, between which the substance of the lens was quite transparent.

4. The patient having received a blow on the eye:—looking from the front, all quite clear; from the side, cracks going from above downwards.

5. The whole posterior wall of the lens full of little brownish points, rendering it difficult to see behind it: when the eye moves, the whole mass of points moves at the same time, no one point approaching the other—the patient always sees something floating before the eye.

6. The patient received a blow from a stone thrown upon the eye:—no inflammation existing at present. From the front, the lens looks quite transparent; examining from the side, the light is reflected by a light crack in the lens, which runs from the upper periphery towards the centre.

A case of conical cornea having been examined, no abnormal change of the deeper tissues could be detected; but no objects lying behind the cornea, upon examination, appeared in the same plane. All lines upon its surface were distorted, and it was only by looking through the side of the cone that the state of the deeper tissues could be determined. The light falling upon the summit of the cone gave it the appearance of a glittering point, surrounded by a dark cloud.

Care must be taken, previous to examining the cornea with the mirror, to see that its surface is quite even and smooth; if not, inequalities upon it may be taken for undulating lines in the lens.

The advantages which have been already derived from the use of the mirror in practice are, we believe, the following:

1st. The possibility of detecting the slightest impediment to the passage of light through the lens.

2nd. Of seeing the reason of the frequent unsatisfactory result of the tearing through of membranes occluding the pupil.

3rd. The advantage of being able, in many cases, to see (through the cataractous lens) the state of the parts behind it,—as the vitreous humour, whether healthy or normal; retina, whether detached, &c.

4th. In the uncertain indications of the external symptoms, we have

the certainty of not mistaking an anæmic for a congested condition of the internal tissues.

5th. The advantage of not treating a patient, who is amaurotic from a detached state of the retina, with too energetic medicines.

ART. III.

On the Peculiarities in Figure, the Disfigurations, and the Customs of the New Zealanders; with Remarks on their Diseases, and on their Modes of Treatment. By ARTHUR S. THOMSON, M.D., Surgeon of the 58th Regiment of Foot.

(Concluded from No. 28, p. 470.)

Wounds and Injuries.—It was to be expected that wounds and injuries would be less frequent among the New Zealanders than the English, because there is no machinery, no building of high houses, no personal combats with fists, and no intemperance among them. The wounds of the New Zealanders heal with great rapidity. I have seen the most severe injuries do well. The famous chief, Hongi, the Napoleon of New Zealand, had a musket wound through the chest and lungs, and lived for several years with a hole in his back, through which he could produce a whistling sound by the expulsion of the air, for the entertainment of his friends when in gay humour. This wound was ultimately the cause of his death.

Diseases of the Eyes.—This class of diseases is frequent. The most frequent form is conjunctivitis and scrofulous ophthalmia, which affections terminate often in partial or complete opacity.

Diseases of the Skin are much more frequent among the New Zealanders than the English. The principal form is psoriasis and scabies, modified by neglect into a most inveterate disease. Tinea capitis is frequent, and also herpes circinatus—ringworm. I have not seen nor heard of a case of herpes zoster.

There are hot springs in the interior, where the natives resort for the cure of cutaneous diseases, the beneficial effects of which may either arise from the sulphur they contain, or the cleanliness they produce.

Scrofula.—Under this term is included, almost entirely, cases of supuration, or swelling of the glands of the neck, king's evil, and some cases of curvature of the spine. There are many other diseases which, with great propriety, might be included under this head; but, without them, it will be seen how painfully prevalent this obscure constitutional disease is among the aborigines of New Zealand.

In some districts, 20 per cent. of the adult natives, taken indiscriminately, are found with the cicatrices of sores on the neck; in other districts, 10 per cent. These are not loose assertions, but numbers obtained from actual enumeration. All, however, who are scrofulous, do not bear on their bodies this outward mark, for "the seeds of disease are so hidden in their constitution, that even the healthiest men, to all outward appearance, are often the first to be taken."*

* Bishop Selwyn's Visitation Tour in 1848, published in *The Church in the Colonies*, No. 20.

No medical man can look at a number of New Zealanders without observing, strongly marked, that peculiar physical development which characterizes the scrofulous diathesis. It is the bane of the whole race, and the remote and predisposing cause of almost all the sickness and mortality which occurs among them. In early childhood we see it producing wasting of the body, fevers, and bowel complaints. After puberty, the swollen and suppurating glands in the neck point out that the disease still lurks in the system; from this period of life until death, exposure to the vicissitudes of the weather, poor food, and a variety of other causes, generate or excite scrofulous disease of the lungs.

The internal use of iodine and cod-liver oil has lately been found by Dr. Davies, the Colonial Surgeon at Auckland, to be a very efficacious remedy. Psoas and lumbar abscesses have been known to prove fatal, and a good many New Zealanders are seen with curvature of the spine.

The causes of this great prevalence of scrofula among them are—breathing an impure air in their sleeping houses, indolent habits, insufficient clothing, bad food, intermarriages with near and scrofulous relations, and change from a tropical to a temperate climate.

To enter minutely into all the above causes of scrofula, is foreign to the object of this paper; but I may observe, that the sleeping houses of the natives are low in the roof, small, and often below the surface of the ground, with a small door and a small window, both of which are shut at night; and for ten hours of the twenty-four the men, women, and children respire an atmosphere which is as unwholesome as the most crowded and worst ventilated houses in some of the poor parts in London. In former days, the constant dread of war, and war itself—the uncertainty and difficulty of obtaining food, previous to the introduction of the fruitful European potatoes, made them exert themselves in fishing, killing birds, rats, gathering seeds, &c.; but now there is little dread of war, and a few days' labour is all that is required to plant potatoes sufficient for the year, and to collect a few pigs, and sell them, so as to obtain blankets and tobacco. The rest of the year is spent in smoking, talking, reading, playing draughts, the indolent occupation of cleaning flax, and sleeping. There are a few persons in some tribes actively engaged in commerce, and in cultivating wheat, but these are the exceptions to the general rule. The clothing of a New Zealander is not sufficient to protect him from the weather. It is a loose gown of common calico (a round-about), buttoned or pinned round the neck, and a rug or blanket overall; the children are similarly clad, and you often see a mother and her infant crouching under a scanty and threadbare blanket. In former days, the quantity of fish eaten was much greater than at present. Potatoes are now more easily got, and are more easily cooked; besides, they have a way of preparing potatoes and maize for food which is highly injurious. It is by making them putrid before they eat them, by steeping them in water. This is evidently a modern evil, the injurious effects of which, I am of opinion, are very great. Intermarriages with near and scrofulous relations must also cause and aggravate scrofula: this is often difficult and impossible to prevent, and the New Zealanders have no inborn dread to incest. I know an instance of a man who has two wives: one of them is his grown-up daughter, and she is the mother of three children to him. The

last cause of scrofula among them is their migration from a tropical to a temperate climate. The New Zealand race keep an exact enumeration of the generations which have passed away since their arrival in New Zealand, and, according to this, it is probable they arrived in New Zealand from the tropics about six or seven hundred years ago. In the tropics, the climate has an average temperature of 84° Fahr.: the country they now live in has a mean temperature of 60° Fahr. There are not many instances of the aborigines of the tropics migrating to the temperate zone in large bodies, but in every instance which has occurred the migration has proved unfortunate.

There may be objections to this last cause of scrofula, and I admit it is difficult to comprehend how a race would increase for several hundred years in an uncongenial climate, and then suddenly decrease; but, from a careful examination of the question, I am clearly of opinion that change of climate must be considered as one of the exciting causes of scrofula, for the New Zealanders flourish best in New Zealand where the climate is warmest, and worst where the climate is coldest.

All other Diseases.—The number of causes included under this head are much more numerous among the English than the aborigines of New Zealand.

During the year 1851, in different parts of the North Island, mumps (*cynanche parotidæa*) prevailed as an epidemic, and the metastasis to the testicle was several times observed; a few cases of diseases of the heart are recorded, and from the prevalence of rheumatism I am surprised they are not more frequent; hydrocele now and then occurs; otitis and deafness are met with, and are most frequently produced by want of cleanliness. Eight cases of erysipelas have been treated, five of them occurred at Kororariker, in the Bay of Islands, in 1838, when that place was a scene of intemperance; and it is probable this habit may have had some influence in producing the attacks, as I never saw a case of erysipelas among them in Auckland.

The New Zealanders are not intemperate, they will drink spirits if it is given to them, and they like the excitement which it produces; but a New Zealander with a shilling in his pocket will rather purchase a loaf than a glass of spirits. Scurvy is a rare disease. *Dracunculus*, or Guinea worm, is unknown. *Elephantiasis Arabica* (elephant leg), which is very common among the Polynesians in the tropical islands in the Pacific, is almost, I was going to say entirely, unknown in New Zealand; but I recollect seeing a native in the interior of the north island with modified elephantiasis of the left leg. Inguinal hernia occurs, and proves occasionally fatal; I saw a strong young man die from strangulated hernia. Double inguinal or labial hernia has been seen in a female; and umbilical hernia is common among children, in consequence of the custom of cutting the cord too short, with the shell which they use for that purpose; it generally disappears after a few years. I have heard no bad result from it.

Cancer.—From very careful inquiries, I have not heard of a native woman dying from cancer or carcinoma of the breast. If the disease should occur, it must be extremely rare indeed. This remarkable peculiarity deserves particular attention; but the absence of the disease from among the New Zealanders is another link in the chain of evidence, that

cancer is a disease of civilization. Sir Astley Cooper recommends a diet of animal food as a means of cure in cancer; but the absence of the disease among a people who live almost entirely on vegetables, would indicate a different opinion. As cancer is said to be rare among the inhabitants of Egypt and Algeria, a migration to these countries has been recommended to those suffering under the malady. The same may be said of New Zealand. But I have already said that, although the aborigines do not suffer from it, yet European women have had the disease commence in the country; and there are no European women sufficiently old, who have been born and brought up in New Zealand, so as to ascertain whether they are liable to the disease or not.

Stone in the Bladder.—No case of this disease has been seen, nor have I heard of one. As Professor Cooper has calculated that one case occurs annually in England for every hundred thousand souls, we might naturally expect, out of a population supposed to be nearly one hundred thousand, that ten years' constant intercourse would have brought one instance of stone under the notice of some of the Europeans living in the country, had the disease existed among the New Zealanders. The vegetable diet, and the large quantity of pure water which the New Zealanders drink, tend to produce urine of a low specific gravity, and comparatively free from uric acid.

Bronchocele.—I heard of one supposed case of this disease, but on inquiry found it to be scrofulous tumour. There is in the North Island of New Zealand, in which almost all the native population live, a large quantity of magnesian limestone, which cannot fail to impregnate the water passing through it; but I have visited villages in this part of the country, but never saw a case of bronchocele. This circumstance furnishes no argument against the theory of the magnesian origin of bronchocele, because the aborigines rarely use well water, and they are very particular that the river water they use is clear and tasteless. If, then, there is one thing more than another the New Zealanders are particular about, it is the purity of the water they drink. Thus, well water is rarely used, or a stream which runs through a wood, where decayed trees are likely to be found.

Hydrophobia has not been seen either among dogs, Europeans, or natives. The absence of the disease in New Zealand is a link in the chain of evidence, that the malady has its origin in specific contagion. There are many dogs in the country, and some of them are very badly fed animals.

Tetanus.—I have not seen nor heard of either an idiopathic or symptomatic case of tetanus among the aborigines.

Diseases of Infancy.—A large number of children die under three years of age. The poor diet on which the mother lives produces thin and watery milk; sometimes the milk is scanty, and food is given to the child which it cannot digest. This produces bowel complaints and fever. Neglected catarrhal complaints from insufficient clothing often produce death. Dentition is occasionally accompanied with irritation and convulsions, the last complaint is not so common as among European children; croup proves fatal; worms are very frequent. *Tabes mesenterica* occurs, but the poor food infants have given to them lays the foundation of a delicate sickly life and a premature old age.

Diseases of the Organs of Menstruation.—As there are a large number of women in New Zealand barren, it may be supposed the menstrual discharge is often irregular or diseased. I have inquired into this subject, but it is a difficult question to arrive at the truth of, because the native women rarely consult Europeans about this class of diseases; from the inquiries I have made, it appears the menses begin to flow at thirteen, fourteen, fifteen, or sixteen. I have heard it stated they begin at ten years of age, but this is not usual. I never saw a New Zealand girl have a child who in appearance was not at least fifteen years of age. I have seen European mothers younger. Sexual intercourse takes place often before the menses appear. The menses are sometimes very irregular, one or two months often passing without a flow; women occasionally have headache for a day or two before the usual period of the menses, but no uneasiness is felt after this. I have heard of some instances of menorrhagia; profuse menstruation occurs. From all inquiries, I am of opinion that the New Zealand women are subject to the same irregularities as women in England: but these irregularities are perhaps not so common, nor do they appear to have so much influence on the constitution. This opinion is founded, however, on no statistical data. Hysteria and chlorosis, the two maladies most influenced by the menses, are almost unknown. I have heard of a half-caste girl who had chlorosis. The sterility which the New Zealand women suffer from, is perhaps caused by too early and promiscuous sexual intercourse. I have been told that women have a way of rendering themselves barren by injuring the womb, but as my informant, one of the clergy of the Church Missionary Society, said it was never attended with loss of life, I am inclined to doubt it. Count Strezlectri, in his 'Physical History of New South Wales,' published in 1845, states, that when once a female of the aboriginal tribes of Australia has borne a child to a European or white, she ceases entirely to reproduce with males of her own race. I doubt the truth of this among the aborigines of Australia, and can deny its accuracy regarding the New Zealand women, for I know several instances of New Zealand women having children with men of their own race, after having had children by Europeans.

Parturition is not attended with the same dangers as among Europeans. Many missionaries and medical men never heard of a woman dying in childbed. A native chief, aged about fifty, told me that out of a tribe numbering 4000 souls, he could only recollect ten instances of women dying in childbed. This is about one death in three years out of 2000 women. The circumstances which caused death, the chief said, were hæmorrhage and cross-births; one medical man was called to a woman with an arm presentation, and another to a protracted labour, the result of a deformed pelvis. Child-bearing extends from fifteen to thirty-five years of age; but I have heard of a woman whose age, from certain known circumstances, must have been forty-seven when she gave birth to a child.

New Zealand women often give birth to large families. Twins and triplets occur; but three children born at one time have never been reared. I knew a woman who had fifteen children, all dead. When a woman has a protracted labour, it is assisted by violent pressure on the

abdomen. I saw a young female who was suffering from extensive ulceration of the muscles of the abdomen, which had come on after a protracted labour. It might have been produced by too violent pressure. Abortions occur, and are often caused and produced by pressure on the abdomen, or sitting over a native oven, such as will be afterwards described. I never heard of a woman with puerperal convulsions. Infanticide is generally perpetrated, the moment the child is born, by pressing the head between the thighs.

Child-bearing is usually easy with the New Zealanders. A New Zealand woman, the bearer of a burden, with a party of travellers, was confined on the road; after the birth of the child she walked four miles, and next day, fifteen. They rise almost immediately after the expulsion of the after-birth. As soon as a New Zealand woman finds her labour about to commence, she takes a blanket, and goes out into the open air, into a quiet retired place. If it is her first child, a woman attends her: after the first child they go out alone. After delivery, the woman proceeds to a stream and washes herself and her infant, and then returns home. During labour, the women kneel down, with their thighs apart, and having their hands resting on a tree or a stick. They hold their breath. Labour seldom exceeds two hours; generally, it is much shorter. Sickness after parturition is rare. The great ease of childbirth may be partly due to the pyramidal shape of the skull. Prolapsus uteri after childbirth is rare. In a large village, containing 400 women, there was only one woman who had this disease.

Epidemic Diseases.—According to all accounts, there have prevailed, during the last sixty or seventy years, three fatal epidemics; one called maripa, or makoko, broke out after the arrival of an European vessel in Mercury Bay, sixty years ago, and proved fatal to a great many persons. It had a dysenteric character, and caused death after a few days' illness. About five years after this, another pestilence broke out, called rewarewa, and so many died, that the living could not bury the dead. It commenced among the natives in the north part of the island, and had a dysenteric character. Tangara is the name of a pestilence which visited the island, and carried off great numbers, the nature of which I cannot ascertain. Since the settlement of the British Government in New Zealand, in 1840, there have been three epidemics—influenza, hooping-cough, and mumps. The first has been prevalent twice, and whole villages were prostrated by it. Hooping-cough and mumps are said to be new diseases; the former prevailed as an epidemic in 1847, the latter in 1851.

On the Method in which Diseases were treated by the New Zealanders.—The instinct to live, which is found so strong in the human breast, has made men, in all ages, endeavour to procure means to ward off death. Among the New Zealanders, the desire to live was as strong as among other races of men; yet the idea which existed in their minds, that all diseases are inflicted, directly or indirectly, by the gods for their sins, or by witchcraft, made them resort to prayers in place of physic for a restoration to health. I insert four out of the many prayers which were used for the treatment of disease, so as to convey some idea of their nature and style:*

* I am indebted for these prayers, and for their translation, to Mr. C. O. Davis, of Auckland, one of the few persons in New Zealand intimately acquainted with the language and modes

He Kopito.—Kopu nui, kopu roa, kopu takitaki, kopu whakaahu tena te ara te hamama na kawea kowhitia, pararitia, pupa, nau mai ki waho.

Translation of the above Prayer for Swollen Stomach.—Big belly, long belly, stretched belly, bursting belly: there is the passage open, take it hence, pluck it out.

He Korere.—Titi puru e, titi puru e, titi kohea, titi maiami, e tena te titi ka titi, tena te puru ka puru, ko te puru ra tena, i purua ai te tupuna a Houtaiki.

Translation of the Prayer for Looseness of the Bowels.—Stop up the looseness, stop up the looseness, the purging will subside, the purging be stayed: there is the purging and there is the stopping up, for this is the remedy that stayed the malady of thy ancestor Houtaiki.

He Manawa.—Kei te manawa, kei taku, kei taku manawa kei te manawa whena, he manawa kaukau. Tina ki roto whena ki roto whakataka atu ki roto.

Translation of the above Prayer for Disease of the Heart, or Shortness of Breath.—It is in my heart, on it is my breath, and in thy breath it is in thy heart, and my heart, in the heart that is strenuous. Let it be overpowered inside, let it be strenuous inside,* let it be thrust back inside.

He Hono.—Tutakina i ou iwi, tutakina i ou toto, tutakina i ou mongameya tena te rangi, ka tutaki, tena te papa ka whena.

Translation of the above Prayer for a Sprained Back.—Close up your bones, close up your blood, close up your marrow, and be united as the heavens, and let the bones be strong as the earth.

Confidence was not, however, placed in such stupid compositions as the above, the exhibition of substances to cure diseases were occasionally resorted to; but the exact value of prayers and medicine will be at once appreciated when I state, that medicine, without the assistance of prayers to the gods, was totally inefficacious. New Zealanders have no idea of the circulation of the blood, nor of any of the proper functions of the different organs of the body. The head, although extremely sacred, is not supposed to contain the organ which produces the intellect. The stomach and bowels are believed to be the seat of some of the faculties of the mind, such as joy, fear, and sorrow. Every external part of the body has a name, and a good many of the internal parts. Their cannibal feasts gave them an opportunity of acquiring anatomical knowledge.

The New Zealand mode of treating surgical cases was often successful and judicious. For a broken bone, splints were made, from the bark of trees, in the shape of the part, and bandages were made from the flax plant. Dislocations were reduced. Sprains were treated with rest and shampooing. They never performed amputation. Scraped roots and leaves were applied hot for boils and sores. Abscesses and boils were opened, often long before they were ripe, and severe pressure applied to squeeze out the matter. The surgical instruments employed for opening

of thinking of the New Zealanders. The translations are literal, but it is a difficult subject, for few of the present generation of Maoris are acquainted with these prayers, or even the exact meaning of the words, and the difficulty is increased by the circumstance that many words have five or six meanings.

* This most probably refers to the god who is supposed to be in the heart of the sufferer.

abscesses were the sharp edge of a shell, a splinter of obsidian, or a sharp-pointed stick, or a thorn. If a person received a wound, it was first washed, and then a plaster of mud applied to exclude the air, and this was allowed to remain until the wound was well; sometimes the wound, if small, was bruised with a stone, to excite bleeding, and afterwards held over the smoke of a fire of certain plants. The bleeding from a wound is sometimes checked by holding it over the smoke of a fire. For cutaneous diseases and sores, bathing in the hot sulphur and siliceous springs at Roturua Taupo, and at several other places, are reckoned very beneficial. For rheumatism, scarifications were made in the skin, or friction and shampooing with the fat of pigeons and whale oil, were used. In lumbago, stones were heated and rolled over the part. Change from one part of the country to another was an esteemed remedy for certain diseases, not for the climate, but to avoid the evil spirits which produced the sickness. Blistering a part was known, and it was done by the application of the leaves of certain plants—e.g., clematis. Local bleeding by scarifying the skin was known, but not venesection. Hæmorrhage from a wound was checked by bandages, and stuffing the wound with mud. Diseases which were particularly supposed to be caused from a devil, or an atua, living in the body, were occasionally starved out, *alias* the patient killed; at other times they were pressed out. Mr. Nicholas, a settler in the Thames, told me that, in 1848, he saw a person killed by an attempt to drive out the atua. A young, healthy New Zealander had a pain in his stomach: he was laid out naked on the ground, heavy baskets of stones were placed on his chest and stomach, on the top of which several persons sat for about a quarter of an hour; when they were removed the unfortunate man was dead—from suffocation, I presume, in consequence of the movements of the lungs being impeded. The evil spirit, in this case, was effectually expelled, but with it went the vital spark.

Vapour and hot-water baths were often used in the treatment of diseases. The vapour bath is made by digging a hole in the ground about two feet deep, and as large as is required; into this a number of very hot stones, about the size of a large orange, are put, a quantity of water is sprinkled over the stones, and over them a thick bed of the leaves of the plants which are supposed to be efficacious in the cure of the disease are spread. The patient either lies down on them, or sits on them. In the former case, the body is covered with a mat and a layer of earth, in the latter with a mat or blanket. The steam generated by the water on the hot stones rises through the leaves, envelopes the body, and produces a copious perspiration. This bath is often given; sometimes the patients are kept in it from sunrise to sunset, but generally for two or three hours. Vapour baths are constructed at Roturua over some of the hot rocks found in this volcanic district.

Mode of making the Hot-Water Bath.—This was not so often used. Captain Cook was wrong when he said the New Zealanders were ignorant of any way of boiling water; they have a highly ingenious way, which they used for giving hot baths. It is of heating stones very hot, and throwing them into cold water contained in a large wooden bowl. The water soon boils, and the heat is kept up by throwing in fresh hot stones. During the heating of the water, certain trees and barks are put

into it, which they impregnate with their properties. It is a medicated hot-water bath, and is chiefly used for cutaneous diseases.

Mode of recovering Persons who have been long under Water.—The body, immediately after being taken out of the water, is suspended, head downwards, over the smoke of a fire; if there is any life in the person, the smoke produces irritation in the nostrils, and sneezing. After this effect is produced, the body is put before a fire, and hot water poured down the throat.

Nominal List of Plants used as Medicine by the New Zealanders, and the Diseases for which they are supposed to be beneficial.—The expressed juice of the *Mesembryanthemum* (pig's ear), Nat. Ord. *Ficoides*, is used for boils. The leaf and root of the native flax, *Phormium tenax*, Ord. *Asphodelæ*, is used for ringworm, bandages, and various internal complaints. The bruised leaves steeped in water of a pepper-shrub, called Horopito, *Drimys auxillares*, Ord. *Magnoliaceæ*, is used as a remedy for cutaneous disease. The leaf of the Kahikatoa tree, *Dacrydium excelsum*, Ord. *Coniferæ*, is used in the form of decoction for urinary and other internal complaints, and in the steam bath. A decoction of the Kareao, Ord. *Loranthæ*, is used for secondary symptoms, and to produce abortion; the young shoot is eaten to cure the itch. The leaf and bark of the Kawakawa, *Piper excelsus*, Ord. *Piperaceæ*, is used for cuts, wounds, cutaneous disorders, gonorrhœa, and in making vapour baths. A weak infusion of the Kohekohe, *Hartighsia spectabiles*, Ord. *Meleaceæ*, is used to stop the secretion of milk. Mosses found on trees, when dried and reduced to powder, are rubbed into the skin for cutaneous diseases. The Kōkōwai (red ochre) is used for sore heads and wounds. The leaves of the Kopakopa, something like the plantain, are used to bandage over ulcers, one side of the leaf draws, and the other heals. The tender leaves of the Koromiko, *Veronica segustrifolia*, Ord. *Scrophalerinæ*, are bruised and applied as a poultice for ulcers, and especially venereal ulcers. The inner bark of the Kowai tree, *Edwardsia microphylla*, Ord. *Leguminosæ*, is used for itch. The bruised pith of the Mamaku, *Cyathea medullara*, Ord. *Filices*, is used as a poultice for swollen feet and sore eyes. The infusion of the bark of the Miro, *Podocarpus ferruginea*, Ord. *Coniferæ*, is drunk for pain in the stomach. An infusion of the root Mouku, a fern, is used as a wash for cutaneous complaints. The bark of the Ngaio tree, *Myoporum læteum*, Ord. *Mysporineæ*, is applied to ulcers and cutaneous eruptions. Charcoal from any wood was used in a state of powder for cutaneous eruptions. The pith of the Ponga, *Cyathea medullara*, Ord. *Filices*, was used as a poultice for cutaneous eruptions. The inner bark of the Poroporo, *Solanum lacineatum*, Ord. *Solaneæ*, is used for the itch. The bark of the Pukatea, *Laurelia Novæ Zelandiæ*, Ord. *Atherospermeæ*, and the bark of the Rata, *Metrosideros robusta*, Ord. *Myrtaceæ*, are used for cutaneous diseases. The infused bark of the Rimu, *Dacrydium Cupressinum*, Ord. *Coniferæ*, is employed to dry up ulcers, and for wounds of all sorts. The water in which potatoes are boiled is used as a lotion for cutaneous diseases. The boiled blood from the ear of a dog is used as a remedy for spear-wounds. A decoction of the Ti, *Cordyline Australis*, Ord. *Asphodelæ*, is used for dysentery. The leaf, tender shoot, or pith of the Tutu, *Coriaria Sarmentosa*, Ord. *Coriariæ*, is used for dysentery; it

is very poisonous to animals and men; when a man has eaten the seeds and stalks of the tutu, and shows signs of poison, he is put under water for nearly half a minute, his stomach is by this means filled with water, he is then taken out and rolled on the ground, until he vomits. The expressed juice from the seeds of the tutu is a favourite drink of the New Zealanders, it is not poisonous, and is very pleasant. The juice of the leaf of the Puatanatana is used as a blister.

There are several other plants used by the natives to cure diseases. Different parts of the country use different substances. None of them are very active. Neither purgatives nor emetics are much used. A large proportion of the remedies are to cure cutaneous diseases, a very significant proof of their great frequency. There are few remedies for pectoral diseases, or indeed any internal or obscure malady. Much of the above information relative to the plants used in medicine is procured from the Rev. Mr. Taylor's 'Leaf from the Natural History of New Zealand.' The natural orders given, I have arranged from a French work, entitled 'Choix de Plantes de la Nouvelle Zeelande Recueillies et Decribes, par M. E. Raoul, Chirurgien de première classe de la Marine Royal, 1846.'

Their Physicians.—From the New Zealanders' ideas about the pathology of diseases, the priests were their only useful medical men. When called to a sick person, the first thing done was to consult the gods as to the termination of the malady; there were several ways of doing this, but the most common was to pluck up a piece of fern, if the root came up clean and free from earth, a favourable prognosis was given; if, on the other hand, much earth adhered to the root, an opposite opinion was given. The sick were conveyed into the bush away from the village, and a small place built for them, and the place was tapued—when a sick person recovered, fern root or some other food was cooked, and a portion was set apart for the gods—the tapu was then removed. When a person was bewitched, the priests were summoned to drive the witch away; the incantations of the priests in such cases were generally performed near water, where they professed to see the spirit depart. In some cases the witch was transferred from one person to another. I have seen a woman have nothing the matter with her, almost die from a belief in witchcraft; she rapidly recovered on a priest visiting her, and repeating a short prayer. The tapuing of the sick, and their removal from among the healthy, is merely an extension of our laws for the prevention of contagious disease.

ART. IV.

Note on the Induction of Sleep and Anæsthesia by Compression of the Carotids. By ALEXANDER FLEMING, M.D., Professor of Materia Medica, Queen's College, Cork.

WHILE preparing a lecture on the mode of operation of narcotic medicines, I thought of trying the effect of compressing the carotid arteries on the functions of the brain. I requested a friend to make the first experiment on my own person. He compressed the vessels at the upper part of the neck, with the effect of causing immediately deep sleep. This experiment has been frequently repeated on myself with success, and I have made several cautious but successful trials on others. It is sometimes difficult

to catch the vessels accurately, but once fairly under the finger, the effect is immediate and decided.

There is felt a soft humming in the ears, a sense of tingling steals over the body, and, in a few seconds, complete unconsciousness and insensibility supervene, and continue so long as the pressure is maintained. On its removal, there is confusion of thought, with return of the tingling sensation, and in a few seconds consciousness is restored. The operation pales the face slightly, but the pulse is little, if at all, affected. In profound sleep, the breathing is stertorous, but otherwise free. The inspirations are deeper. The mind dreams with much activity, and a few seconds appear as hours, from the number and rapid succession of thoughts passing through the brain. The experiments have never caused nausea, sickness, or other unpleasant symptom, except, in two or three instances, languor. The period of profound sleep, in my experiments, has seldom exceeded fifteen seconds, and never half a minute.

The best mode of operating is to place the thumb of each hand under the angle of the lower jaw, and, feeling the artery, to press backwards, and obstruct the circulation through it. The recumbent position is best, and the head of the patient should lie a little forwards, to relax the skin. There should be no pressure on the windpipe.

The internal jugular vein must be more or less compressed at the same time with the carotid artery; and it may be thought that the phenomenon is due, wholly or in part, to the obstructed return of blood from the head. I am satisfied that the compression of the artery, and not of the vein, is the cause. The effect is most decided and rapid when the arterial pulsation is distinctly controlled by the finger, and the face loses somewhat of its colour; and, on the other hand, is manifestly postponed and rendered imperfect when the compression causes congestion of the countenance.

This mode of inducing anæsthesia is quick and certain. The effects diminish immediately when the arteries are relieved from pressure, and are not liable to increase, as happens sometimes with chloroform and ether, after the patient has ceased to respire their vapours. So far as my experience goes, it has shown no tendency to cause faintness; and usually, after its employment, no unpleasant feeling whatever remains.

I think it may be found useful as a remedial agent in certain headaches, tetanus, asthma, and other spasmodic diseases, and to prevent pain in such small operations as the extraction of a tooth or the opening of an abscess. Whether the compression can be continued *with safety* sufficiently long to make it available in larger operations, has to be ascertained. But, whatever be the practical value of this observation, it is at least interesting as a physiological fact, and may be the means of throwing light on the causes of ordinary, medicinal, and hypnotic sleep, and of coma. Some facts encourage the supposition that the circulation of the brain is languid in ordinary slumber, and the etymology of the word carotid shows the ancient belief in the dependence of deep sleep on some interference with the passage of the blood through these vessels; and it is not an unreasonable conjecture, that hypnotic sleep may be sometimes caused or promoted by the contracted muscles and constrained position of the neck compressing the carotid arteries, and diminishing the supply of blood to, and pressure on, the brain.

PART FOURTH.

Chronicle of Medical Science.

REPORT ON THE PROGRESS OF ANIMAL CHEMISTRY DURING
THE YEARS 1852-3-4.

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THE following are the most important works on physiological and pathological chemistry which have appeared during the last three years.

1. MOLESCHOTT: *Der Kreislauf des Lebens. Physiologische Antworten auf Liebig's Chemische Briefe.*—Mainz, 1852.
2. ROBIN ET VERDEIL: *Traité de Chimie Anatomique et Physiologique, ou des Principes immédiats Normaux et Morbides qui constituent le Corps de l'Homme et des Mammifères.* 3 vols. 8vo. Accompagnés d'un atlas de 45 planches gravées, en partie coloriées.—Paris, 1853.
3. HEINTZ: *Lehrbuch der Zoochemie.*—Berlin, 1853.
4. SCHLOSSBERGER: *Lehrbuch der Organischen Chemie mit besonderer Rücksicht auf Physiologie und Pathologie, u. s. w. Dritte durchaus umgearbeitete und vermehrte Auflage.*—Stuttgart, 1854.
5. SCHLOSSBERGER: *Erster Versuch einer Allgemeinen und Vergleichenden Thier-Chemie. Erste Lieferung. Begiron der Vergleichenden Gewebschemie.*—Stuttgart. 1854.
6. GORUP-BESANEZ: *Anleitung zur Qualitativen und Quantitativen Zoochemischen Analyse. Zweite umgearbeitete und vermehrte Auflage.*—Nürnberg, 1854.
7. BECQUEREL ET RODIER: *Traité de Chimie Pathologique appliquée à la Médecine Pratique.*—Paris, 1854.
8. LEHMANN: *Lehrbuch der Physiologischen Chemie. Zweite Auflage (Zweite Umarbeitung).* 3 Bde.—Leipzig, 1853.
9. DAY: *A Translation of LEHMANN'S Physiological Chemistry, in 3 vols.*—London. (Printed for the Cavendish Society.) 1851—1854.
10. FUNKE: *Atlas der Physiologischen Chemie, zugleich als Supplement zu C. G. LEHMANN'S Lehrbuch der Physiologischen Chemie.*—Leipzig, 1853.
11. FUNKE: *Atlas of Physiological Chemistry. Being a Supplement to LEHMANN'S Physiological Chemistry.*—London. (Printed for the Cavendish Society.) 1853.
12. LEHMANN: *Handbuch der Physiologischen Chemie.*—Leipzig, 1854.

MOLESCHOTT'S volume is evidently intended as an attack by the Dutch chemists upon the special doctrines of Liebig and the Giessen school. We need do little more than mention the subjects of his different letters. 1. Revelation and the laws of nature. 2. The sources of human knowledge. 3. The indestructibility of matter. 4. The growth of plants and animals. 5. The earth in its connexion with the production of plants and animals. 6. The circulation of matter. (The author in this letter argues strongly in favour of Mulder's views regarding the humus theory. Most of our readers are probably aware that Mulder believes that the humic-acid group, in combination with ammonia, exerts a considerable influence on the nutrition of plants; while Liebig holds that the main use of manure is

referable to the inorganic substances which it contains.) 7. Plants and the soil. 8. Plants and animals. 9. Nutrition and respiration. (In this letter, Liebig's well-known division of nutriment into plastic and respiratory food is very severely criticised.) 10. The development of the food into blood, &c. (Moleschott here gives us his own division of food into albuminous bodies, fat-formers, fats, and salts). 11. The ashes (or mineral constituents) of man and animals. 12. The disintegration of the animal tissues. (This letter contains an excellent popular account of the subject of which it treats.) 13. The disintegration of the vegetable tissues. 14. The development of heat in plants and animals. (Liebig's view, that the development of animal heat is solely due to a process of oxidation, is here opposed with much ingenuity.) 15. The gradual development of matter. (The formation of organic matter in plants, from inorganic constituents, and the various forms of decomposition of organized structures after death, are here considered.) 16. Matter rules man. (In the letter with this strange heading, the author attempts to demonstrate the influence of food—or, in other words, matter—on the activity and power of man; he shows, by numerous examples, the injurious effects of a solely animal or solely vegetable diet, and demonstrates the necessity of a mixed diet in the case of man. 17. Force and matter. 18. Thought. 19. The will. 20. Vital economics.

This little work is well deserving of a careful perusal; and we should be glad to see it translated, as a companion volume to Liebig's 'Chemical Letters.'

Robin and Verdeil's treatise is of a most elaborate description. We have already reviewed it at such length,¹ that it is unnecessary again to enumerate its contents.

Taking the work as a whole, we may state as our opinion, that it will, for many years to come, prove of invaluable aid to the lecturer or writer on animal chemistry, but that it is too cumbrous and elaborate in its structure for the student or general medical reader. It is honourably distinguished from all other French chemical works with which we are acquainted, by the familiarity with English and German literature which its authors exhibit; and it is accompanied by an admirable Atlas.

Heintz's '*Lehrbuch der Zoochemie*,' a thick volume of upwards of eleven hundred pages, next claims a brief notice. It is divided into two parts, of which the first occupies 833 pages, and is devoted to the consideration of the constituents of the animal body. This part is arranged as follows:

- I. *Inorganic substances occurring in the animal organism.*
- II. *Organic constituents of the animal body.*

These are divided as follows:

- I. Basic compounds.
- II. Acid compounds.
- III. Indifferent substances.
- IV. Substances whose nature and properties are not as yet accurately known, as, for instance, the extractive matters.

The second part is devoted to "the methods of analysing animal substances." The Introduction, which includes two sections on the recognition of seminal and blood spots, is followed by general remarks on the qualitative and quantitative analysis of animal matters; and these are succeeded by descriptions of all the best methods of analysing individual substances—namely, the bones, urine and urinary sediments, blood, milk, bile, and animal concretions. This part concludes with sections upon the analytical separation of the gases occurring in the animal body; upon the qualitative and quantitative determination of the carbon, hydrogen, nitrogen, sulphur, and phosphorus in organic substances; with tables for the calculation of analytical results.

¹ No. 25 (Jan. 1854), p. 94.

In the first part, the different substances entering into the composition of the body are very fully and accurately considered in reference to their history and occurrence, their most essential chemical properties and compounds, their products of decomposition, and their qualitative and quantitative determination. It is to be regretted that Heintz has not imitated Lehmann's admirable example of entering into the consideration of the importance of the various substances in the animal economy, and their metamorphoses within the organism. The author's high reputation as an analytical chemist renders it unnecessary for us to allude to the excellence of the second part. Like the work of Robin and Verdeil, it contains much that might have been omitted without detriment, and it omits many subjects—as, for instance, respiration, digestion, nutrition, &c.—which we should have been glad to have found in it.

Schlossberger's '*Lehrbuch der Organischen Chemie*' has passed through three editions in the course of four years. It is a work of great value, but, as its title implies, it is not so exclusively devoted to the subject of this Report as the preceding volumes.

The same author's '*Erster Versuch einer Allgemeinen und Vergleichenden Thier-Chemie*' is not yet sufficiently advanced to enable us to express an opinion on its general merits. We shall, however, have occasion to notice it in a future page in connexion with the chemistry of "cartilage and bone," and of "the nervous system."

Gorup-Besanez published the first edition of his '*Anleitung zur Zoochemischen Analyse*' in 1850; the second edition, now before us, has been carefully revised, and received numerous additions. It is divided into a *general* and a *special* part.

The *first* or *general* part treats of the operations employed in chemical investigations, of the reagents made use of, of the necessary apparatus, of the composition, properties, and recognition of the compounds occurring in zoo-chemical investigations.

The *second* or *special* part enters fully into the analysis of the blood (the various methods of Scherer, Becquerel and Rodier, Figuier and Dumas, and Schmidt, being given), the urine and urinary sediments, the milk, the bile, serous or albuminous fluids (chyle, lymph, pus, liquor amnii, transudations), the digestive fluids, expectorated and vomited matters and the excrements, the bones, concretions, the semi-solid tissues, the expired air, and the ash of animal substances.

The mode of arrangement and the execution of this volume are equally admirable, and we are glad to be enabled to state that an English translation of it is in preparation.

Becquerel and Rodier's '*Traité de Chimie Pathologique*' presents no claim whatever to our favourable consideration; the authors seem to be profoundly ignorant of all chemical investigations that have been carried on out of France, and their book does not in any respect represent the present state of animal chemistry.

Lehmann's '*Lehrbuch der Physiologischen Chemie*,' and Funke's '*Atlas*' to it, are too well known to the English reader, through the translations issued by the Cavendish Society, to require any comment. The readers of this journal are well aware of the very high estimate in which the '*Lehrbuch*' is held by the reviewers to whom this department of medical science is entrusted.

Lehmann's '*Handbuch der Physiologischen Chemie*' is a condensation of his '*Lehrbuch*.' It includes, in a moderate octavo volume, the positive facts of physiological chemistry, in so far as they are yet established, given in the most compressed form, and excludes all discussions on disputed points, and all names and references. Our only objection to it is its brevity.

Lehmann in both his works divides physiological chemistry into three parts :

- I. The theory of the chemical substrata of the animal body; or, zoo-chemistry in the strict sense of the word.

- II. The theory of the animal juices and tissues (phlegmato-chemistry and histo-chemistry).
 III. The theory of zoo-chemical processes—that is to say, the chemistry of the metamorphosis of matter, of nutrition, and of secretion.

We shall adopt Lehmann's arrangement in the following pages; and in considering the additions that have been recently made to our knowledge of the "organic substrata of the animal organism," shall commence with the

ORGANIC NON-NITROGENOUS ACIDS.

Oxalic Acid.—Lehmann,² in the new edition of his 'Lehrbuch,' points out that oxalate of lime may possibly be formed in the urine after its emission, during the acid fermentation which has been so well described by Scherer. We know that there is a close connexion between the excretion of uric acid and the formation of this salt, from the circumstance that, in most specimens of urine, both sedimentary and non-sedimentary, oxalate of lime cannot be recognised by the microscope so long as the fluid is fresh, but as soon as crystals of uric acid present themselves, crystals of oxalate of lime, in varying numbers, may also be detected; indeed, we generally find that, in morbid urine, the abundance of these crystals is proportional to the rapidity with which the free uric acid separates. Since uric acid, when acted upon by certain oxidizing agents, may be decomposed by the chemist into urea, allantoine, and oxalic acid, there is no difficulty in the supposition that a part of the uric acid is decomposed during this acid urinary fermentation, and that oxalic acid is thus produced; and this view is much strengthened by the recent observation of Ranke,³ that urate of soda, when exposed with yeast to a temperature of about 90° Fahr., became converted in a few days into oxalic acid and urea.

In connexion with this subject, we must notice an essay 'On the History of the Development of Oxaluria,' by Dr. Beneke,⁴ as well worthy of an attentive perusal.

Formic and *acetic* acids have been detected by Scherer in the acid fluid expressed from muscular flesh,⁵ and from the spleen,⁶ and likewise in the blood in leucæmia.⁷ Schottin⁸ has found formic acid and (in a lesser quantity) acetic acid in the normal sweat; and Campbell⁹ has found formic acid in vomited matters, in the blood, and in the urine.

Butyric acid has been detected (and not as a mere product of decomposition) in the sweat, by Schottin,¹⁰ who believes that he has also found *metacetic acid* in the same fluid; it has likewise been found in the gastric juice by Gruenewaldt.¹¹

Valerianic acid in association with leucine has been found in the urine in typhus fever, by Frerichs;¹² the leucine had probably undergone a partial decomposition into valerianate of ammonia.

The *non-volatile fatty acids* will be noticed when we consider the *fats*.

Lactic acid has been detected by Lehmann¹³ in the juice of the smooth muscles; and by Scherer¹⁴ in the juice of the spleen, and in leucæmic blood. Neither

² Lehrbuch der Phys. Chem., vol. i. p. 43.

³ Journ. f. pr. Chem., vol. lvi. p. 16.

⁴ Zur Entwicklungsgeschichte der Oxalurie. Ein dritter Beitrag zur physiologischen Heilkunde. Von Dr. F. W. Beneke. Göttingen, 1852.

⁵ Ann. der Chem. und Pharm., vol. lxx. pp. 196—201.

⁶ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 298.

⁷ Ibid., p. 321.

⁸ Arch. für Phys. Heilk., vol. xi. pp. 73—104.

⁹ Chem. Gaz., vol. xi. p. 310; and vol. xii. p. 52.

¹⁰ Op. cit.

¹¹ Succ. Gastrici Humani Indoles Physica et Chemica. Diss. Inaug. Dorp. Liv. 1853.

¹² Schmidt's Jahrbücher, vol. lxxxiv. p. 146.

¹³ Lehrbuch der Phys. Chem., vol. i. p. 107.

¹⁴ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

Schottin nor Lehmann could find it in the sweat, although Favre¹⁵ not only maintains that it is present, but professes to have determined its quantity. Robin and Verdeil¹⁶ have recently found lactate of lime in large quantity in the urine of the horse. The discussion between Lehmann and C. Schmidt regarding the presence of this acid in the gastric juice, can hardly be said to be satisfactorily settled. "There can be no doubt (says Lehmann), when we consider Schmidt's well-known accuracy as a chemist, that, in the cases which he analysed, the gastric juice contained no lactic acid, and that it was replaced by free hydrochloric acid. . . . I have as yet been unable to determine the conditions under which it occurs in the gastric juice, and those under which it is absent."

Scherer¹⁷ has recently published an excellent memoir, 'On the Recognition of small quantities of Lactic Acid in Animal Matters.'

Hippuric acid (or, rather, hippurate of soda) has been found by Verdeil and Dollfus¹⁸ in the blood of the ox; and Hervier¹⁹ has also found it in morbid human blood. The hippurates of soda and potash have been found by Robin and Verdeil in the urine of the pig, but they could not detect the presence of these salts in the urine of the dog. Duchek²⁰ found hippuric acid in his own urine whenever he had taken green plums (the ripe fruit of *prunus domestica*). Their analysis, however, revealed the presence of a considerable quantity of benzoic acid. The hippuric acid began to appear about seven or eight hours after he had eaten the fruit, and ceased to appear from three to five hours later.

Schlossberger²¹ has detected hippuric acid in the cutaneous scales in a well-marked case of ichthyosis. He digested a quarter of a pound of the scaly matter for fourteen days in alcohol of 80°, and obtained a deep yellow tincture, which, when carefully evaporated, yielded a thick syrup. A microscopic examination showed the presence of numerous fat-globules, and of beautiful tablets of cholestérine; and on treating this syrup with a comparatively small quantity of spirit (so as not to take up much fat), the filtered yellow solution yielded on spontaneous evaporation, club-like crystals, mostly arranged in twos in oblique crosses; these crystals resolved themselves under the microscope into conical tufts of four sided prisms, presenting the greatest similarity with the groups of hippurate of lime depicted by Robin and Verdeil in their 'Atlas.' No trace of lime could, however, be detected in these crystals, which were acid, difficult of solution in cold water and in ether, less so in alcohol, and readily soluble in boiling water. They remained unchanged in mineral acids, but dissolved in a solution of potash; when heated in a glass tube, they readily fused into a yellow oil, and then developed white vapours, which condensed in a solid form on the walls of the tube. On the application of a stronger heat an unmistakable odour of prussic acid was recognised, and a blistered carbonaceous residue was left, which burned away on a platinum spatula without leaving any residue. The aqueous solution of the crystals yielded, with nitrate of silver and a drop of ammonia, a white curdy precipitate, like chloride of silver, which on boiling dissolved in water.

From these reactions, Schlossberger felt assured that the crystals were actually composed of hippuric acid, which has hitherto been found only in the urine and the blood; and this result is the more singular, since Schottin, in the memoir 'On the Sweat,' to which we have already referred, found that benzoic acid, when administered to a healthy man, appeared unchanged in the sweat, and not in the form of hippuric acid, as in the urine.

It has been long known that benzoic acid becomes converted in the human body into hippuric acid, in which form it is excreted by the kidneys. It has been shown

¹⁵ Arch. Gen. de Méd., 1853, cinquième série, vol. ii. pp. 1—21.

¹⁶ Traité de Chimie Anat., vol. ii. p. 377.

¹⁷ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. iv. p. 235.

¹⁸ Traité de Chimie Anat., vol. ii. p. 446.

¹⁹ Gaz. Méd. de Paris, 1853, No. 5.

²⁰ Prag. Viertelj., 1854: quoted in p. 270 of this volume.

²¹ Ann. der Chem. und Pharm., vol. xc. p. 376.

by the later experiments of Erdmann and Marchand,²³ that cinnamic acid undergoes a similar change; and this has led to the inquiry, whether other acids of the benzoic acid group, as, for instance, toluylic and cumic (or cuminic) acids, underwent the same change. The recent observations of Hofmann²⁵ and Ranke²⁴ show that this is not the case.

Uric acid has been found by Scherer²⁶ in considerable quantity, as a normal constituent, in the juice of the spleen. Mr. Henry Gray,²⁶ being anxious to confirm Scherer's observation, worked in one experiment on the spleens of twenty-five oxen, but wholly failed in detecting this substance. (Mr. Gray's chemical experiments were, we believe, superintended by Dr. Noad.)

Cloëtta²⁷ (in vainly searching for pulmonic acid) found uric acid in the pulmonary tissue. He examined the lungs of six oxen, and always obtained this substance from the water-extract. The following was his method of procedure: the chopped lung, after remaining in distilled water for twenty-four hours, was exposed to very strong pressure. The albumen and pigment were removed by boiling, and the filtered fluid was then treated with baryta water till no additional precipitate was formed. The filtrate was now slowly evaporated on the water-bath, the baryta compounds that formed on the surface being removed as they appeared. When the volume of the fluid did not amount to much more than 50 c. c.,²⁸ a little acetic acid was added; and after some time a precipitate was formed, which exhibited the chemical reactions and the well-known crystalline forms of uric acid.

He obtained 60 milligrammes of uric acid from the water-extract of one ox-lung.

Reference has been made, in a previous page, to Dr. Garrod's observations on uric acid in the blood and blister fluid, and its absence in the sweat in one case.

Arppe²⁹ has published an improved method of obtaining uric acid from pigeons' dung.

Pneumic (or *pulmonic*) acid probably belongs to the conjugated nitrogenous acids. This acid, of which as yet we know very little, was discovered by Verdeil³⁰ in the tissue of the lungs. The minced pulmonary tissue is stirred with water, and exposed to strong pressure; the decanted acid fluid is heated in order to coagulate the albumen, and is then filtered, neutralized with baryta water, and evaporated to three-fourths of its volume. After the removal of albuminous and some other matters by sulphide of barium, we evaporate the fluid till crystals of sulphate of soda are formed; we then add a little sulphuric acid, and boil with alcohol. The acid gradually separates from the alcoholic solution on cooling; it crystallizes in oblique rhombic prisms, is extremely glistening, and refracts light strongly, loses no water of crystallization at 212° Fahr., but at a higher temperature decomposes. It dissolves readily in water; is insoluble in cold, but dissolves in boiling, alcohol; is insoluble in ether, forms crystallizable salts with bases, and contains not only carbon, hydrogen, and oxygen, but also nitrogen and sulphur.

It appears to be a constituent of the lungs of all mammals. Verdeil obtained about five centigrammes from the lungs of a perfectly healthy woman who was guillotined. Morbid conditions appear to occasion an augmentation rather than a diminution of this substance: thus, a single lung from a man with general pneumonia in its second stage, yielded rather more than the two lungs of the guillotined woman. It appears to be formed in the substance of the lung itself, and probably bears much the same relation to the pulmonary tissue that creatine does to muscle. Verdeil believes, that by decomposing the carbonates of the

²³ Journ. f. Pr. Chem, vol. xxxv. p. 307.

²⁵ Ann. der Chem. und Pharm., vol. lxxiv. p. 342.

²⁴ Journ. f. Pr. Chem, vol. lvi. p. 3—6.

²⁵ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

²⁶ Gray on the Structure and Use of the Spleen, p. 209. London, 1854.

²⁷ Arch. für Path. Anat., vol. vii. p. 168.

²⁸ c. c. indicate cubic centimetres.

²⁹ Ann. der Chem. und Pharm., vol. lxxxvii. p. 237.

³⁰ Traité de Chimie Anat., vol. ii. p. 460.

blood with which it comes in contact, it contributes very considerably to the evolution of carbonic acid, and is thus an important factor in the respiratory process. This, however, requires confirmation.

Two new acids have been obtained by Marcet from human urine. His own description of them may be found in vol. xi. p. 371, of this Review.

Cynuric acid is the term given by Liebig³¹ to an acid which he has discovered in the urine of dogs, and which seems to take the place of uric acid. He was only able to obtain it in a comparatively small number of cases, in which it was deposited, in the form of a very minute precipitate, after the urine had stood for some days (and, we presume, after a few drops of some acid had been added). It contains little or no nitrogen. For further chemical particulars regarding it we must refer to the original memoir.

BASIC BODIES.

Nitrogenous Basic Bodies next claim our attention. Those which do not contain oxygen, although of very high interest in a chemical point of view, scarcely come within the scope of this Report, except in so far as some of them have been recently found to occur, more frequently than was formerly supposed, as products of the decomposition of animal matter. Thus, for instance, trimethylamine ($3 \text{C}_2\text{H}_5 + \text{N}$) has been found in herring-brine by Wertheim,³² who, however, mistook it for propylamine ($\text{C}_6\text{H}_7 + \text{N H}_2$) till Hofmann and Winkles³³ recognised its true nature; and, by Buckheim,³⁴ in the spirit in which anatomical preparations had been long kept. Winckler³⁵ obtained a fluid containing propylamine by distilling fresh urine with lime. Schlossberger³⁶ believes that the poison which is occasionally present in foreign sausages is due to the development of one of these alkaloids.

Of the nitrogenous basic bodies containing oxygen we must notice the following:—Leucine, tyrosine, hypoxanthine, xanthocystine, lienine, thymine, myeline (?), creatine, creatinine, urea, allantoin, and taurine.

Leucine is best prepared, according to Zollikofer,³⁷ by boiling the ligamentum nuchæ of the ox (or any other elastic tissue) for forty-eight hours in sulphuric acid diluted with one and a half times its bulk of water, neutralizing with milk of lime, boiling the pulpy mass which is thus formed, and filtering. The salts of lime, which become deposited during evaporation on the sand-baths, must be removed as much as possible; on further evaporation on the water-bath, crystals of leucine are separated in abundance.

Leyer and Köller³⁸ have shown that albumen, fibrine, caseine, horn, feathers, hedgehog-spines, hairs, and the elytra of the cockchafer, yield, as products of their decomposition, leucine and another base, to which we shall presently refer, tyrosine, when treated with dilute sulphuric acid.

Reinhold Hofmann³⁹ has proposed a test (or rather a modification of an old test by Braconnet) for distinguishing whether leucine is free from tyrosine, a basic body which is often associated with it. Pure leucine is precipitated by nitrate of protoxide of mercury in white flakes, without any reddening of the supernatant fluid; any reddening that may occur being due to the presence of tyrosine.

³¹ Ann. der Chem. und Pharm., vol. lxxxvi. p. 125; or Chem. Gaz., vol. xi. p. 315. 1853.

³² Journ. für Pr. Chem., vol. lili. p. 435.

³³ Ann. der Chem. und Pharm., vol. lxxxiii. p. 116; Quart. Journ. Chem. Soc. vol. vii. p. 63. 1854.

³⁴ Lehmann's Lehrbuch, vol. i. p. 129.

³⁵ Buchner's Repert. für Pharm.: quoted in Chem. Gaz., vol. x. p. 320. 1852.

³⁶ Arch. für Physiol. Heilk., vol. xi. p. 742.

³⁷ Ann. der Chem. und Pharm., vol. lxxxii. p. 168.

³⁸ Journ. für Pr. Chem., vol. lviii. p. 273; Ann. der Chem. und Pharm., vol. lxxxiii. p. 332; and Chem. Gazette, vol. xi. p. 346. 1853.

³⁹ Ann. der Chem. und Pharm., vol. lxxxvii. p. 123.

Liebig⁴⁰ states that he has found leucine in the decoction of the liver of the calf; and according to Robin and Verdeil⁴¹ it exists in the pulmonary tissue and in the blood; they have not, however, attempted to determine it quantitatively. Frerichs and Städeler⁴² have found it in association with tyrosine in the human liver, in certain diseased states; these bodies have also been found in the blood of typhus and variolous patients; and leucine has been found in the urine in these cases.

Gössmann⁴³ has succeeded in artificially converting thialdine ($C_{12}H_{12}N S_2$) into leucine ($C_{12}H_{12}N O_4$).

Tyrosine ($C_{10}H_9N O_3$) is formed, along with leucine and other compounds not yet investigated, when albuminous or horny bodies are decomposed either by acids, alkalis, or putrefaction. Piria,⁴⁴ has recently described a simple method of obtaining this substance from horn shavings.

We have already referred to the sources of tyrosine discovered by Leyer and Köller, who give in their memoir full particulars of the experiments which they performed.

Piria gives the following test for the detection of tyrosine, which is available even when the quantity does not exceed five or six milligrammes. It is based upon the fact, that when sulphuric acid acts on tyrosine, there is a formation of tyrosine-sulphuric acid, whose neutral salts yield a dark violet colour with perchloride of iron. Place a little tyrosine (a few milligrammes) on a watchglass, moisten it with one or two drops of sulphuric acid; after allowing the glass to stand covered for half an hour, dilute the mixture with water, saturate it, when heated, with carbonate of lime, and filter; if we now add to the filtrate a few drops of a solution of perchloride of iron in which there is no free acid, we at once obtain a very rich violet colour, which is very similar to that which this reagent induces with salicylic acid.

Reinhold Hofmann⁴⁵ gives the following test for tyrosine. Its solution at a boiling heat is precipitated by nitrate of protoxide of mercury in red flakes, and the supernatant clear fluid assumes a dark rose-red colour, which, however, disappears after a time with a deposition of red flakes. The red colour is permanently destroyed on heating the mixture with a little nitric acid, the tint not being restored by subsequent neutralization. By means of this colour, one part of tyrosine may be detected in more than 1000 of fluid. If an excess of acid be present in the nitrate of mercury, there is neither colour nor precipitation.

Hypoxanthine ($C_{10}H_4N_4O_2$) has been found by Gerhard⁴⁶ (a pupil of Scherer's) in the blood of oxen, and by Scherer⁴⁷ himself, in larger quantity, in the blood of a patient with leucæmia.

Mr. Gray⁴⁸ failed in confirming Scherer's observations regarding the occurrence of this substance in the spleen.

Xanthocystine is the term applied by Chevallier and Lassaigne⁴⁹ to a substance which they extracted from the miliary tubercles in a dead body that had been buried for two months. It was insoluble in water and alcohol, but dissolved in ammonia and in the mineral acids; the ammoniacal solution deposited minute white granules on evaporation; hexagonal tablets separated from the acid solutions on evaporation; the substance did not fuse on heating, but puffed up, became

⁴⁰ Letters on Chemistry, third edition, p. 354; and Gregory's Handbook of Organic Chemistry, third edition, p. 422. 1852.

⁴¹ Traité de Chimie Anat., vol. iii. p. 420.

⁴² Quoted in p. 273 of the present volume.

⁴³ Ann. der Chem. und Pharm., vol. xc. p. 184.

⁴⁴ Ibid., vol. lxxxii. p. 281.

⁴⁵ Ann. der Chem. und Pharm., vol. lxxxvii. p. 124.

⁴⁶ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

⁴⁷ Ibid., p. 324.

⁴⁸ Op. cit., p. 209.

⁴⁹ Journ. de Chim. Méd., troisième série, vol. vii. p. 208; and quoted in Lehmann's Lehrbuch, vol. i. p. 146.

yellow and black, developed an odour of burned horn, and gave off alkaline vapours. The investigation was not carried any further.

Lienine is a new nitrogenous substance, obtained by Scherer⁵⁰ from the fluid of the spleen. It contains no sulphur, and consists of C 53·71, H 8·95, N 4·82, and O 32·52.

Thymine is a new organic base, discovered by Gorup-Besanez⁵¹ in the tissue of the thymus gland. It was obtained by thoroughly extracting minced thymus glands (from the calf) with cold water, the fat having been previously removed. The fluid thus obtained was freed from albumen by boiling in the water-bath, was then treated with baryta water as long as any turbidity ensued, was filtered, and carefully evaporated on the water-bath to the consistence of a syrup. The residuc, when extracted with spirit, and allowed to evaporate slowly, deposited impure thymine in wart-like granular masses, which was obtained in a state of purity by repeated solution in boiling alcohol, from which it was precipitated on cooling and evaporation. Only 200 milligrammes were yielded by twenty-one pounds of glandular structure. It was obtained pure, and in a crystalline form, by repeated solution in hot alcohol.

It has not yet been submitted to ultimate analysis, but the following properties of it have been ascertained :

Thymine forms slender snow-white crystals, grouped concentrically ; it is perfectly inodorous and tasteless. It is readily soluble in water, and in boiling alcohol, difficult of solution in cold alcohol, and insoluble in ether. The concentrated aqueous solution exerts no action on test-paper. When quickly heated on platinum foil it burns with a bluish and not very luminous flame, and leaves no residue ; when heated in a glass tube it yields a sublimate.

It dissolves in potash without any development of ammonia, and is likewise soluble in caustic ammonia. It forms crystalline compounds with acids, and a double salt with bichloride of platinum ; the latter crystallizes in bright yellow octohedra, which are insoluble in alcohol, but dissolve tolerably easily in water.

The substances which it most closely resembles are sarcosine and alanine. It may be distinguished from sarcosine by its crystalline form, its tastelessness, and the form and character of its compounds with acids (hydrochloric and sulphuric), and with bichloride of platinum ; while it differs from alanine in its not having a sweet taste, in the insolubility of its platinum compound in alcohol, and in the character of its salts.

Myeline is the name given by Virchow⁵² to a substance which he has discovered in diseased lungs, in an hepatic cyst, in healthy and diseased spleens, &c., &c. He has given it this name from its resemblance to the nerve-medulla. Its chemical nature seems extremely doubtful.

Creatine has been found by Lehmann⁵³ in the smooth muscles of the stomach of the pig, and by Siegmund⁵⁴ in the muscular substance of the uterus in the eighth month of pregnancy. Verdeil and Marcet⁵⁵ have found it in the blood.

A most elaborate account of the modes in which creatine crystallizes is to be found in Robin and Verdeil,⁵⁶ and ten figures of the crystals are given in their 'Atlas.' The crystalline characters are also given in Funke's 'Atlas.'

Dessaigues⁵⁷ has examined some of the products of the transformation of creatine, and has attempted to determine its rational formula.

⁵⁰ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

⁵¹ Ann. der Chem. und Pharm., vol. lxxxix. p. 115 ; and Chem. Gaz., vol. xii. p. 137, 1854 ; or Gorup-Besanez, Anleitung zur Zoochem. Analyse, p. 119, 1854.

⁵² Arch. für Path. Anat., vol. vi. p. 562.

⁵³ Lehrbuch der Phys. Chemie, vol. i. p. 150.

⁵⁴ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 50.

⁵⁵ Traité de Chimie Anat., vol. ii. p. 480.

⁵⁶ Op. cit., vol. ii. pp. 483—488.

⁵⁷ Comptes Rendus, May 8, 1854 ; and Chem. Gaz., vol. xii. p. 201, 1854.

Creatinine has been found by Verdeil and Marcet⁵⁸ in the blood; the same chemists have detected it in the urine of the horse, the pig, and the sheep; and Socoloff⁵⁹ has found it in the urine of the horse and the calf.

Much original matter regarding the forms in which it crystallizes will be found in Robin and Verdeil's work.

Urea.—Several important memoirs have been recently published regarding this substance; amongst which we must especially notice Liebig's⁶⁰ memoir "On certain Urea-compounds, and a New Method of Determining the Chloride of Sodium and the Urea in Urine;" and Bischoff's⁶¹ treatise 'On Urea as a Measure of the Metamorphosis of Tissue.'

We have not space to enter fully into the description of Liebig's⁶² volumetric method for the determination of the urea and the chloride of sodium in the urine. It rests essentially upon the three following facts:

1. Nitrate of protoxide of mercury at once induces a thick white precipitate in a solution of urea; this precipitate does not occur on the addition of a solution of chloride of mercury (corrosive sublimate). On mixing a solution of urea with chloride of sodium, and adding gradually, in small portions, a dilute solution of nitrate of protoxide of mercury, a white cloudiness ensues at the place where both fluids meet; disappearing, however, immediately on shaking, leaving the liquor as bright and transparent as before; without the chloride of sodium it would have retained its cloudiness. This deportment lasts until the nitrate of protoxide added exactly suffices to transform the chloride of sodium into corrosive sublimate; beyond this point a single drop of the mercurial salt produces a lasting white turbidity ($\text{HgO.NO}_3 + \text{Na.Cl.} = \text{Hg.Cl.} + \text{NaO.NO}_3$). If we know the amount of mercury in the solution of nitrate of protoxide of mercury, which has been added to a solution of urea containing an unknown quantity of chloride of sodium, until a permanent precipitate is formed, we consequently know the amount of chlorine or chloride of sodium present in the solution; one equivalent of mercury in the mercurial solution used, corresponding exactly to one equivalent of chlorine or chloride of sodium. Conversely, if the amount of chloride of sodium in the solution of urea be known, the amount of mercury in the mercurial solution that is consumed may be calculated.

2. On mixing solutions of nitrate of protoxide of mercury and common phosphate of soda, a white precipitate of phosphate of protoxide of mercury is at once formed, which soon becomes crystalline; a solution of corrosive sublimate may, however, be mixed with the alkaline phosphate without any turbidity. If we treat the fluid in which the phosphate of protoxide of mercury is deposited with chloride of sodium, before the precipitate has become crystalline, it re-dissolves, being decomposed with the chloride of sodium into corrosive sublimate and phosphate of soda. One equivalent of phosphate of protoxide of mercury requires to re-dissolve it one equivalent of chloride of sodium; if, therefore, the amount of chloride of sodium added be known, the amount of mercury in the mercurial solution may be calculated.

3. On gradually adding to a dilute solution of urea an equally dilute solution of nitrate of protoxide of mercury, and neutralizing the free acid of the mixture from time to time by baryta water or a dilute solution of carbonate of soda, a flocculent snow-white precipitate is obtained, which is quite insoluble in water. If the addition of the salt of mercury and of carbonate of soda be continued alternately as long as this precipitate is formed, a point is reached at which the addition of a drop of carbonate of soda occasions a yellow colour, from the formation of the hydrated oxide or the basic nitrate of mercury. All the urea is

⁵⁸ *Traité de Chimie Anat.*, vol. II. p. 489.

⁵⁹ *Ann. der Chem. und Pharm.*, vol. LXXX. p. 114.

⁶⁰ *Ibid.*, vol. LXXXV. pp. 289—328.

⁶¹ *Der Harnstoff als Maass des Stoffwechsels*. Giessen, 1853.

⁶² We the less regret this omission, because the entire memoir is translated in vol. VI. of the *Quarterly Journal of the Chemical Society*.

then precipitated, and the precipitate contains four equivalents of protoxide of mercury to one equivalent of urea. Hence, if the quantity of mercury in the mercurial solution be known, we may calculate the amount of urea in the fluid, by observing the quantity of mercurial solution required for the complete precipitation. (We shall presently show that the occurrence of *allantoine* might interfere with the correctness of our results in determining urea quantitatively by this method.)

The following remarks, from the new edition of Lehmann's 'Physiological Chemistry,' will sufficiently explain the general steps of the process:—

"Liebig⁶³ has suggested a very ingenious method for determining volumetrically the amount of urea in the urine, which is closely connected with a chemical fact which he has recently discovered⁶⁴—namely, that if chloride of mercury (corrosive sublimate) in solution, and bicarbonate of potash in excess, be added to a solution

of urea, we obtain a compound of urea and mercury, $\overset{+}{U} + 4 \text{ Hg O}$, which is perfectly insoluble in water. This method has, further, this advantage, that we simultaneously determine the amount of *chlorine* in the urine. The following are the main steps in the process. In order to remove the phosphates and sulphates of the urine, a definite quantity of the fluid is mixed with half its volume of a fluid containing one volume of a saturated solution of nitrate of baryta to two volumes of a saturated solution of caustic baryta. We take about 15 c. c. of the filtered alkaline fluid (which consequently contains for every three volumes, two volumes of urine), and then, without neutralizing it, we add from a burette a solution of nitrate of protoxide of mercury of known strength, as long as any precipitate is formed. The mixture must be well stirred during this process. The precipitate is the above-mentioned compound of urea and protoxide of mercury

$\overset{+}{[U + 4 \text{ Hg O}]}$. When a few drops of the turbid fluid are poured into a watch-glass, and one drop of a solution of carbonate of soda is added, the mixture soon becomes yellow when treated with an excess of the solution of mercury, but it remains white when the solution of mercury is insufficient to precipitate all the urea. Very different methods may, of course, be employed for the preparation of the test-fluid (of nitrate of protoxide of mercury); Liebig has, however, proposed a very simple method for this purpose, which depends upon the fact that nitrate of the protoxide is decomposed by phosphate of soda, but that chloride of mercury (corrosive sublimate) is not thus affected. If, however, a solution of common salt, of known concentration, be added to a mixture of these salts before the precipitate of the phosphate of mercury has become crystalline, the quantity of the oxide of mercury may be very easily calculated from the volume of the chloride of sodium necessary for its re-solution (for one equivalent of chloride of sodium necessarily corresponds to one equivalent of the phosphate of mercury). We may, however, at once obtain a solution of chloride of sodium suited for the purposes of these experiments, when we consider that a solution which is saturated between the temperatures of 0° and 100° C. constantly contains 27% of salt.

"The method of determining the amount of *chlorine* in the urine is based upon the fact that, on the one hand, urea may be precipitated by the nitrate or protoxide, but not by the chloride of mercury; and, on the other hand, that the nitrate becomes converted into chloride of mercury when brought in contact with chloride of sodium. In order, therefore, to find the amount of chlorine in the urine, a definite volume of it should be decomposed with the solution of baryta; the urine which is filtered from the precipitate should then be treated with nitric acid until it is completely neutralized, and the solution of the nitrate of mercury poured upon it until the precipitate no longer dissolves on being stirred (that is to say, as long as chloride of mercury is formed). The quantity of the chloride of mercury, or of the chlorine, contained in the urine may be calculated from the volume of the solution of mercury which has been used."

⁶³ Ann. der Chem. und Pharm., vol. lxxxv. p. 289—328.

⁶⁴ Ibid., vol. lxxx. p. 123.

Liebig investigated the influence of the putrefaction of urine on this method of determining the urea, and has frequently obtained the same results with putrid urine, provided the decomposition had not progressed too far, as with fresh urine. As, however, in medical chemistry we always obtain fresh urine for examination, it is unnecessary for us to follow him in this branch of his inquiry.

In concluding this description of Liebig's celebrated method, it may be remarked, that Dr. Limpricht⁶⁵ has found that allantoine is likewise precipitated by nitrate of protoxide of mercury, in precisely the same manner as urea. Hence, if this substance were present in urine, it would be a source of error. Allantoine has, however, never yet been discovered either in normal or morbid human urine (even after the ingestion of uric acid⁶⁶), if we except the observations of Frerichs and Städelers,⁶⁷ who have found it in the urine of dogs when the respiration was impeded. (See p. 543.)

Neubauer⁶⁸ has recommended a modification of Millon's method, which consists in the decomposition of urea, by means of hyponitrous acid ($\text{NO}_2\cdot\text{HO}$), into carbonic acid, nitrogen and water, and the weighing of the carbonic acid. Neubauer induces this decomposition in an apparatus adapted to the quantitative determination of the carbonic acid, and determines the loss of weight arising from the nitrogen and carbonic acid, which are dried over chloride of calcium or sulphuric acid. On multiplying the loss of weight by 0.834,⁶⁹ we obtain the quantity of urea. Neubauer has satisfied himself by experiments that the accuracy of the result is unaffected by the extractive matters, uric or hippuric acid, or sugar. As Neubauer does not recommend, or even allude to, this method in his recently published work 'On the Analysis of the Urine,' we may conclude that he prefers Liebig's method, which he describes very fully.

Draper⁷⁰ determines the urea in urine from the quantity of carbonate of baryta which is formed when we gradually add nitroso-nitric acid to urine which has been freed by filtration from mucus and placed in a flask, through whose cork there passes a small funnel-tube, and a second tube to convey away the carbonic acid which arises. With the aid of an aspirator, the gas which is developed is caused to pass through two flasks containing baryta water. It takes about two hours to make one determination in this way. For further details, we must refer to the original memoir.

Dr. Edmund Davy⁷¹ has likewise published "A New Method of Determining the Amount of Urea," which has been already sufficiently noticed in this journal.⁷²

Dr. W. Marcet has recently pointed out a method by which we may directly obtain the urea of the urine—that is to say, without making it pass through any combinations or decompositions. Full particulars of his mode of proceeding will be found in vol. ii. p. 512 of Robin and Verdeil's 'Traité de Chimie Anatomie,' and (in his review of that work) in vol. xi. p. 375 of this journal. He arrives at the conclusion (which, as far as we are aware, has not recently been doubted either by physiologists or chemists), that "by far the largest proportion of this substance exists in the free state, as one of the immediate principles of human urine."

We postpone, till we treat of the *urine*, any consideration of the observations of Bischoff and others who have recently attempted to estimate the amount of this substance which is daily excreted. In connexion with the physiological relations of urea, we have only to observe, in the present place, that Verdeil and Dollfus⁷³ have found it in the blood of cattle, and that Moleschott⁷⁴ detected it, in combination with oxalic acid, and associated with other oxalates, in the muscular

⁶⁵ Ann. der Chem. und Pharm., vol. lxxxvii. p. 99.

⁶⁶ Ibid., vol. lxxv. p. 340.

⁶⁷ Müller's Arch. für 1854, pp. 382 and 394.

⁶⁸ Quoted in Liebig and Kopp's Jahresbericht für 1853, p. 702.

⁶⁹ According to Gorup-Besanez, Neubauer's calculation is incorrect, and the following should be the rule: Multiply the loss of weight by 60, and divide the product by 100; the quotient is the quantity of urea. See Anleitung zur Zoochem. Analyse, p. 310, note.

⁷⁰ Phil. Mag., fourth series, vol. vi. p. 290.

⁷¹ Phil. Mag.

⁷² Vol. xiv. p. 531.

⁷³ Ann. der Chem. und Pharm., vol. lxxiv. p. 214.

⁷⁴ Arch. für Phys. Heilk., vol. xi. p. 493.

juice of frogs whose livers had been extirpated some days previously (Grohé,⁷⁵ however, who has repeated these experiments under Liebig's superintendence, could find neither urica nor oxalic acid in this fluid); that Hautz⁷⁶ found about 0.5% of urea (but no uric acid) in the alkaline, almost colourless, urine of toads; and that Fiedler⁷⁷ has confirmed the observations of Schottin and others regarding its occasional presence in the sweat.

Allantoine.—In connexion with this substance, it is sufficient for us to mention Limpricht's⁷⁸ memoir (to which we have already referred) "On the Combinations of Protoxide of Mercury with Allantoine;" a brief note by Wöhler⁷⁹ "On the Fermentation of Allantoine," in which he mentions that a solution of this substance treated with yeast, and allowed to stand for four days at a temperature of 82° Fahr., ceases to contain allantoine, but contains in its place urea and the ammonia-salts of oxalic, carbonic, and some other syrupy acids; and a memoir of Frerichs and Städeler⁸⁰ 'On the Occurrence of Allantoine in Urine in Cases of Impeded Respiration;' they found this substance in the urine of two dogs, the action of whose lungs was artificially impeded; and there was a doubtful trace in the case of a man with disease of the respiratory organs.

Taurine must now be added to the list of organic bases which can be formed artificially. Strecker⁸¹ has succeeded in forming it from isethionate of ammonia, $\text{NH}_4\text{O} \cdot \text{C}_4\text{H}_5\text{O} \cdot 2\text{SO}_3$, which $= \text{C}_4\text{H}_7\text{NO}_6\text{S}_2 + 2\text{HO}$, and therefore only differs from taurine by two equivalents of water. For his mode of proceeding, we must refer to the original memoir, or to vol. iii. p. 462 of the translation of Lehmann's 'Physiological Chemistry.'

FATS AND LIPOIDS.

Heintz,⁸² who has for many years been actively engaged in the examination of the fats, has recently investigated the composition of mutton and beef suet, human fat, spermaceti, and the non-volatile fatty acids contained in butter. We may refer those of our readers who wish for the latest and most complete history of the chemistry of this class of bodies to Heintz's 'Zoochemie,' pp. 383—523, and pp. 1067—1080. In a memoir "On the Composition of Human Fat," published in vol. lxxxiv. of 'Pogg. Annalen,' and translated, in an abridged form, in the fifth volume of the 'Quarterly Journal of the Chemical Society,' Heintz gives the following as the main results of his investigation:

1. Human fat does not consist merely of olein and margarin, but is a mixture of at least *six* different fats.
2. The first of these fats is present in very small quantity only, and appears to be identical with the *stearophanin* discovered by Francis in the berries of *Cocculus Indicus*. The formula of its fatty acid is $\text{C}_{36}\text{H}_{72}\text{O}_4$.
3. The second fat is a new substance, to which he gives the name of *anthropin*; its fatty acid crystallizes readily, and its formula is $\text{C}_{34}\text{H}_{68}\text{O}_4$.
4. The third fat is *margarin*.
5. The fourth fat is *palmetin*, which yields palmetic acid by saponification; it appears to be the most abundant of the four.
6. The liquid portion of human fat is composed essentially of *olein*, but it likewise contains a small quantity of *another fat*, which, by saponification, yields an acid whose baryta salt differs essentially, both in its physical and chemical characters, from oleate of baryta.

These views are materially modified in a subsequent memoir in vol. lxxxvii. of 'Pogg. Annalen,' in which he believes that he has shown:

⁷⁵ Ann. der Chem. und Pharm., vol. lxxxv. p. 245.

⁷⁶ Ibid., vol. lxxxiv. p. 127.

⁷⁷ Schmidt's Jahrbücher, 1854, vol. lxxxiv. p. 11.

⁷⁸ Ann. der Chem. und Pharm., vol. lxxxviii. p. 94.

⁷⁹ Ibid., p. 100.

⁸⁰ Müller's Arch. für 1854, No. 4.

⁸¹ Comptes Rendus, vol. xxxix. p. 63.

⁸² Pogg. Ann., vol. lxxxiv. p. 246; vol. lxxxvii. pp. 21, 267, 588; vol. lxxxix. p. 579; vol. xc. p. 137.

1. That *anthropic acid* is a mixture of about seven parts of palmitic acid with five of stearic acid.
2. That *margaric acid* is a mixture of about ten parts of palmitic acid with one part of stearic acid.
3. That the solid part of human fat consists only of *two* fats—namely, of stearin and palmitin, in which the palmitin strongly predominates.

Stearin has been carefully examined by Duffy.⁸³

The *fat of the elephant* has been investigated by Filhol and Joly;⁸⁴ the *oil of the squalus maximus* by Ronalds;⁸⁵ and the *fat of cantharides* by Gössmann.⁸⁶

We shall notice v. Bibra's observations on the brain-fats when treating of the chemistry of nervous tissue.

In connexion with the occurrence of fat in certain organs, both in health and disease, we may refer to the observations of Lang,⁸⁷ who found from 1·8 to 3·98 of fat in the dried substance of the kidneys of cats, but was unable to detect it in the kidneys of the ox or calf; and the observations of Lehmann⁸⁸ on the occasional appearance of fat in the kidneys of deer and hares. In human kidneys, fat was as often present as absent, according to Lang. Professor Beale's investigations, in reference to the amount of fat in the human liver and kidneys, have been already published in this journal.⁸⁹ Dr. Bacon⁹⁰ found that the fat in a very fatty liver amounted to 53·13 per cent.

We have not had an opportunity of consulting a prize thesis by Schultze⁹¹ bearing upon this subject.

The question regarding the *formation of fat* in the animal economy from starch, has been most decisively confirmed (if, indeed, any confirmation were required) by the observations of MM. Lacaze-Duthiers and Riche⁹² on the insects inhabiting galls. They have examined the composition of the galls, and of the larvæ of the cynips inhabiting them, and have incontestably proved that the fat which abounds in these larvæ is produced from the starch which forms the interior of the gall in which the animal lives.

NON-NITROGENOUS NEUTRAL BODIES.

Glucose, or Grape-sugar.—Horsley⁹³ has lately proposed an alkaline solution of bichromate of potash as a test for sugar. Many of the fallacies that may exist in the application of the reduction of copper test are pointed out by Dr. Lionel Beale, in his article 'On Sugar in Urine, and its Tests,' published in this Review.⁹⁴ Frerichs and Städeler, in the memoir to which we have already alluded (see p. 543), found that allantoin reduced oxide of copper, but that creatine had no such power.

In reference to testing for sugar, we may also refer to p. 467 of the last volume of the translation of Lehmann's 'Physiological Chemistry' (Lehmann there points out the cautions that must be adopted in order to obtain correct results with Fehling's method for determining sugar quantitatively), and to a memoir by Rigaud⁹⁵ on milk-sugar, grape-sugar, and the sugar of *quercus tinctoria*, in their relation to an alkaline solution of sulphate of copper.

It is now a definitely established fact, that sugar is a normal constituent of the blood.⁹⁶ V. Becker⁹⁷ has distinctly shown, by direct experiments, that its

⁸³ Quarterly Journal of the Chemical Society, vol. v. pp. 197, 303.

⁸⁴ Comptes Rendus, vol. xxxv. p. 393.

⁸⁵ Chemical Gazette, vol. x. p. 420. 1852.

⁸⁶ Ann. der Chem. und Pharm., vol. lxxxvi. p. 317; and vol. lxxxix. p. 123.

⁸⁷ De Adipe in Urina et Renibus. Diss. Inaug. Dorpat, 1852.

⁸⁸ Lehrbuch der Phys. Chem., vol. i. p. 236.

⁸⁹ British and Foreign Medico-Chirurgical Review, vol. xii. p. 226.

⁹⁰ Quoted in British and Foreign Medico-Chirurgical Review, vol. xiv. p. 529.

⁹¹ De Adipis Genesi Pathologica: Comm. præmio orn. Gryphæ. 1852.

⁹² Ann. des Sciences Natur., quatrième série, vol. ii. p. 81.

⁹³ Medical Times, vol. ix. p. 68. 1854.

⁹⁴ Vol. xi. pp. 106—116.

⁹⁵ Ann. der Chem. und Pharm., vol. xc. p. 297.

⁹⁶ See translation of Lehmann, vol. iii. pp. 467—529.

⁹⁷ Zeltschr. für Wissensch. Zoologie, vol. v. p. 123. 1853.

amount is influenced by highly saccharine food. Thus, for instance, he found that the blood of rabbits which had been fed solely on carrots yielded 0.584% of sugar, while there was only 0.109% in the blood of those animals when fed upon oats, and only 0.045% when they had fasted for twenty-four hours. As much as 1.198% of sugar was found in the blood of a rabbit which had been so abundantly supplied with sugar, from time to time, during several hours, that some of this substance had even passed into the solid excrements.

Sugar has been detected by Frerichs⁹⁸ in the *liquid of ascites*. The case was that of a girl, aged nine years, with lardaceous liver. The liquid, removed in the ordinary manner (by tapping), was rich in sugar, a substance hitherto not found in this situation. He has since often attempted to discover it in similar cases proceeding both from hepatic and cardiac disease, but without success. Dr. G. D. Gibb⁹⁹ has likewise found sugar in the fluid of ascites.

The question regarding the *formation of sugar in the liver* is one of so much physiological interest, that we shall notice the leading investigations bearing on this subject, although one or two of them hardly fall within the limits of time to which this Report is devoted. Bernard and Barreswil¹⁰⁰ have found sugar in the hepatic tissue even of animals not using saccharine or amylaceous food. Bernard¹⁰¹ subsequently extended his researches, and showed the existence of sugar in the liver, not only of all vertebrata (so far as he experimented on them), but also in that of the Gasteropoda, Acephala, and Decapoda. Frerichs¹⁰² has confirmed these observations for the liver of man and many animals; Van der Broek for that of dogs and rabbits; Baumert¹⁰³ for that of the fox, the dog, the cat, and the sheep; and Lehmann and Kunde¹⁰⁴ for that of frogs. Vernois,¹⁰⁵ in a very elaborate memoir "On the the Sugar of the Liver, and the Modifications which it undergoes in Disease," has subsequently contributed materially to our knowledge of this subject. A brief abstract of his researches is given in vol. xii. p. 550 of this Review. We must also direct the attention of our readers to a short but very valuable article by Valentin,¹⁰⁶ "On the Existence of Sugar in the Liver and other parts of Hybernating Animals," to Dr. Pavey's¹⁰⁷ memoir "On Saccharine Matter and its Physiological Relations to the Animal Economy," and to v. Becker,¹⁰⁸ "On the Behaviour of Sugar in the Animal Metamorphosis." (Several references to v. Becker's most important memoir will be found in future parts of this Report.)

Moleschott¹⁰⁹ operated upon the livers of twelve frogs, and found undoubted evidence of sugar by Trommer's test. The question arises, Is this sugar of the liver derived from the blood, or is it formed by the actual hepatic tissue? Bernard advocates the latter view, since he has thus obtained sugar, wholly independent of food, from carnivorous and herbivorous animals, from animals during hybernation, and from the foetus. These observations have been confirmed by Frerichs, Van der Broek, Baumert. Further, both Bernard and Lehmann have found that the portal blood of the dog and horse contains little or no sugar, while the blood of the hepatic veins differs from that of any other venous blood, in containing this substance in considerable quantity. To these data, Moleschott adds an important fact. "If," he observes, "the sugar is not formed in the liver, but only strained off by it from the blood, then the blood of those animals whose liver had been removed, would be found surcharged with sugar, exactly as urea accumulates in the blood when the kidneys have been extirpated; but with frogs, some of which had been without the liver for two and even three weeks, I found no

⁹⁸ Wien. Med. Wochenschr., No. 6, 1854. See this journal, No. 28, p. 416, foot-note.

⁹⁹ Med. Times, vol. ix. p. 451.

¹⁰⁰ Comptes Rendus, vol. xxvii. p. 514.

¹⁰¹ Ibid., vol. xxxi. p. 572.

¹⁰² Handwörterbuch der Physiol., vol. iii. part 1, p. 881.

¹⁰³ Journ. für Pr. Chem., vol. liv. p. 357.

¹⁰⁴ Kunde: De Hepatis Ranarum Extirpatione. Diss. Berol., 1850.

¹⁰⁵ Arch. Gén. de Méd., cinquième série, vol. i. p. 657. 1853.

¹⁰⁶ British and Foreign Medico-Chirurgical Review, vol. xiii. p. 533. 1854.

¹⁰⁷ Guy's Hospital Reports, vol. viii. p. 319.

¹⁰⁸ Zeitsch. für Wissen. Zool., vol. v. p. 123. 1853.

¹⁰⁹ Müller's Archiv für 1853, p. 156.

sugar in the blood, flesh, gastric juice, urine, or, finally, in the water in which twenty-six of these animals, thus, mutilated, had passed two days." From these facts, Moleschott concludes that the sugar contained in the liver is formed by the liver itself.

Inosite, or muscle-sugar, has been further investigated by its discoverer, Scherer,¹¹⁰ who gives the following test for it:—If we evaporate inosite, or a mixture containing it, with nitric acid on platinum foil, almost to dryness, and then moisten the residue with ammonia and a little chloride of calcium, and again carefully evaporate to dryness, a vivid rose-red tint is exhibited on the platinum foil. Other carbo-hydrates, as milk-sugar, starch, cane-sugar, glucose, &c., yield no such reaction. The test is as sensitive as it is characteristic; half a milligramme (about $\frac{1}{170}$ th of a grain) yielding a very intense colour.

Socoloff¹¹¹ found that he could readily obtain inosite from the muscular structure of the heart, but not a trace of it from the juice of any other muscles; and Panum of Copenhagen arrived at precisely similar results.

The occurrence of *cellulose* as a constituent of the animal body, has been so fully discussed by Dr. Arlidge in the last volume of this Review (pp. 439—448, and pp. 470—482), that it is unnecessary to do more than refer our readers to that gentleman's review and memoir.

PIGMENTS.

Heintz¹¹² has published a memoir "On the Pigments in Gall-Stones," containing the results of numerous ultimate analyses.

Virchow, as is well known, some years ago discovered peculiar reddish-yellow elongated crystals in the bile of persons who had suffered from cancer of the liver, or retention of the bile consequent on catarrh of the gall-bladder.¹¹³ To these crystals he assigned the name of *bilifulvin*; and he frequently pointed out that there is a great similarity between this bilifulvin and hæmatoïdin (a red or reddish-yellow substance, either crystalline or amorphous, discovered by Virchow in extravasated blood, and to which we shall presently refer more fully). Zenker and Funke have recently shown, that if these substances are not identical, there is, at all events, the closest relationship between them, since they have independently proved that bilifulvin may be very readily converted into hæmatoïdin.

The *urine pigments* in disease have been made the subject of investigation by several chemists. We can do little more than mention the titles of their memoirs. Virchow¹¹⁴ has described the *blue pigment* which occasionally occurs, and has given the results of his investigations in the memoirs referred to in the foot-note. Heller¹¹⁵ has published a memoir "On Uro-erythrin as a Constituent of the Urine in Disease;" Dr. Harley has published two memoirs, one entitled, "Researches on the Colouring Principles of the Urine,"¹¹⁶ and the other, "On Uro-hæmatin and its Combination with Animal Resins."¹¹⁷ Kletzensky¹¹⁸ has contributed a paper "On Uro-glauclin considered as an Oxide of Indigo;" Hassall¹¹⁹ two memoirs "On the frequent Occurrence of Indigo in Human Urine, and on its Chemical, Physiological, and Pathological Relations;" and lastly, v. Sicherer,¹²⁰ a memoir "On the Formation of Indigo in the Human Organism."

¹¹⁰ Ann. der Chem. und Pharm., vol. lxxx. p. 375.

¹¹¹ Ibid.

¹¹² Pogg. Ann., vol. lxxxiv. p. 106.

¹¹³ They are depicted in Funke's Atlas, Plate VI. figs. 2 and 4.

¹¹⁴ Archiv für Path. Anat., vol. i. p. 428; Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 303; Arch. für Path. Anat., vol. iv. p. 316; and vol. vi. p. 259.

¹¹⁵ Arch. für Chem. und Mikrosk., p. 361. 1858.

¹¹⁶ Pharmaceutical Journal. Nov. 1852.

¹¹⁷ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. v. p. 1.

¹¹⁸ Arch. für Chem. und Mikrosk., p. 414. 1858.

¹¹⁹ Proceedings of the Royal Society, vol. vi. p. 327; and vol. vii. p. 122.

¹²⁰ Ann. der Chem. und Pharm., vol. xc. p. 120.

The animal pigments are very fully considered by Heintz in his 'Zoochemie;' and by Verdeil and Robin, in the third volume of their 'Traité de Chimie Anatomique.'

THE PROTEIN BODIES.

Numerous memoirs have been published upon this subject, amongst which we may especially notice the following:

MIALHE:¹²¹ On Albumen, and the different States in which it occurs in the Animal Economy.

SCHERER:¹²² On Paralbumen and Metalbumen.

PANUM:¹²³ On Fibrin in general, and its Coagulation in particular.

PANUM:¹²⁴ New Observations on the Albuminous Bodies.

LIEBERKUHN:¹²⁵ On Albumen and Casein.

LIEBERKUHN:¹²⁶ On the Action of Acetic Acid and the Alkalies on Albumen.

PARKES:¹²⁷ On the Decomposition of Chloride of Sodium by Acetic Acid in the presence of Albumen, or the Coagulation of the Albumen of the Serum in the presence of Acetic Acid and a certain amount of Chloride of Sodium.

PARKES:¹²⁸ On the Precipitation of Albumen by Acids and Neutral Salts.

LECONTE and DE GOUMOENS:¹²⁹ On the Albuminoids.

VIRCHOW:¹³⁰ On a peculiar Behaviour of Albuminous Fluids on the addition of Salts.

MÜHLHAUSEN:¹³¹ On certain Products of Decomposition of the so-called Protein Compounds.

We must content ourselves with referring the reader to the additions and notes to the translation of Lehmann's 'Physiological Chemistry,' for an abstract of the most important of the above memoirs.

There is one protein-body, however, to which we must allude more fully—namely, "the crystallizable protein substance of the blood," to which Lehmann has provisionally applied the term *Hæmato-crystalline*, and which was previously named *hæmatoidin* by Virchow.

The observations of Funke (1852), Kunde (1852), and Parkes (1852) on this subject are so fully given by Dr. Sieveking, in a critical review which recently appeared in this journal,¹³² that we need only notice the later investigations on this important subject. Lehmann has paid more attention to this substance than any other chemist; and an abstract of his views regarding it will be found in pages 485—495 of the third volume of the translation of his 'Physiological Chemistry.' He has, however, treated the subject more fully in three memoirs¹³³ in the 'Berichte der Gesellschaften der Wissenschaften zu Leipzig,' 1852.

In the latest of these memoirs he gives the ultimate analysis of hæmato-crystal-

¹²¹ L'Union Médicale, Nos. 80, 81, 83, 86, 90, 1852; and Schmidt's Jahrbücher, vol. lxxvi. p. 287. 1852.

¹²² Ann. der Chem. und Pharm., vol. lxxxii. p. 135. (Paralbumen has been recently found by C. O. Weber, in the contents of a tumour in a child nine weeks old. Arch. für Path. Anat., vol. vi. p. 520.)

¹²³ Schmidt's Jahrbücher, vol. lxxvi. p. 153. 1852. (An epitome of a Danish thesis.)

¹²⁴ Arch. für Path. Anat., vol. iv. p. 41, 1852; and Schmidt's Jahrbücher, vol. lxxv. p. 274. 1852.

¹²⁵ Pogg. Ann., vol. lxxxvi. pp. 117, 296. 1852. (An abstract of this memoir is given in the Chemical Gazette, vol. x. p. 366. 1852.)

¹²⁶ Arch. für Path. Anat., vol. v. p. 162.

¹²⁷ Medical Times, new series, vol. i. p. 84. 1850.

¹²⁸ Medical Times and Gazette, vol. v. p. 6. 1852.

¹²⁹ Arch. Gén. de Méd., Juin, 1853; and British and Foreign Medico-Chirurgical Review, vol. xiii. p. 538.

¹³⁰ Arch. für Path. Anat., vol. vi. p. 572.

¹³¹ Ann. der Chem. und Pharm., vol. xc. p. 171.

¹³² British and Foreign Medico-Chirurgical Review, vol. xii. p. 348. 1853.

¹³³ A good abstract of these memoirs is given in the Annalen der Chem. und Pharm., vol. lxxxviii. p. 377.

line obtained from the blood of the dog. After being extracted with alcohol, ether, and water, it yielded, after deducting the ash, the following numbers :

	I.	II.	III.
Carbon	55.41 ...	55.24 ...	55.18
Hydrogen	7.08 ...	7.12 ...	7.14
Nitrogen	17.27 ...	17.31 ...	17.40
Oxygen, with a little sulphur . .	20.24 ...	20.33 ...	20.28

The sulphur in three cases was determined at 0.253, 0.206, and 0.248 per cent.

We believe that the latest researches on this subject are those of Teichmann,¹³⁴ whose memoir "On the Crystallization of the Organic Constituents of the Blood," has been fully noticed in vol. xiii. p. 541 of this Review.

THE MINERAL SUBSTANCES OF THE ANIMAL BODY.

Burin du Buisson¹³⁵ has confirmed the statements of Millon and Hannon that *manganese* exists in human blood. The following is the best method of demonstrating its existence:—We treat the blood-ash with a mixture of nitric and nitrous acids, boil it, evaporate it, and re-dissolve it in water, till it becomes neutral; we then precipitate the iron with benzoate of ammonia, and afterwards the manganese with carbonate of soda. As a mean of ten experiments, he found that 1000 parts of blood contained 0.078 of peroxide of manganese. He further believes that he has found that the manganese belongs solely to the corpuscles, and not to the serum.

Wackenroder¹³⁶ has published an elaborate memoir "On the Presence of Small Quantities of *Copper* in the Animal Organism." The following are his conclusions :

1. Domestic animals which live upon pure vegetable food have no copper in their blood, or, at least, not sufficient to render it observable in half a pound of blood.

2. The blood of man and domestic animals living upon mixed food may contain very considerable quantities of copper, and sometimes also of lead. The presence of these metals is, however, by no means to be regarded as constant or normal.

3. The origin of these small quantities of copper and lead can only be attributed to nutritive or medicinal substances containing these metals.

4. It is not probable that these portions of copper and lead, small though they be, can remain without any influence on the human organism.

5. The bodies of many animals of the lower classes, snails for instance, invariably contain a proportionally large quantity of copper.

Considerable discussion has recently taken place regarding the presence of *iodine* in organized beings. Chatin a few years ago found it not only in all spring waters, in plants, and in most kinds of food and drink, but also as an essential constituent in land and fresh-water animals. Lohmeyer¹³⁷ has tested Chatin's statements with great care. Besides finding no iodine in the air (in which Chatin determined it *quantitatively*), he was unsuccessful in searching for it in cows' milk or in hens' eggs.

THE CHEMISTRY OF THE ANIMAL FLUIDS.

The Saliva.—The chemistry of this secretion was so fully discussed by the author of this Report on a recent occasion in this journal (vol. xii. pp. 167—178), that little remains to be noticed on the subject. The most important works and memoirs published since the beginning of 1852 on the saliva, are :

¹³⁴ Zeitschr. für rat. Med., new series, vol. iii.

¹³⁵ Revue Médicale, Février, 1852.

¹³⁶ Arch. der Pharm., vol. lxxv. pp. 140, 268; and vol. lxxvi. p. 1. (Quoted in the Chemical Gazette, vol. xii. p. 172. 1854.)

¹³⁷ Nachrichten von der Gesellsch. der Wiss. zu Göttingen, p. 181. 1853. (Quoted in the Chemical Gazette, vol. xi. p. 352. 1853.)

1. BIDDER AND SCHMIDT: *Verdanungssäfte und Stoffwechsel*, pp. 1—28 (of which we have given a tolerably full abstract in the above-mentioned review).
2. COLIN:¹³⁸ *Experimental Researches on the Salivary Secretion in the Solidungulates and Ruminants.*
3. BECHER AND LUDWIG:¹³⁹ *On the Law which Regulates the Chemical Composition of the Submaxillary Saliva in the Dog.*
4. BERNARD:¹⁴⁰ *On Elective Elimination by the Salivary and other Secretions.*
5. BERNARD:¹⁴¹ *A Memoir on the Saliva.*
6. THORNTON HERAPATH:¹⁴² *On the Quantitative Determination of the Sulpho-cyanide of Potassium in Saliva.*

Colin finds that in the solidungulates the secretion of the two parotids alternates, the parotid of the side on which mastication is going on secreting at least one-third more than that of the opposite side. He did not observe this alternating action in the secretion of the submaxillary glands, which is apparently uniform on both sides. When the animal consumes dry food, there are secreted from 5000 to 6000 grammes of saliva from all the glands in the course of one hour; about one-third or one-fourth more when the animal consumes oats, and one-third or one-fourth less when living on succulent roots.

Becher and Ludwig found that the solid residue of the saliva diminishes in proportion to the amount which the gland has already yielded; the organic constituents sinking far more rapidly than the inorganic. Fluctuations in the quantity of water in the blood did not disturb this law, as was proved by the examination of saliva collected after one or more venesections; nor was it affected by the injection of chloride of sodium into the blood, although the quantity of the salts in the saliva was somewhat augmented thereby.

Bernard's memoir "On Elective Elimination by the Salivary and other Secretions," has been already noticed in this journal (vol. xi. p. 558).

Bernard's memoir "On the Saliva," although of considerable length, contains little or nothing that is worthy of being extracted. The organic substances occurring in mixed saliva, are (according to him) albumen, casein, epithelium, phosphorized fat, mucus, and a peculiar organic matter; and he denies the ordinary occurrence of sulpho-cyanide of potassium in the saliva.

The following is Herapath's method of determining the sulpho-cyanogen in saliva. The saliva was first evaporated to dryness in a water-bath or gas-oven, and the residue was acted upon with dilute hydrochloric acid. The solution, after being filtered, in order to remove any flocculi that might be suspended in it, was poured into a colorimeter, where it was diluted with distilled water to the necessary degree, care having been taken to add previously a drop or two of a solution of perchloride of iron. The test produced was then compared with that of a standard solution of pure sulpho-cyanide of ammonium, containing a known per-centage of sulpho-cyanogen. The following is an abstract of his results: In 10,000 parts of the saliva of a man aged 24 years, the mean of five experiments was 0.379 of sulpho-cyanogen (or 0.634 of sulpho-cyanide of potassium). In two of these experiments, where the saliva was obtained by smoking tobacco, the mean quantity of sulpho-cyanogen was 0.605; while, in the three cases in which it was not smoked, the mean quantity was only 0.288. In 10,000 parts of the saliva of a woman aged 28, the mean of three experiments was 0.104 (corresponding to 0.173 of sulpho-cyanide of potassium); the maximum being 0.2027, and the minimum 0.0486. Hence it is obvious that the amount of sulpho-cyanogen in the saliva, even of the same individual, is liable to great fluctuations.

The Gastric Juice has been made the subject of inquiry by the following chemists:

¹³⁸ *Comptes Rendus*, vol. xxxiv. pp. 681, 977.

¹³⁹ *Zeltsch. für rat. Med.*, new series, vol. i. p. 278.

¹⁴⁰ *Arch. Gén. de Méd.*, new series, vol. i. p. 5.

¹⁴¹ *Gazette Médicale*, Nos. 7, 11, 22, 23. 1853.

¹⁴² *Chemical Gazette*, vol. xi. p. 294. 1853.

1. BIDDER AND SCHMIDT: Die Verdauungssäfte und der Stoffwechsel, pp. 29—97. (Noticed fully in the review already mentioned.)
2. GRUENEWALDT: Succus Gastrici Humani Indoles Physica et Chemica, etc. 1853.
3. SCHROEDER: Succus Gastrici Humani Vis Digestiva, etc. 1853.

These theses by Gruenewaldt and Schroeder have been so fully noticed in p. 263 of this volume, that it is needless to discuss them in this, their more proper place. We will merely remark that the *sarcina* was frequently observed in the gastric juice examined by Gruenewaldt, both when the stomach was empty and when full, the woman with a gastric fistula on whom he experimented being apparently in perfect health. Hence he agrees with Virchow,¹⁴³ that this organism must not be regarded as a special symptom of a peculiar form of disease.

The *Bile* has not been submitted to chemical examination during the time to which this Report is devoted, but a very large amount of information on many points bearing on its chemistry and physiology, will be found in the work of Bidder and Schmidt, to which we have so often referred (pp. 98—239). (The most important of their results are given in the Appendix to the translation of Lehmann's 'Physiological Chemistry.')

Moleschott¹⁴⁴ has recently instituted a series of carefully conducted experiments on frogs, with the view of determining the seat of the formation of the main constituents of the bile. Like Kunde,¹⁴⁵ he extirpated the liver, but succeeded in keeping the animals alive for a longer period. He could not succeed in detecting a trace of the biliary acids or of bile-pigment, either in the blood, or in the lymph, or in the flesh, or in the urine of the frogs on which he operated. Hence he regards it as an established fact, that the essential constituents of the bile are previously formed in the liver.

Enderlin,¹⁴⁶ however, believes that cholic acid (or cholate of soda) is a normal constituent of the blood. He is led to this view from observations made with Pettenkofer's test on the alcoholic extract of the dried blood of a pregnant woman, and on that of animals. Some of Enderlin's previous results have been completely overthrown by subsequent inquiry, and such will probably be the fate of this.

Planta and Kekule¹⁴⁷ have published analyses of two gall-stones found in the gall-bladder of a man, aged 60, who committed suicide; and Wolff¹⁴⁸ has analyzed a concretion found in the gall-bladder of a woman, aged 29, who died from tuberculosis.

The *Pancreatic Juice* has been examined by the following chemists:

1. BIDDER and SCHMIDT: Die Verdauungssäfte und der Stoffwechsel. pp. 240—259.
2. COLIN:¹⁴⁹ On the Pancreatic Secretion of the larger Domestic Ruminants.
3. WEINMANN: Ueber die Secretion der Bauchspeicheldrüse. Dissertatio inaug. 1852.
4. KROEGER: De Succo Pancreatico. Dissertatio inaug. 1854.

The quantity of the pancreatic juice varies very much in different animals. According to Colin, it does not stand in a direct ratio to the volume of the gland. While the pancreas of the ox and of the horse yields 260 or 270 grammes in an hour, that of the swine, which is about half the size, yields only 12 or 15 grammes in an hour. Bidder and Schmidt found that a strong dog, weighing 20 kilogrammes (the kilogramme is about 2·2 lbs. avordupois), secreted 7·86 grammes

¹⁴³ Arch. für Path. Anat., vol. i. p. 268.

¹⁴⁴ Arch. für Physiol. Heilkunde, vol. xi. p. 479.

¹⁴⁵ Diss. Inaug. Berol. 1850.

¹⁴⁶ New York Mon.-Schr., iii. 1852. (Quoted in Schmidt's Jahrbücher, vol. lxxvii. p. 4. 1853.

¹⁴⁷ Ann. der Chem. und Pharm., vol. lxxxvii. p. 367.

¹⁴⁸ Quoted in Liebig and Kopp's Jahresbericht für 1853, p. 616.

¹⁴⁹ Comptes Rendus, vol. xxxii. p. 374, and vol. xxxiii. p. 85.

(the gramme being about 15·4 grains) in eight hours and a quarter; there being 1·614 grammes secreted in the first hour, while in the eighth there was only 0·73 of a gramme. In this case, however, the secretion was only collected from the lower and larger duct, while the course of the fluid into the intestine through the upper and smaller one was not impeded. From these observations on the dog, Bidder and Schmidt calculate that an adult man, weighing 64 kilogrammes (or about 10 stone) secretes 150 grammes (or nearly 4·8 ounces) in twenty-four hours. The experiments of Kroeger, which were also made in conjunction with Bidder, yield a far higher result. He finds that a dog, for every kilogramme's weight, secretes, on an average, 89·3 grammes of pancreatic juice in twenty-four hours; and hence he calculates that an adult man, weighing 64 kilogrammes, would secrete, in twenty-four hours, 5·715 kilogrammes, or 12·6 lbs.—that is to say, about the eleventh part of the weight of the whole body. Weinmann, who was assisted by Ludwig, found in a series of thirty-seven observations, extending over seven days, that a dog, for every kilogramme's weight, secretes 35·184 grammes in twenty-four hours; hence calculating from these data, a man would secrete 2·252 kilogrammes (or nearly 5 lbs.) daily. As the views of Bidder and Schmidt are pretty fully given in "The Chemistry of Digestion," in the twelfth volume of this journal, and likewise in the Appendix to the translation of Lehmann's 'Physical Chemistry,' and as an abstract of Kroeger's thesis may be found in the last number of this journal, it is unnecessary to pursue the subject further in the present place.

The *Intestinal Juice* has not, so far as we are aware, been subjected to examination since the publication of Bidder and Schmidt's work. A full report of their investigations is given in "The Chemistry of Digestion," and in the Appendix to Lehmann.

The *Intestinal Gases* have been carefully examined by Valentin,¹⁵⁰ in a memoir entitled, "Remarks on the Gases of Digestion of Horses." An abstract of his researches is given in page 265 of the present volume.

The *Excrements* have been made the subject of inquiry by the following investigators:

1. OSBORNE:¹⁵¹ The Examination of the Fæces in Disease.
2. WEHSARG: Mikroskopische und chemische Untersuchungen der Fæces gesunder erwachsener menschen. Inaug. Abhandl. Giessen, 1853.
3. IHRING: Mikroskopisch-chemische Untersuchungen menschlicher Fæces unter verschiedenen pathologischen Verhältnisse. Inaug. Abhandl. Giessen, 1853.
4. MARCET:¹⁵² On the immediate Principles of the Excrements of Man and Animals in the Healthy Condition.

Osborne's is rather a medical than a chemical paper; and the theses of Wehsarg and Ihring have been noticed in vol. xiv. p. 528 of this journal. It appears from Marcet's experiments, that healthy human excrements contain:

1. A new organic substance, possessing an alkaline reaction, which its discoverer names *excretine*. In its pure state it appears in circular groups of crystals, which have the form of acicular four-sided prisms, and polarize light very readily. It is very soluble in ether, cold or hot, but sparingly soluble in cold alcohol; it is insoluble in water, and is not decomposed by dilute mineral acids. It fuses between 200° and 203° Fahr., and at a higher temperature burns away with inorganic residue. It does not dissolve when boiled with a solution of potash. It contains nitrogen and sulphur, though in small proportions. The products of its

¹⁵⁰ Arch. für Physiol. Heilkunde, vol. xiii. p. 356. 1854.

¹⁵¹ Dublin Quarterly Journal of Medical Science, No. 29, February, 1853, p. 104.

¹⁵² Proceedings of the Royal Society, June 15th, 1854, vol. vii. p. 153.

decomposition have not yet been investigated. Marcet considers that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhoea, attended with loss of appetite; but none could be directly obtained from beef on subjecting it to the same process of extraction as fæces; neither could it be found in ox-bile, the urine, or the substance of the spleen.

2. A fatty acid, having the properties of margaric acid, but not constantly present. He is uncertain whether the margaric acid in the fæces is free, or combined with excretine; but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal, and their acid set free. Not having been able to discover stearic acid in human evacuations, he supposes that what is contained in the fat taken in the food must be converted into margaric acid in its passage through the alimentary canal.

3. A colouring matter similar to that of blood and urine.

4. A light granular substance, which he is inclined to regard as a combination of phosphate of potash and a pure organic matter.

5. An acid olive-coloured substance, of a fatty nature, which he names *excretolic acid*. It fuses between 77° and 79° Fahr., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash, is very soluble in ether and in hot alcohol, and slightly so in cold water. He believes that it is combined in the excrements in the form of a salt with excretine or a basic substance closely allied to it.

6. No evidence of butyric or of lactic acid was obtained.

The fæces of various animals yielded the following results :

1. The excrements of carnivorous mammals—viz., the tiger, leopard, and dog (fed on meat)—contain a substance allied in its nature to excretine, but not identical with it. They contain no excretine, but yield butyric acid, which is not present in human excrements.
2. The excrements of the crocodile contain cholesterin, and no uric acid, while those of the boa yield uric acid, and no cholesterin. [It is probable that the semi-solid urine and the excrements were not duly separated in this experiment.—*Rep.*]
3. The fæces of herbivorous animals—viz., the horse, sheep, dog (fed on bread), wild boar, elephant, deer, and monkey—contain no excretine, no butyric acid, and no cholesterin.

A memoir by Zimmermann,¹⁵³ entitled “A Contribution to the Knowledge of Typhus-stools,” belongs rather to microscopy than chemistry.

(To be concluded in the next number.)

¹⁵³ Deutsche Klin., 19, 1854; and Schmidt's Jahrbücher, vol. lxxxiii. p. 147. 1854.

ANNALS OF MICROLOGY.*

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[Third Year.]

PART I.—ANATOMICAL AND PHYSIOLOGICAL MICROLOGY.

THE notices of investigations in this department which we have reviewed in the present part of our Annals will be found to be less numerous, and in several respects less important, than those which we have had to present to our readers on former occasions. The chief labours of microscopic inquirers appear to have been within the last year devoted to pathological histology. In some quarters important memoirs have appeared relating to comparative or zoological histology (as those of Leydig, Müller's Archiv); but to notice these would lead us too far from the more immediate object of these Annals, which is the elucidation of those branches of histology interesting to the medical observer. It is, doubtless, hardly consistent with science to draw the line which separates comparative from human histology, so much do these subjects illustrate each other; but, for practical purposes, the distinction is necessary. In the compilation of these Annals we have to acknowledge ourselves especially indebted to that ample record of contemporary medical science, Schmidt's Jahrbücher.

HISTO-GENESIS.

Origin of Epithelial Tissue.—Günsburg,† from the result of investigations on embryos of from five to six weeks to three months, regards this structure as formed by the solidification of the plasma into a textureless basis-membrane, in which subsequently elementary granules appear; around these granules envelopes are formed, and afterwards a nucleus becomes developed in the centre of the cell-vesicle. This

* Owing to the departure of Mr. Holmes Coote for Smyrna, the Surgical Report is omitted from this number. The Report on Forensic Medicine is postponed for want of space. The following Foreign journals were received during the months of December, January, and February:

GERMAN.

1. Archiv für Anatomie, &c., von J. Müller. Dec.
2. Archiv für Pathol. Anat., von Virchow. Band vii. Heft 2.
3. Archiv für Phys. Heilkunde, von Vierordt. 1854, Heft 4.
4. Zeitschrift von Rat. Med., von Henle und Pfeuffer. Band v. Heft 1, 2.
5. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. Nov., Dec., Jan.
6. Zeitschrift für Klinische Med., von Gunsberg. Band vi. Heft 1.
7. Zeitschrift (Henke's) für die Staatsarzneikunde, von Behrend. 1855, Heft 1.
8. Annalen des Berlin Charité-Krankenhause. Band v. Heft 2.
9. Schmidt's Jahrbücher. Dec., Jan., Feb.
10. Verhandlungen der Phys.-Med. Gesell. zu Würzburg. Band v. Heft 3.
11. Vierteljahrsschrift für die Prak. Heilk. Prag, 1854. Band 4.

FRENCH.

12. Revue Méd. Chir. de Paris. Dec., Jan., Feb.
13. Bull. Gén. de Thérapentique. Dec., Jan., Feb.
14. Archives Générales de Médecine. Dec., Jan. Feb.

ITALIAN.

15. Bulletino delle Scienze Mediche. Bologna. August, Sept.

NORWEGIAN.

16. Norsk Magazin for Lægevidenskaben. Bind viii. Hefte 5—10.

EAST INDIAN.

17. Indian Annals. No. 3.

AMERICAN.

18. American Journal of the Medical Sciences.
19. Philadelphia Medical Examiner.

† Schmidt's Jahrbücher, No. 2, p. 149. 1855.

view, if confirmed, would obviously assign to the nucleus a position of very little importance, and indeed it would then be almost impossible to conceive what physiological value it possessed.

This author has followed out the development of the cylindrical epithelium in the lungs and intestinal canal in three embryos of six weeks, eight weeks, and five months respectively, and concludes that it takes place exactly in the same way as in the epidermis.

Blood and Bloodvessels.—Some essays of Drummond* on these subjects may be consulted with advantage.

Hairs.—A memoir on the origin, nature, and growth of hairs, by Reissner,† will tend to confirm the views of other recent observers.

PERMANENT TISSUES AND DEFINITE ELEMENTS.

So much importance attaches to the determination of the primary physiological and anatomical characters of so extensive an element as the connective tissue, and so novel are the views recently advanced with regard to it by Virchow, that we think it necessary to give, somewhat at length, an account of the researches of Bruck on the same structure, which differ considerably from those of Virchow.

Connective Tissue.—Bruck (Prof. C., of Basle) has investigated the connective tissue, at various stages in the progress of its development. He refers its origin to the fibrillation, or splitting into fibres, of a dense structureless blastema, by a process of simple dehiscence, without any intervention of cells, into a fibrous form of intercellular substance; but he does not claim this mode of genesis for all the structures to which the name of connective tissue is applied. In the tissue of the umbilical cord, and that of the Whartonian structure, it can be shown that the gelatinous tissue of Schwann does not disappear, but progressively increases, separates the cell-elements from each other, and finally exhibits a clear striation, splitting, and fibrillation in the longitudinal axis of the cord. There is no evidence of a cell-metamorphosis contributing to this formation of fibres. In other parts of the body, the direct origin of the fibre-tissue, by dehiscence of a structureless intercellular substance, is more difficult to be shown, though the author thinks he has often witnessed a secondary increase of the so-called formed intercellular substance. Other tissues, however, are found which are undoubtedly developed, not from intercellular substance, but from the melting together or fusion of cells; thus, the amnion and allantois, according to Bruck, manifestly show originally a cellular formation; but the contours of the polyhedral cells disappear very early, though their nuclei remain longer, often become elongated, and, in the further growth of the membrane, becoming separated from each other, appear less numerous than at first.

In this perfectly homogeneous, transparent, and very thin membrane, fine folds appear, in the direction of which it may be torn artificially into striæ and fibres. The same may be observed in the umbilical vesicle in animals in which it continues longer. In all these cases there is to be seen, in a secondary membranous blastema, a tendency to the formation of folds, splittings, and fibrillations. The same takes place equally, more or less, in all investing tissues—as the capsules of the Pacinian bodies, the membrana propria of glands, and the sheaths of the primitive muscular fibres. To reduce all these structures to a common histo-genetic origin, the author proposes to designate as extra-cellular substance, all that has hitherto been regarded as blastema, basis, and intercellular substance, or cell product. The renal and seminal canals he finds at first to consist of solid cell-masses, on the outer surface of which the peculiar secondary gland-membrane first appears as a structureless and very thin blastema layer, devoid of nuclei. Bruck also recognises the existence of a kind of connective tissue in which simple nuclei play the part of cells. Many so-called free nuclei exist, which, on further examination, are

* Edinburgh Monthly Journal of Medicine, Oct. and Nov. 1854.

† Schmidt's Jahrbücher, No. 2, p. 151. 1855.

proved to be cell-nuclei: that is, surrounded by a distinct cell membrane; but there are also to be found, for a long time, in many connective-tissue structures, not only the entire or rudimentary remains of original cell-nuclei, but also in pathological as well as in foetal connective-tissue frequently, a greater or less quantity of round, oval, oblong, and pointed nuclei, in which no cell wall can be demonstrated; and neither by acetic acid, boiling, or other means, can any trace of an investing membrane be found. These nuclei generally disappear, especially in pathological structures, after they have become elongated into small rod-like or pointed bodies.

Cell-nuclei are also to be met with, which reach an enormous length, and may properly be called fibres. That these elements are not elongated cell-membranes is evident from their appearance, and also because the fibre-forming nuclei entirely disappear under the action of potash. Further, in the nuclei of the elastic fibre cells, the pointed ends are characteristic, and they may be thus readily distinguished from the rod-like stunted nuclei of the contractile fibre cells; their sharp dark outlines, and their homogeneity stand in contradistinction to the pale, often finely-granular, or nucleated nuclei of the muscle cells. In the very open connective tissue, cells will be met with, arrested, as it were, in the process of elongation into fibres; besides the elongated nuclei, cells also are seen, the ends of which are produced into long and fine threads, the nuclei being drawn out very fine, but capable of being brought into view by acetic acid.

Bruck combats the opinion of Virchow, that the nucleus-fibres are hollow, and form a very extensive system of tubes ministering to the process of nutrition.* He has never seen any such cavities, and thinks their existence improbable, as the elastic elements do not become solidified by degrees, but are so from the commencement. According to this author, the greater part, if not the whole of the actual elementary parts, on which Virchow's theory is based, is to be referred neither to the connective nor the elastic tissues, but to another—namely, that of undeveloped capillary vessels. The author bases this opinion on the results of the investigations of the Whartonian gelatinous tissue; in very small embryos the number of the spindle-shaped cells, which constitute the vascular walls, continually decreases, until finally but one or two remain. The smaller the number of these cells, so much the thinner will be the walls of the vessel when complete, and so much the greater will be the amount of development of the individual cells, which increase much in length, and throw out thread-like processes, by which they become connected to each other. These fine connecting threads not infrequently contain single, and even many, elongated blood-corpuscles, lying in rows, and thus showing that here already a circulation has been established. According, however, to Bruck, the stellate formative-cells of Schwann have nothing to do with the first development of the vessels, but appear constantly at a later period, when the larger vascular subdivisions are completed, and their peripheral extensions are further accomplished by the continually increasing intercellular substance. In the tail of the larvæ of the frog, at the time when the heart and larger vessels are being formed, and the circulation established, there may be seen at the peripheral extremities of the vessels free branched cells, which by degrees attach themselves to the outer vascular loops, and become connected with their fine, and at first solid, prolongations.

Amongst the parts in which the vascular system never passes beyond the foetal stage of development, the capsule of the lens, and the capsulo-pupillary membrane in the first half of foetal life, exhibit beautiful examples of vessels forming broad, wide meshes, and with thin, structureless walls. The vessels of the capsule of the lens lie in the structureless membrane, and course along pretty extensively without forming branches or anastomoses; they often have many nuclei running in the direction of the long axis of the tube; frequently a vessel will be seen to run into a long, thin, solid thread, which exhibits enlargements, in which nuclei are deposited.

* British and Foreign Medico-Chirurgical Review, No. xxvii., *Annals of Micrology*.

Around the lens will sometimes be found a thick mesh-work, which, with many large branches, springs from the arteria centralis, but is not throughout permeable to blood. Often, rows of spindle-shaped cells are to be seen, which the author regards as undeveloped vessels of a larger kind; sometimes, a single spindle-shaped cell, with extensive prolongations, appears to connect two large vessels. On the capsule of the lens, no considerable intercellular substance is to be found between the vessels; but it will be seen in abundance, and in continually increasing quantity, around the arteria centralis bulbi and its branches. The above-named cells, as well as those of the intercellular substance of the Whartonian gelatinous tissue in the earlier periods, have large round or oval, never oblong or pointed, nuclei; all these cells Bruck regards as the undeveloped elements of the vascular system. Similar cells occur in the most different tissues, in the areolar connective tissue, and in the umbilical cord—in this last, in all stages of transition to complete and incomplete capillaries; but Bruck has never seen elastic fibres or contractile fibre cells in the umbilical cord, or in the Whartonian gelatinous tissue. According to these views, Bruck considers the so-called corneal corpuscles of Virchow and Strübe, for the most part, as abortive or incompletely-developed vascular elements of the cornea; and he puts the spindle-shaped appendages of the fine capillary vessels of the cornea, described by Coccus, in the same category: and likewise those fine prolongations, whether blind or anastomosing, and containing no corpuscles, lately described by Kölliker in connexion with the capillaries of the cornea. These first-described vessels are, as we have seen in inflammation of the cornea, capable of undergoing a sudden development, and then become filled with blood-corpuscles.

Besides these so-called vascular cells, which are characterized by a more or less rounded or oval nucleus, there are also to be found in the cornea, especially in the pig and calf, elements with long-pointed, sharply-contoured nuclei, which we may regard as young elastic fibres; they anastomose seldom, and run chiefly in the direction of the lamellæ of the cornea. Bruck has not found spiral fibres in the cornea; he regards this structure as one closely related to the Whartonian gelatinous tissue and the lens, but which has reached a higher degree of development, and forms the transition to the more highly-organized tissues—as, for instance, the areolar connective tissue. These undeveloped vascular cells occur in other parts of the body besides the eye structures; they are more or less constant in almost all connective-tissue formations, as those which are usually considered poor in vessels or altogether devoid of them—as the serous membranes, the ligaments; in the central organs of the nervous system—as the infundibulum: in many glands, and the marrow of bones.

The author gives the following characteristic distinctions between the spindle-shaped and the caudate cells of embryonal tissues in general, and embryonal connective tissue in particular:

The contractile fibre cells are distinguished by their permanent independent character, by their small disposition to fusion, by their remarkable bi-polar growth, and the persistence of their nuclei, which do not transgress the rod-like form, and lastly, by their well-known behaviour to acetic and nitric acids and water.

The elastic fibre cells are characterized by the almost unlimited growth of their spindle-shaped or pointed nuclei, their resistance to the action of acetic acid and potash, and their disposition to anastomose, and the formation of a network by a usually uniform bi-polar growth.

The vascular cells are recognised by their round or oval nuclei increasing by partition, by the multi-polar growth of the cell body, and their remarkable proneness to fusion.

The so-called stellate pigment cells appear to differ from the vascular cells only in the different constitution of their cell contents.

REGENERATION OF TISSUES.

Tendons, regeneration of.—BONER* has instituted some experiments on the regeneration of tendon, for which purpose he made sections of the tendo-Achillis in the rabbit, and examined the parts at various times after the date of section. He finds that when a plastic exudation takes place, the walls of the sheath of the tendon become united, and finally degenerate into a thin solid string, the use of the tendon becoming permanently lost. On the other hand, when an effusion of blood takes place, perfect union of the divided parts is subsequently brought about. The effused blood coagulates very soon, the blood-corpuscles become disintegrated, the fibrine softens, and, after a couple of days, the whole presents a homogeneous, here and there granular, appearance. On the fourth day, the blood-corpuscles have almost completely disappeared, the whole mass being uniformly red, and filled with granules; round cells, with large indistinctly-bordered nuclei, begin to be seen, but soon lose their rounded form, and become elongated, the nuclei assuming a spindle shape. In eight to ten days, the coagulum has become almost completely white, the cells are very delicate in outline, and thin prolongations are thrown off from them, which here and there may be traced connecting one cell with another. The intercellular substance shows a clear longitudinal striation, which is more marked each day, and the whole coagulum more and more assumes the appearance of a true tendon, the tendinous tissue being completely organized about the end of the second week; it is not, however, till the fourth week, or even later, that the normal strength and consistence of tendon are assumed.

Boner concludes from his investigations, that the structure of tendon, like that of the cornea, is made up of flat stellate cells, or a fibrous intercellular substance.

Regeneration of Nerves.—The following observations will prove of value for the purpose of comparison with those of Waller and Budge, which we formerly recorded. SCHIFF* considers that the regeneration of nervous tissue takes place by the formation of new fibres in the old sheaths, even before the previous ones are completely disorganized. At the seat of section, the parts become red, and somewhat swollen; and between the bundles of fibres, small, rounded, or angular nuclei, with nucleoli, show themselves; between them will be seen a quite structureless mass, resembling connective tissue, in which soon appear nuclei in rows, and at first round, but afterwards oblong, and placed opposite, but in alternate order. On the appearance of these nuclei, the mass becomes separable into laminae, in which the continuation of the primitive fibres of both ends of the nerve are visible. While at first the whole mass comports itself to potash and acetic acid, like areolar tissue, the cylinder fibres now, on the addition of potash, distinguish themselves from other fibrous tissues, assuming a pale yellow colour, and clear contour. These stages of the development proceed from both ends to the centre uniformly, but the following changes take place more rapidly in the upper. The substance lying between the rows of nuclei, acquires a finely striated appearance, and on the side of every finely striated portion may be seen a dark line, which is the expression of a membrane in which the nuclei are placed. The cylinders assume by degrees a somewhat marked, pale greyish-yellow colour, so that the new nerve has now the appearance of the primitive fibres of the olfactory, and, like this, shows here and there indications of a double border, but it is not so dark; the outer line of this double border is stronger than the longitudinal striation in the substance within. Potash brings out the cylinders very clearly, but does not attack them. Acetic acid shows the nuclei distinctly. Somewhat later, the dark investments of their contents (axis-cylinders) seen in the single nerve fibres are, as it were, removed, and between them lie broad quadrangular, or somewhat rounded, laminae, which are found to be fat; when these laminae exist, the nuclei of the sheath are no longer visible; their number increases, and they at last become

* Virchow's Archiv, Band vii. Heft 1, p. 162.

† Schmidt's Jahrbücher, No. 9, p. 284. 1854.

united to the sheath, which is at first very thin, and but slowly increases in thickness, and hence the newly-formed nerves for a long time exhibit a small diameter. This author does not agree with Kölliker in the opinion, that the formation of a nervous cicatrix is quite parallel to that of embryonal nerve tissue. As to the period at which a regenerated nerve is capable of reassuming its functions, Schiff has observed that, in the infra-orbital and lingual nerves, the communication of sensations was re-established before the regeneration of the marrow in the new part was completed, and at a time when only single fat particles showed themselves in parts remote from each other. The sensitive, and probably also the motor, communication, would seem to require, not the marrow, but the axis-cylinder. The period occupied in the regeneration is from eleven to seventeen days; it is shorter in wounds by section than in those by laceration. The vascular nerves heal more easily and quickly than the sensitive, and these, again, than the motor nerves.

These observations are very well borne out by the investigations of Bruck,* on the sciatic nerve of a cat, which had been cut across in the middle of the thigh, and examined some months after, when the powers of the extremity had been completely restored. The regeneration appeared to be as complete as possible, union having taken place fibre for fibre, there being no blind or ununited extremities; and in each fibre the cicatrix was still visible where the junction of the divided parts had been effected. Nowhere was it observed that two or more fibres were united together, nowhere was there any intermediate substance, exudation, or callus; each end of a central fibre had again found a peripheral fibre, with which it had united so as to form a continuous and isolated line. Above and below the cicatrix the fibres in all these parts, and in all their relations, were perfectly normal. The seat of the cicatrix was marked by a more or less deep circular constriction of the nerve tubes, which on both sides of it were somewhat swollen out, and flask-like. The marrow was in all the fibres, as well above as below the cicatrix for a short distance, somewhat granular, finely striated, and presented a double contour; but at the point of section, and in the dilated parts, it was completely clear and transparent. In this clear space, without the application of reagents, and without further preparation, the axis-cylinder could in many instances be seen, its diameter sometimes unchanged, or, as it might be, a little enlarged or diminished. In a few instances it stopped short on one side or the other, and was no longer visible. The regeneration was most complete in the outer sheath and the axis-cylinder, while the nerve-marrow was sometimes not complete, or replaced by another transparent substance.

HISTOLOGY OF ANIMAL FLUIDS.

Blood-corpuscles, enumeration of.—Vierordt† continues his researches on this subject, notwithstanding the strictures of P. Dubois-Reymond and Ludwig. He has slightly modified his processes. He dilutes the blood still more than formerly, namely, 679·9 times; he uses for this purpose a solution containing $2\frac{1}{4}$ grammes and 0·16—0·17 gramme of salt, in 100 cubic centimetres of water. The capillary tube employed has a diameter of half a millimetre, and its size is measured by being filled with quicksilver, instead of by micrometry.

Amongst various results of his investigations, the following is very remarkable. He finds that the quantity of the blood-corpuscles and the colour of the blood in one and the same animal, stand in no simple relation, nor are they reducible to any law; pale blood was generally found richer in coloured corpuscles than would have been expected from its colour. These results were so contradictory, that Vierordt gave up the attempt to determine the relation of the quantity of corpuscles to the colour of the blood. In three animals bled to death, death ensued in two as soon as the number of the corpuscles sunk to 52% of the normal number.

Decrease of Blood-corpuscles during Hybernation.—In a marmot examined the 28th of November, the mean number of blood-corpuscles in the cubic millimetre

* Schmidt's Jahrbücher, No. 10, p. 789.

† Ibid., No. 1, p. 4. 1855.

was found to be 5,828,000; on the 5th January this number had decreased to 5,106,000; on the 4th of February, to 2,356,000. Hæmorrhage appears to have taken place during the last operation. The animal's weight also was observed to diminish at each period.

Proportions of coloured and colourless Corpuscles in Splenic Vein.—The mean of four enumerations by Vierordt on the body of a criminal, gave 4·9 colourless to 1 coloured.

*Lymph, Micrological Characters of.**—A rare and very remarkable case of lesion of the lymphatics has given to M. Gubler an opportunity of analysing this fluid, so seldom in the human subject recorded to have been submitted to chemical or microscopical examination. We, therefore, produce here the most important results of the observation.

A female, in a good state of general health, presented, at the anterior and superior part of the left thigh, about two centimetres below the fold of the groin, several small phlyctenulæ, or translucent vesicles, of the appearance and size of a grain of sago boiled. They were covered only by the epidermis, and appeared manifestly to result from a varicose dilatation of the sub-epidermic lymphatics. They were disposed in two divergent lines, enclosing a very acute angle, the apex of which corresponded very nearly with the opening of the internal saphena; the sides diverted outwards were lost before they reached the inguinal region; the superior followed exactly the fold of the groin; the inferior ran a little more transversely. There were four very apparent and prominent vesicles a little lower down, and lying on the border of the sartorius. The largest amongst them, when torn with the point of a needle, gave exit immediately to an opaline fluid, which ran in a little stream down the thigh, falling at the rate of about fifty drops per minute, and this continued until arrested by compression. The smaller vesicles gave exit to but a very small quantity. This liquid, collected in a vessel, coagulated, in from one quarter to half an hour, in the same manner as blood—that is to say, it formed a clot swimming in a fluid; but the serum, so to speak, retained the same colour as the clot, was undistinguishable by the sight, and could only be recognised on shaking the mass.

Submitted to chemical and microscopical analysis, this fluid was found to present the characters assigned to lymph. Other abnormal conditions of the lymphatics existed in this limb which it is not necessary to specify here. It may be mentioned, however, that on one occasion lymph escaped from a rupture of one of the vesicles, and continued to flow for forty-eight hours; and it has been calculated that about 2830 grammes, or nearly six pounds, escaped in the twenty-four hours.

Physical properties.—At the moment of exit from the vesicle, the liquid was white, opaque even when seen in drops, having much the appearance of milk deprived of its cream, with a slightly dull yellow tint. It had a strong alkaline reaction, a feeble saline taste, and a scarcely sensible animal odour. As before observed, it separated into a serum and clot, the former still retaining the opaque white colour, and scarcely distinguishable from the latter.

Microscopic examination, with powers from 300 to 500 diameters.—A considerable quantity of yellowish corpuscles occupied the field, similar to those of blood recently withdrawn from the circulation, but of very unequal size. Some had the usual dimensions of blood-corpuscles, but the greater part were sensibly smaller, reaching a diameter of only $\frac{1}{150}$ th of a millimetre. Lastly, there existed a certain quantity of coloured corpuscles, like the preceding, but much smaller, being only about half the dimensions in diameter of the larger ones—that is, about $\frac{1}{300}$ th of a millimetre. These little globules were seen to be rounded on all sides as they rolled over the field; they were constantly spheroidal, and exhibited no flattening, excavation, or anything which indicated a nucleus; their surface was smooth, their outlines regular, and their yellow colour appeared as intense as that of the best formed blood-corpuscles, if not even more so.

Besides these coloured corpuscles, which, in the opinion of Gubler and Qué-

venne, are to be regarded as only modifications of those of the blood, there were others less numerous, and pale or colourless, and of very various dimensions. The smallest, having the same dimensions as those last described, were white or colourless at a certain focal distance; but on approximating the object-glass a little towards them, they appeared to present a very light greenish tint. They were spheroidal, covered with small, but very apparent, granulations, which, however, did not render their outline irregular. The largest of these white globules exceeded in size the largest blood-corpuscles. Their form was regularly spherical, their outlines smooth and uniform, and their walls finely punctuated. There was no visible nucleus, and no appreciable greenish tint. These larger white globules, reaching to $\frac{1}{100}$ th of a millimetre, were very rare, but the smaller white ones were present in abundance: intermediate ones were constantly seen. Lastly, there were suspended in the liquid, infinite multitudes of molecular granulations, scarcely visible from their extreme tenuity, reaching to only about $\frac{1}{800}$ th of a millimetre in size.

The above elements may be regarded as those proper to an average specimen of this fluid, but on some occasions others were observed. Thus, in one examination there was found a considerable number of discoid blood-corpuscles, and bodies analogous to the white globules of the blood; in another specimen the small white globules, and the small spheroidal bodies of a hæmatoid nature above noticed, were less numerous. With these exceptions, the same elements were constantly to be found, and with the same characters, the relative proportions of them alone varying.

Certain changes were produced in these little bodies when allowed to rest, all, however, more or less attributable to histolytic influence, and, as we conceive, in no way characteristic; the discoid bodies became globular, granular, and mulberry-like, and were at the same time diminished in diameter. The small spherical blood-globules manifestly multiplied as the lenticular bodies disappeared; a part of the molecular granules grouped themselves into pellicular masses, more or less extensive, in which were to be seen some very brilliant globules (oil particles).

Weak acetic acid dissolved almost all the red globules, which first became enlarged; a few of the colourless corpuscles resisted its action for some time, but finally disappeared. The white globules were greatly altered by this reagent, but did not become dissolved; the largest became much swollen, their cellular wall becoming pale and thin, and losing its punctuated appearance. The granulations assembled in the cavity of each cell in a single rounded mass, resembling in form and aspect an ordinary ex-centric nucleus. In the small white globules the granulations of some became more apparent on the addition of the acid; others exhibited a very pale zone, which seemed to be a rudimentary cell-wall, closely embracing a granular nucleus. By the action of ammonia, also, the red globules instantly disappeared; the white globules dissolved in this reagent, but more slowly. The molecular granules were dissolved by ether, oily drops resulting on its evaporation. Iodine coagulated the albuminoid matter, and coloured it yellow, as well as the globules. Water swelled out the discoid bodies, and rendered them vesicular, depriving them at the same time of their colouring matter.

The clot was composed of a mass, which appeared amorphous when thick, but which was manifestly fibrous and striated when examined in thin particles, and showed delicate fibrillæ under the microscope. The following is a *résumé* of the observations on the microscopic elements of this lymph.

It contained, in suspension in a serous liquid:—(1) hæmatoid corpuscles, always of a diameter inferior to those of blood, some lenticular-like blood-corpuscles, properly so-called, others very small, spheroidal, and smooth; (2) pale, scarcely-coloured globules, being those more usually designated as lymph-corpuscles, some exceeding in size the red corpuscles of blood, others much smaller; (3) granular fatty molecules.

The authors regard the first-named elements merely as modifications of the blood-corpuscles, presenting a similar aspect and similar chemical reactions; the second resemble the white corpuscles of the blood, but differ from them in certain

regards; these are the veritable corpuscles of lymph of authors. The difference between the white globules of the blood, and the large white globules of lymph, seems, according to MM. Quévenne and Gubler, to be manifested chiefly in the different comportment of their nuclei to acetic acid; but they do not attach much importance to it.

CARTILAGE AND BONE.

Cartilage and Bone, Structures of.—Bruck,* after pursuing his views on the connective-tissue elements, describes those of bone and cartilage, especially in their relation to each other, and advances some views different in many most important respects from those generally entertained. He considers that all the primordial parts of the vertebral skeleton, as far as their ossification is concerned, again disappear, and become fused into a secondary blastema, out of which the so-called bone and cartilage marrow tissue are produced. All that is comprised under the term bone in the adult, is, with few exceptions, an entirely independent, generally later formation, which from the commencement is produced as bone, and is either entirely independent of the pre-existing cartilaginous parts, or is developed as an "apposition" to them; consequently, to refer the bone-structures with their medullary canals, or the bone-corpuscles, to the elements of cartilage, is, in his opinion, untenable; and the cartilaginous pre-formed bone, when it remains as such, contains no true bone-corpuscles, but only ray-like, round, or oval ossified cartilage cavities. The author therefore distinguishes the proper bone-tissue from the ossified cartilage, even when the so-called secondary formation of bone is produced by the deposit of an organic cartilaginous base. The secondary bone is neither ossified connective-tissue nor ossified fibrous cartilage, but an independent form of tissue. Ossification and fibrillation are in no way connected; but, on the contrary, the more, cartilaginous, as well as other, tissues undergo ossification, the more fibrous do they become. The bone-corpuscles, then, are not ossified cartilage-cells, but independent formations.

NERVES.

The observations of Gegenbaur, Kölliker, Leydig, H. Müller, and Virchow, on the body of a beheaded criminal, though chiefly of value in a physiological aspect, throw light on some hitherto obscure points in normal histology.

Retina.—In some investigations on the retina, it was found, that in the neighbourhood of the yellow spot, the cones are smaller, but of greater length, and closely pressed together, while, external to it, the rods become interposed. The cones in this situation were scarcely pyriform, had a pretty uniform thickness of 0.002", had no point, and were from 0.012"—0.014" in length. At the periphery of the retina, these little bodies were thicker, assumed rapidly a more pyriform or oval shape; so that, with a length of 0.008", their greatest breadth was 0.003"—0.004". Their somewhat conical points, which, in the peripheral parts, were marked off by a transverse line, had a length of about 0.006"; the isolated rods were about 0.0008"—0.0009" in breadth, and 0.012"—0.014" long. It was also observed, by careful focalizing, that the points of the cones lay somewhat deeper than the extremities of the rods. Vertical sections through the yellow spot showed a remarkable thinness in this situation; the yellow diffused colour had its seat in the middle layers, but little, if at all, in the inner cell and outer rod-layers.

Nerves of the Papillæ of the Cutis.—Kölliker is of opinion, from the result of his examination of the parts in a recent state, in the decapitated case already noticed, that the transverse striæ of the corpuscula tactus of Meissner are nuclei, which lie in spindle-shaped connective-tissue corpuscles, and that they are by no means to be regarded as nerves, as Meissner and others have supposed. The nerves often lie in spiral coils around the corpuscula on the outside, but at other times run straight beside them; their terminations could not be clearly defined.

Retina, Histology of Yellow Spot.—Bergmann,† of Rostock, had an opportunity of investigating the structures of the eye in the case of a beheaded criminal, six hours after death. His views differ somewhat from those of Kölliker.

* Loc. cit.

† Henle und Pfeuffer's Zeitschrift, Band v. p. 245.

Half of one retina was examined fresh; the section had been made through the middle of the yellow spot, behind which only small cones were found, some with numerous rows of rods between them; but this part of the investigation appears to have been very incomplete, owing to the structures breaking up into fragments, the localities of which could not be determined.

The second eye was prepared and hardened before section, and consequently the retina exhibited no folds, but there were some differences of level, owing to the difference of thickness in different parts, while, probably, a great part resulted from the action of the chromic acid. The small angular fovea centralis lay very sharply defined in the middle of a somewhat pyriform field, the point of which was turned towards the colliculus of the optic nerve. This space was bounded on its upper and under sides by delicate borders, which did not reach completely to the point; but there intervened between them here a middle elevation, the *plica centralis* of former observers. The borders inclined towards each other, but did not touch. Bergmann proposes to call this little field, the *Area centralis retinæ*. In this area the nervous layer becomes suddenly very thin. Some sections from the optic nerve to the fovea showed well the particular disposition of the nervous matter in this situation; the layer of ganglionic bodies was found not to be continued over the base of this depression, while both the granular layers, with the intergranular layer, though very thin, were continuous throughout. Bergmann uses this as an argument against the opinion, that the ganglionic bodies constitute the perceptive part of the retina. Kölliker, Hannover, and others, consider the fovea as a physiologically imperfect part. Bergmann describes a peculiar arrangement of the fibres in the neighbourhood of the fovea, by which those coming from the outer granular layer take an oblique course, and finally pass into the inner layer. This observer claims for this portion of the retina a higher physiological importance than is accorded to it by others.

*Nervous Ganglia of Lymphatic Glands.**—Schaffner asserts that further researches have confirmed him in his opinion of the existence of microscopic ganglia in the lymphatic glands. He has, by degrees, brought the whole of some of the minute axillary glands of the mouse under the field of the microscope; a section being made longitudinally, and the structures treated with acetic acid, nerve fibres and ganglia were found, but very sparingly.

Périnèvre.†—Under this name, M. Robin describes a structure investing the primitive bundles of the nerves, forming an uninterrupted sheath, which extends from the point of exit of the nerves from the dura mater or the ganglia, to the peripheral terminations of the nerve-tubes; it is absent from the branches of the sympathetic which present a grey colour and soft consistence, but exists in those which are white. Each tube is composed of a wall, some thousands of a millimetre thick, homogeneous in substance, and having neither fibres nor fissures; it is, however, slightly granular, and provided with longitudinal nuclei, much further separated from each other, and much less numerous in proportion as the tube is large. It is rendered hard, and assumes the appearance of parchment on the addition of nitric acid. Each nerve filament, whether visible to the naked eye or not, is enveloped by this périnèvre; it is to be distinguished from the neurilemma, which forms a sufficiently thick coat on the large nerves, and supports the nutrient vessels, but the périnèvre exhibits no capillaries.

VARIOUS STRUCTURES.

Structure of the so-called Worm of the Dog's Tongue.‡—It may be interesting to mention here the results of a microscopic examination of this little body, made by VIRCHOW. He has found it present in all dogs, as well old as young; its phy-

* Henle und Pfeuffer's Zeitschrift, Band v. p. 255.

† Gazette Médicale de Paris, No. 38. p. 589. 1854.

‡ Virchow's Archiv, Band vii. Heft 1, p. 170.

siological use he considers, with Morgagni, to be that of furnishing a support to the very long tongue of these animals. Two structures may be discerned in it by the naked eye: one, red, fleshy, and separated into irregular divisions; the other, white, hard, and apparently cartilaginous. In the former, Virchow has found striated primitive muscular fibres, which pass from side to side in rather sharp curves; abundant nerve filaments were seen; there were also some longitudinal muscular fibres. In the dense white part, hitherto generally considered cartilaginous, he found no cartilage elements, but a thick fatty tissue, enclosed in a dense fibrous covering, from which the muscular tissue springs. Virchow, therefore, regards this little body as neither sinew nor cartilage, but as a quite peculiar, half-muscular, half-fatty and fibrous structure.

DONDERS* finds that the bile contains no hepatic cells, but only cylindroid epithelium, with nuclei. The liver cells are, in the higher animals, subject to molecular changes. In the secretions of the pancreas, and in the saliva, some dissolved and half-disintegrated gland cells are constantly found. He considers the mucus of the intestinal wall to be produced by the rupture of distended cells on the free surface; some cells burst and discharge their contents without losing their nuclei.

In the very open tissue of the papilla the fat, after absorption, becomes heaped up into large drops (it is usually infiltrated uniformly between the bloodvessels), and these form, by flowing together after death, a kind of compound cells.

The papilla possesses very numerous capillary vessels, which lie close under its delicate investing membrane; lymphatic vessels are, according to Donders, but rarely seen in the papillæ, and then but as a central vessel near the branch of the bloodvessel: he has not observed a network commencement of the lymphatics in the papilla. The pancreatic juice has not, in his opinion, any particular import in the absorption of the fat. He confirms the presence of fibre cells and a contractile power in the papillæ, but denies the existence of open mouths; though, as in his former experiments with Menzonides, he has often observed the entrance of solid molecules.

Structure of the Lymphatic Glands.—DONDERS† confirms, generally, the opinions of Reichert and Kölliker on the structure of the lymphatic glands. He finds them to possess a strong, but very thin capsule, in which he has not seen fibre cells, though he in no way throws doubt on the statements of Heyfelder. From the capsule, bands pass into the gland tissue, which divide it into lobules. The external lobules are bordered all round by the capsule, the internal not entirely so. The contents of the lobules consist chiefly, but not entirely, of cells lying in a little fluid; between the cells there is a prolongation of the capsule in the form of a very fine stroma, consisting of a network of fibres, with very large meshes. In the outer lobules, capillary vessels are found. After ligature of the ductus thoracicus, the lymphatics of the gland become filled. The lymph and the chyle do not, in the opinion of Donders, entirely pass through the glands, but find a circuitous channel. In coagulated, as well as in injected glands, he has found the contents of the vessels to escape, and to become infiltrated into the parenchyma. The lymph passing out of the glands contains less water, and more fibrin and morphological elements—namely, lymph-corpuscles, which afterwards become blood-corpuscles.

Peyer's Glands.—DONDERS‡ agrees with Brücke in considering these glands as entirely analogous to the outer lobes of the lymphatic glands. They possess a stroma, and capillary vessels, take part in the fatty absorption, and stand in connexion with the lymphatic glands.

Corpora Amylacea.§—We have hitherto found these bodies noticed as occurring under conditions little, if at all, departing from the normal; GÜNSBURG has, how-

* Schmidt's Jahrbücher, No. 9, p. 286. 1854.

† Ibid.

‡ Ibid.

§ Ibid., No. 10, p. 15. 1854.

ever, found them to constitute a large element in certain tumours in the brain, near the vessels of the neck, and behind the stomach.

Cilia: Nasal Membrane.—GEGENBAUR* was unable to detect any ciliated epithelium on the upper eyelid, but found, in this situation, the ordinary pavement epithelium, which corresponds with Henle's later observations.

According to the observations of Gegenbaur, Leydig, and Müller, a ciliated epithelium is found over the whole nasal mucous surface, even in the olfactory region on the cribriform plate, which, according to Todd and Bowman, possesses, in lower animals, a peculiar, unciliated cell-layer.

It was endeavoured to determine the direction of the ciliary wave by finely pulverized carbon, but no results were obtained to warrant a belief that the stream took place in any determinate direction. The epithelial cells in the olfactory region appeared to be more delicate than in the under parts, and here and there had two, and sometimes three, nuclei.

Cilia: Auditory Membrane.—KÖLLIKER, in the same case (that of the beheaded criminal), examined into the distribution of ciliary motion in the auditory apparatus, and showed that it was absent from the membrana tympani; neither could it be demonstrated to exist in the ossicula: it was present, however, on the promontory, on the outer wall near the membrana, and also on the floor and roof of the cavity.

Cilia: Brain.—LEYDIG examined this organ with a view to determine the question, whether cilia exist on the surfaces of the ventricles, and, if so, in what situations. This observer had found that in fishes, amphibia, and birds—and, according to Valentin, in mammalia—the epithelial cells of the choroid plexus were ciliated; but, though the cells of the choroid plexus, in the case under examination, were well seen, and presented a clear border, there were no cilia to be seen in this situation, nor in the cells of the ependyma of the lateral and third ventricles, though fine vertical sections were made, which brought clearly into view the sharp border of the cell-layer. In the fourth ventricle, however, ciliary motion was distinctly observed; the cells were of a round, flat form, the cilia were tolerably long, but very fine, and, when moistened with blood-serum, moved with great activity—a fact which was verified by Müller, Kölliker, and Virchow. When the cilia became inactive, their motion could be once again, for a short time, vigorously re-established by the application of a solution of potash. This ciliary motion, it is to be remarked, was chiefly confined to the neighbourhood of the striæ acousticae (calamus scriptorius).

Kidneys.—LEYDIG could find no trace of ciliary motion in these organs.

Conarium.—M. FAIVRE† recognises three elements entering into the structure of the conarium in man and animals: a fibro-vascular capsule, a soft parenchymatous matter, and an inorganic substance. The parenchymatous matter, hitherto generally regarded as formed by the grey substance, is found to be of a completely different nature. Under a power of 500 diameters, it is seen to be invariably composed of rounded or oval bodies, with clear outlined borders, and variable size; they are insoluble in water, nitric acid, and alcohol. The author, therefore, considers the pineal gland in man, as well as in animals, to be formed by a quite peculiar histological element, distinct from that which enters into the composition of the nervous tissue.

Choroid Plexuses (Structure of).‡—FAIVRE distinguishes three elements in the choroid plexus in the human subject, which he describes as the choroid villousities, the choroid mass, and the choroid vesicles. The last exist only in the human subject, being found in the lower part of the lateral ventricles, under the form of

* Observations on the Body of a Beheaded Criminal: Verhand. der Phys. Med. Gesellschaft zu Würzburg, Band v. Heft 1, p. 14. 1854.

† Gazette Médicale de Paris, No. 36, p. 555. 1854.

‡ Ibid.

a cluster, which, when closely examined, is seen to be formed by a fold of cellular lamina, adherent in certain places to a subjacent vascular basis, the free portions thus assuming the shape of vesicles; each of these apparent vesicles contains a soft white mass of cellular tissue, and a large quantity of small inorganic granules, partly composed of carbonate of lime. In man, as well as in the higher animals, numerous concretions are formed in the choroid plexus, sometimes produced in the cells of the epithelium (?), sometimes in the vesicles, or attached to the walls of the vessels, or free in the spaces comprised between the coils of the vessels and the epithelial layer. These concretions consist of carbonate of lime, phosphate of lime, cholesterine, silica, phosphate of magnesia, carbonate of potash, and ammoniaco-magnesian phosphate.

M. Faivre thinks that the choroid plexuses, the surfaces of which are so much increased by the villositics, have a close relation to the production of the cephalo-rachidian fluid.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By E. A. PARKES, M.D.,

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1. *On Herpes Tonsurans* (Cazenave). By Professor HEBRA. (Zeitschrift der K.K. Gesellschaft der Aertze zu Wien, December, 1854.)

PROFESSOR HEBRA, in this paper, gives first an historical account of the disease which, under the various names of porrigo scutulata, ringworm, teigne tondante, tinea tonsdens, tricophyton tonsurans, rhizophyto-alopecia, has been the subject of so much discussion.

Willan described the disease correctly, except that he derived it from little achorous pustules, an unusual commencement. Plumbe gives a very good account of it. Alibert, apparently, was little acquainted with it; and Rayer, who gave it the name of teigne annulaire, erroneously considered it, with Willan, as a pustular affection. Later authors, as Green, Gibert, Riecke, Wilson, Fuchs, have described the disease either as favus, or as a species of herpes. In 1840, Cazenave described the disease under the term herpes tonsurans, although he knew it had been called porrigo scutulata by Willan, and teigne tondante by his countryman Mahon. In 1845, Malmsten discovered a fungus in the roots of the hair, and Gruby soon afterwards confirmed the observation. Very lately, Robin and Bazin have also carefully described the cryptogamic plant, though the plate given by Robin does not (according to Hebra) represent the fungus of herpes tonsurans, but that of plica polonica.

Hebra then describes the *symptoms* as follows:

1. On hairless parts there are two forms.

(a) Vesicles with clear, or sometimes yellow, contents, grouped or isolated, or normally-coloured on reddened patches of skin. Except in paucity of number, these vesicles are not different from those of herpes preputialis, labialis, &c. In a few hours, however, they dry and form a thin, yellow-brown scurf, which is sometimes surrounded by a ring of vesicles, which rapidly run through the same course. The vesicular character is so decided, that Hebra thinks Cazenave quite justified in the name he has adopted.

(b) More frequently than the vesicular form of the herpes tonsurans is the second or macular form; small deep-red spots, elevated in the very slightest degree above the level of the skin, form and become covered with thin white scales. In a few days, the increase of the spots at their borders has augmented the size of the patch to that of a sixpence, the outer border being more defined and redder than the centre. Afterwards

the centre loses its clear-red colour, and becomes bluish-red, or yellow, or even natural. This appearance characterizes the ringworm of the English. Frequently many maculæ begin at once; they pass and fuse into each other, and give rise to various shapes.

2. On hairy parts the herpes tonsurans is chiefly distinguished from the disease on hairless parts by the dry, ragged remains of hairs of unequal length, as if the hairs had been cut off unequally; the hairs also often drop off, and the yellow-white, or yellowish-brown, paper-thin, dry, bran-like scales of cuticle are disclosed.

The *diagnosis* of the disease on the hairy parts is especially given by the condition of the hair; on hairless parts, the number, arrangement, and figure of the vesicles, or the colour, form, size of the spots, and the grouping of the patches, with the microscopic characters, suffice for the determination.

The microscopic examination of the cuticle, or of the roots of the hairs, or in the hairs, discloses, with a little care, the fungus, which is the smallest at present known.

The *cause* of this disease is unhesitatingly referred by Hebra to the growth of a fungus finding a fit soil. We have, therefore, only to learn how and in what manner the soil is prepared for the fungus, and how the fungus gets there. With respect to the first point, Hebra points out that a *macerated* cuticle represents the fit receptacle; and if in any way the cuticle becomes moistened, the fungus will grow, if it can arrive at the part. He calls particular attention to the fact that when fomentations are continually applied to limbs, an eruption of little vesicles or maculæ often appears; in many cases these resemble closely both favus and herpes tonsurans, and on microscopic examination, the fungus can be detected. In fact, this eruption is owing to the fungus, which finds a fit receptacle in the macerated cuticle. As to the mode in which the fungus arrives at this cuticle, there can be no doubt that in many cases it is brought on the fomenting cloths. In other cases, its presence cannot be so easily accounted for.

Sometimes favus and herpes tonsurans occurs at different parts on the same individual, and Hebra evidently inclines to the opinion, though he will not absolutely decide the point, that the two diseases are owing to the same fungus, at different periods of its growth.

The *treatment* of ringworm is said to be very successful. No internal remedies are given, but the cuticle and the hair are both removed, by applying a mixture of caustic potash, and lard. This ointment is rubbed for ten minutes, night and morning, on the head, during four to six days; a flannel cap is worn during the days the rubbing is carried on, and for several days after, until the cuticle is detached, and the normal-coloured skin is seen below. The head is washed with warm water, and the cure is complete. The entire time occupied is about twelve days. If the disease be on the scalp, the hairs must be pulled out.

2. *A Case of Cancer of the Pulmonary Artery.* By Dr. A. WERNHER. (Henle's Zeitschrift für Rat. Med., Band v. p. 109.)

A MAN, 22 years of age, came under observation with a large and rapidly-growing encephaloid tumour of the knee and tibia, for which amputation was recommended, but had not been performed, when, on January 27th, five days after the patient was first seen, he was attacked with sudden pain in the cardiac region, just to the left of the sternum, with dyspnœa and rapid respiration. Auscultation and percussion disclosed no signs. On the following day, the pain extended to the right side; there was no cough or expectoration, no cardiac palpitation, increase of precordial dulness, or murmur. The pulse was 140. The following night there were many severe attacks of dyspnœa, in which the pulse was scarcely to be felt. On the third day after the attack, some pure coagulated blood was coughed up, and on the following day, a smaller quantity of blood was expectorated. Two or three

days subsequently, the dyspnœa diminished, and the patient passed nearly into the same state as before the attack.

All this time the tumour of the knee had been growing, and on the 7th February, amputation was performed. It was found to be an exquisite specimen of encephaloid;—but we pass over its microscopic characters. On the following day there was much fever, and on the 10th February, a return of hæmoptysis. The febrile symptoms continued, and there was increasing weakness till the 19th, when there was shivering, and dull percussion-note of the bases of the lungs. On the 20th and the following days, very frequent cough, serous, bloody, offensive sputa, mucous râle all over the lungs, shivering, heat, miliaria. On the 24th, death occurred, with the symptoms of asphyxia and profound collapse.

On post-mortem examination, the iliac and crural veins and their branches were normal, not thickened, and without coagula; the vena cava was also perfectly healthy. The heart was healthy. The pulmonary arteries, on the other hand, contained coagula of coherent cancer-masses, forming fibres and strings of dull-white colour, like boiled rice, which strings were composed of many thinner ones, closely pressed together. These masses filled almost all the branches of the right pulmonary artery; the left pulmonary artery contained also many, but was freer. The walls of the vessels were normal in most cases; in some these were incorporated with the contained cancer masses. The smaller branches were dilated, from the pressure of the masses. The capillaries and the pulmonary veins were perfectly free. Under the microscope, the masses were found to be made up, almost throughout, of cells, exactly like primary cancer-cells, large, oval, with one or two nuclei. In the blood of the right heart, and of the vena cava ascendens, precisely similar cells were found. Nothing similar was found in any other blood.

Besides this, there were large gangrenous abscesses in the lungs; and it was noticed that the arteries leading to them were particularly obliterated by the cancer-masses, and that the vessels in their walls were in the same state.

There was no trace of either young or old tubercle.

This singular case, then, consisted in primary cancer of an extremity and secondary cancer in the branches of the pulmonary artery; and all the facts seem to show that the cancer-cells passed as such from the primary growth through the medium of the venous blood, to the right side of the heart, and then into the pulmonary arteries. A general infection of the blood is negatived by the fact that nowhere, except in the direct track of the venous blood coming from the tumour, were cancerous masses found. In fact, an infection of the blood, a general cancerous disease, does not appear to have gone before the primary local manifestation in the tibia. Dr. Wernher, we may remark, pauses at this place to argue against the hypothesis of a general cancerous disease, or cachexia, preceding in any case local disease; and urges that the cancer is in fact first a local one, like syphilis, and that the general cachexia is entirely secondary.

In the case now related, it would appear that after the removal of the tumour of the leg, the secondary cancer of the pulmonary artery grew very rapidly.

Dr. Wernher then compares the symptoms of his case with those of cancer of the lung given by Walshe, and finds a remarkable similarity, except that there was superadded gangrene, from the blocking up of the arteries.

3. *Communication of the Left Ventricle with the Right Auricle.* By Dr. BUHL. (Henle's Zeitschrift für Rat. Med., Band v. Heft 1.)

DR. BUHL relates the case of a girl, aged 19, who had suffered almost from birth from palpitation, dyspnœa, and cyanosis. When visited a short time before her death, the following physical signs were noticed: considerable bulging of the thoracic wall, on a level with the second rib; dull percussion-note over the whole sternal region, from the second rib downwards, and on either side, to half an inch outside the nipples; pulsation over the whole front, more on the right than on the left

side; marked systolic thrill, more marked also on the right than the left side; marked systolic murmur on the right side, with its maximum close to the sternum, on a level with the fifth cartilage; moderate fulness, without undulation of the cervical veins; slight visible pulsation of the carotids, but of no other artery. On section, the right auricle was found of enormous size; the left auricle was extremely small; the left ventricle was somewhat hypertrophied; at the upper part of the septum the muscular substance was transformed into a thick fibrous tissue, in the middle of which was an oval opening (one-and-a-quarter centimetres long, half a centimetre broad), surrounded with a firm cartilaginous ring, and leading into the right auricle, immediately above the insertion of the tricuspid; the aortic valves were thin; the right ventricle was dilated; the tricuspid valve greatly thickened, particularly near the point where the opening existed; at this point, also, it was rigid, shortened, and therefore incompetent; the calibre of the pulmonary artery was increased, that of the aorta much narrowed; the jugular veins were not remarkably enlarged. This condition sufficiently explains the physical signs.

During the systole arterial blood must have been mixed in large quantity with the venous blood in the right auricle; during diastole a small quantity only of venous blood would pass from the auricle into the ventricle, as the entrance into the right ventricle was so easy. Professor Buhl points out, that in this way the arterial was less contaminated with the venous blood than the venous with arterial blood, and he conjectures that the transmission of blood already surcharged with oxygen through the lungs may have produced the dyspnoea; as if it had been surcharged with carbonic acid. If the dyspnoea was not owing to this cause, it must have been to the pressure of the heart on the lungs.

The cause of the opening is referred, not to congenital defect, but to inflammation and ulcer-formation in the ventricular wall, occurring during foetal life or soon after birth; and the observations of Dittrich are referred to, who had remarked that an abscess at this point of the heart might cause a communication between the left ventricle and the right auricle.

4. *Notes on Twenty-two Cases of Disease of the Heart in Hindoos.* By JAMES HINDER, Esq., Sub-Assistant-Surgeon. (Indian Annals, No. III. p. 69.)

THE following extract gives the principal facts of this interesting paper; in which, as in the essay of Dr. Morehead, recently analysed by us, we find abundant evidence that our experience of rheumatic affections in this country does not run counter to that of our Indian brethren.

“Of these cases, recent inflammation of the endocardium, or of the muscular structure, was not observed in a single instance.

“There were three cases of acute pericarditis; all terminated fatally, and were examined after death.

“On examination, extensive disease of the pericardium was observed; in front it was loosely attached to the heart by recent adhesions of soft lymph,—laterally the attachments were closer. On laying the pericardium open behind, about six ounces of pure pus gushed out; the walls of the abscess were formed by the visceral and parietal portions of the membrane, which were here greatly thickened.

“The walls of the ventricles appeared to be thinner than natural, an old deposit was noticed near the free edge of the mitral valves. No other abnormal appearances were observed in the organ.

“The liver was greatly congested.

“Rheumatism was associated with one of these cases. One patient was twenty years of age—not the case of rheumatic pericarditis,—the other two men, thirty-five years each.

“In all the other cases, with one exception, there was evidence of disease of one or more of the valves of the heart.

“Five of the nineteen cases in which chronic disease existed, or more than one-

fourth, could be traced to rheumatism as a pathological cause, the patients having, as usual, been attacked long before they came under treatment, at periods varying from five months to ten years. In most of the other cases the history was very obscure, and could not be depended on with any degree of confidence.

“Age does not appear to have had much effect as a predisposing cause, except in the rheumatic cases. In the patients now under consideration, eight were under thirty years of age, and only three, or somewhat less than one-sixth, under twenty.

“Of the five rheumatic patients, three were under twenty years of age, and the remaining two, twenty-five each; this is satisfactory, for all experience in Europe proves that ‘rheumatic pericarditis is peculiarly a disease of youth.’

“The particular orifices affected were as follows—viz. :

	Cases.
“1. Aortic constriction	2
2. Mitral regurgitation	7
3. Mitral regurgitation and aortic constriction	2
4. Mitral and tricuspid regurgitation	3
5. Tricuspid regurgitation.	2
6. Seat of disease uncertain	2

“These conclusions cannot be altogether depended on, as the evidence is founded chiefly on the physical signs; it is clear, however, from the table, that the mitral valves are individually the most liable to disease.

“Remains of former pericarditis existed in one case, in which the bag of the pericardium was completely obliterated by universal adhesion of the parietal to the visceral layer. And although the aortic valves were also slightly diseased, and the lungs emphysematous, the heart was neither dilated nor hyphertrophied; on the contrary, it was decidedly smaller than natural; both kidneys were affected with fatty degeneration, this lesion inducing the fatal termination of the case. It may further be remarked that the kidneys were not diseased in the other cases examined after death.”

5. *On Tubercular Disease in the East.* By Dr. WILSON. (Indian Annals, No. 3, p. 182.)

In the zillah jail at Rajshye, there was an average of 846 prisoners. The average annual mortality from tubercle was a little under 8 per 1000, or nearly the same rate as at home. Thus, in six years 165 men died, and in almost all, post-mortem examinations were made. Tubercles were present in one-fourth of the whole.

Dr. Wilson remarks :

“The natives of India form no exception to the dark races in other parts of the globe, or at least this much may be said, that the exemption from phthisis is by no means so universal as has been supposed, if portions of the continent of India are so exempt it is very desirable to have information regarding them, as convincing as that given of places where the disease has been ascertained. It is not surprising, that, previous to the universal practice of percussion and auscultation, the extent of the prevalence of the disease should have been overlooked, but at first sight it is surprising that it should not be universally recognised up to the present time, the explanation is found in the difficulty of always procuring post-mortem examinations, and in the peculiar and latent nature of the chest symptoms, and partly, no doubt, from education at home settling in the mind a belief to the contrary.

“The disease, besides the common wasting form, which has procured for it the descriptive names of consumption and decline, shows itself in two varieties, more commonly than in Europe—the latent, and febrile; the latter is, possibly, often only a hurried termination of the latent. In the latent form, the chest symptoms may never appear, the disease being fatal by hurrying on of concomitant disease in the bowels, the effect of climate being to excite to activity the abdominal symptoms.” (p. 188.)

6. *On Chlorosis simulating Phthisis.* By Dr. RILLIET. (Archives Générales de Méd., Fév. 1855.)

AMONG the various forms of chlorosis is one little known, remarks M. RILLIET, which is accompanied by fever and grave functional derangement; it bears, in fact, a very close resemblance to phthisis. Thus, a young lady consulted M. Rilliet, with a diagnosis from her ordinary attendant that her lungs were profoundly diseased. The affection had commenced with dyspepsia, gastralgia, gradual diminution, and, at last, cessation of the catamenia, dyspnœa, pallor, and great prostration; then cough, with expectoration formed of saliva and greyish fœtid sputa; then hæmoptysis came on, and speedily profuse sweats, rapid wasting, and the other more obvious symptoms of consumption. The physician in attendance declared that there were unequivocal signs of tubercles at the apex of the right lung. After learning these particulars, M. Rilliet thought it scarcely necessary to examine the chest in order to confirm the diagnosis. He did so, however, and, to his astonishment, was not able in any way to find the slightest physical evidence of tubercular deposition. He satisfied himself, in fact, that the physical signs must have existed only in the imagination of the medical attendant, who, too much impressed by the general state, had believed he heard that which would explain it. Unable to find anything in the lungs to account for this state, M. Rilliet examined the abdomen; here he found a tumour, over which he heard a placental murmur—in fact, the young lady was pregnant. This did not account for all the symptoms, however, but, on inquiry, he found the patient had been very chlorotic, and was evidently so, even at the time, to a considerable extent. He concluded, then, that the pregnant state, and the moral condition it necessitated, had combined with the general chlorotic condition to produce symptoms simulating so strongly those of phthisis. The result of the case completely confirmed the diagnosis.

In this instance, the existence complicated the case, but M. Rilliet goes on to refer to other simply chlorotic cases, with the same apparent symptoms of phthisis. When the case is recognised, it is necessary to disregard the cough, the night-sweats, &c., and to refer only to the cause, and for this cause iron is the remedy. A great objection exists in the minds of many physicians—Trousseau among the number—to the use of iron in tuberculosis. “Iron,” says Trousseau, “augments, to a frightful extent, the accidents depending on tuberculous cachexia.” Yet iron (with, of course, moderate exercise, and plenty of good air and food) is the only remedy for the simulated phthisis of chlorosis. Hence the necessity of an accurate diagnosis.

7. *On Ulceration of the Frænum of the Tongue in Hooping Cough.* By Dr. GAMBERINI. (Annal. Univ. de Méd., 1854, and Archives Gén. de Méd., Février, 1855.)

THE very frequent existence of little ulcers on the frænum of the tongue in hooping-cough has been noticed by two or three observers. Since 1844, Gamberini has looked for its presence in all the cases seen by him; he finds it generally, but not always, present, even in the most severe cases; the ulcer has seldom a round form; usually it lies transversely across, and cuts the frænum; it may be placed on the inferior surface of the tongue, near to, but not on, the frænum; it is never preceded by a vesicle, but commences at once as ulcer. Gamberini thinks this ulcer is produced mechanically, by the projection and laceration of the frænum against, and by, the teeth during the violent paroxysms of cough; it is in those cases in which the tongue during the cough is not carried against the teeth, but is retracted somewhat towards the pharynx, as sometimes happens, that the ulceration is absent. If the incisor teeth are of unequal height, the ulcer exists only, or is deepest, at the point where they project most. In one case in which the teeth had not appeared, there was no ulcer. In other cases of convulsive cough, Gamberini has not found the ulcer, but he does not regard this as militating against his explanation of its origin.

8. *On the Absence of Typhus in the Tropics, and in the Southern half of the Earth.* By Dr. M——. (Henle's Zeitschrift für Rat. Med. 1854, Band v. p. 256.)

IN an interesting paper, this anonymous writer passes in review the various evidences found in writers which show that the European typhus is unknown, or almost so, in the tropics and in the southern hemisphere. He appears to be extremely well read in English literature, and draws many of his illustrations from English authors. He has not even omitted the late observations on the occurrence of three cases of typhoid fever in Burmah, by Mr. Scriven;* but he considers these to be doubtful. His conclusion is, that "Typhus is a disease only of the northern temperate zone, and that its southern limit is the isothermal line of 72° Fah.; it does not occur in the southern temperate zone, or at any rate it is not endemic there."

QUARTERLY REPORT ON MIDWIFERY.

By ROBERT BARNES, M.D. (Lond.)

Late Physician-Accoucheur to the Western General Dispensary; Physician to the Metropolitan Free Hospital.

I. DISEASES OF THE SEXUAL SYSTEM.

1. *Observations on Painful Atrophies of the Mamma, Cirrhosis Mammæ, and Atrophic Sarcoma.* By Dr. A. WERNHER, of Giessen. (Zeitsch. für Rat. Med., Band v. Heft 1, 2. 1854.)
2. *On the Extirpation of the Uterus.* By REICHE, of Magdeburg. (Deutsche Klin. 43. 1854.)
3. *On Synostosis Sacro-iliaca in the Obliquely-contracted Pelvis.* By Dr. W. LAMBL. (Vierteljahrs. für et Prakt. Heilk., Band iv. 1854.)

1. THE essay of Dr. WERNHER is an excellent clinical contribution to the pathology of the female breast. We can only find space for some general conclusions. Dr. Wernher says he has seen many tumours of the breast which so closely coincided, in outward appearances, with scirrhus, that they would certainly have been declared scirrhus, and treated accordingly, but which minute microscopical examination showed to want all the elements of cancer, and to consist entirely of the tissue of the mamma in a peculiar atrophic condition. These observations induced him to examine a series of tumours in the Giessen Museum, part of which belonged to the collection of Soemmering, and described as "extirpated cancers with successful result." To his astonishment, he found in most of these the same appearances as those described above, as having occurred to him in recent cases.

The disease is never seen but in elderly women, who have passed the fortieth year. It was always seen in one breast only, the other being quite sound. The diseased breast did not differ much in size from the healthy one, sometimes it was less full. Round the nipple there was an oval roundish tumour circumscribed. To the feel it appeared a hard knotty mass, which plainly included the whole glandular substance, but perhaps scarcely the fourth part of the circumference of the breast. In the remainder of the breast, at the edges and basis, nothing was found but fat. The diseased gland, as compared with the healthy one, was clearly contracted into a smaller space. The tumour was freely movable on the pectoral muscle, but closely united to the skin in the vicinity of the nipple. Here the skin was plainly thinned, free from fat, and its motion over the gland was completely lost. The colour was like that of the other breast. The nipple was harder, more knotty, and sometimes drawn inwards in funnel-shape. In one case, even the

* Medical Times and Gazette, Feb. 1854.

axillary glands were swollen. The tumour was always very painful; the pains came on spontaneously by fits and starts, were shooting, worse and better according to the weather, and extending towards the axilla and arm of the diseased side. The general condition varied; in some it was altogether favourable, or only disordered through sleepless nights; in others, there were complications apparently unconnected with the affection of the breast. Microscopical investigation first freed the author from his error that these tumours were scirrhus. (The microscopical appearances are figured, and several cases minutely detailed.) He is disposed to assimilate this condition of the breast to that known as cirrhosis in the liver.

2. Dr. REICHE states that he has extirpated the entire uterus seven times; in all cases the result was fatal. He, however, advocates the operation in cases of cancer confined to the organ. He then describes the method of operating; this presents nothing calling for analysis. Partial extirpation he represents as a painful operation, but one free from danger. It is indicated in all degenerations limited to the neck of the uterus.

3. The form of pelvic distortion described by Dr. LAMBL must not be confounded with the pelvis obliquè ovata of Nägele. He refers to four cases of the affection recorded: the first by Robert, in 1841, which occurred to M. Dittmayer. The woman was 31 years old. The preparation is in the Würzburg Lying-in Hospital. The second by Kirchhoffer, of Altona. The woman was 21 years old. The preparation apparently in possession of Kirchhoffer. The third by Dr. Lambl. The woman 24 years old. The preparation in the Museum at Prague. The fourth by Robert, observed in Paris. The woman 17 years old. The wheel of a carriage going over her had injured the pelvis when six years old. The preparation in possession of Prof. Dubois. The cases yet known indicate an unfavourable prognosis for the mother, and even for the child. The mother died in all four cases; the Cæsarean section was resorted to in three cases, namely, those of Robert and Kirchhoffer; cephalotripsy in one child, born dead in one, destroyed in one, living in two. Complete synostosis of the sacro-iliac synchondrosis is not an essential character of this deformity of the pelvis; osseous union was only partial in one, and complete in three. In all cases the oblique contraction depends upon narrowing of the sacrum and stretching of the ossa innominata; these last are longer and higher than normal, and rise more perpendicularly. Only one of the pelves is symmetrical. The dimensions of the pelvis from behind forwards are, in consequence of the stretching of the pelvis, not very different from the normal, and in the outlet are even somewhat greater; the oblique diameter, on the other hand, never reaches the normal, and at the outlet not even the half.

Dr. Lambl describes minutely the pelvis of his own case, but without the aid of engravings it would be difficult to give a sufficiently clear description. There was perfect bony union of both sacro-iliac articulations. The author considers that this synostosis was not of an inflammatory origin. He enumerates three kinds of causes of this distortion. 1. Mechanical violence, as fracture and dislocation. 2. Inflammation of the bones, and subsequent change of texture. 3. Arrest of development in relation to variations in size and form. The asymmetry is caused by the dislocation and abnormal position of the joint of the left os innominatum, and through the difference of its size from the right.

II. DISEASES OF FŒTUS.

On Fœtal Luxations, and especially on the Congenital Sub-coracoidal Luxation of the Humerus, &c. By Dr. A. MAYER, of Würzburg. (Verh. d. Phys. Med. Ges., Band v. Heft ii. 1854.)

THE luxations of the unborn child are obviously to be distinguished from those occasioned during labour, and call for a widely different treatment. Dr. MAYER gives a short historical retrospect of the first kind of luxations. He states that

during his own thirty years' orthopædic experience, 36 real foetal luxations have come before him.

a. Three were left shoulder dislocations forwards.

b. One complete symmetrical dislocation of both humeri at the elbow-joints in a man of 27.

c. One symmetrical luxation of the heads of both humeri backwards and outwards in a child three weeks old, who bore at the same time many other distortions.

d. Of luxations in the joints of the hand he has observed seven.

e. One incomplete luxation of the left patella outwards, which had remained unheeded eleven years.

f. One complete luxation of the left knee inwards, with secondary, acute-angled ankylosis of the tibia backwards.

g. Fourteen luxations of the hip-joint; of these, nine were symmetrical, five one-sided.

Dr. Mayer has seen but three cases of congenital dislocation of the shoulder-joint; all were on the left side; in all the head of the humerus was thrown forwards, under the coracoid process, and a tolerably perfect new joint had formed on the anterior surface of the scapula. The first case was complicated with many defects of development. The child died of atelectasis in two hours. The second case was seen in a man 36 years old, who was content to bear with his atrophied arm. The third case was the son of a healthy man, but his mother had died, a few weeks after his birth, of caries of the spine and hectic. The patient was 24 years old. Before detailing this case, Dr. Mayer refers to the treatises of R. W. Smith, of Dublin, Melicher, Malgaigne, d'Ontrepont, Dupuytren, and Ammon, upon similar luxations. In his own case he found the left shoulder almost atrophied to a skeleton. This atrophy extended to the arm, but in lesser degree to the fore-arm. There was but limited motion in the shoulder. The acromion projected considerably, and underneath, instead of the head of the humerus, the finger sank into an empty cavity. The deltoid was so atrophied, that it could scarcely be made out. The head of the humerus lay under the coracoid process in an abnormal flat joint on the fore-part of the scapula, and on the ribs. Abduction and elevation of the arm impossible for the patient, but slight motion can be given by the surgeon. A minute comparison of the dimensions of the two arms showed that all the dimensions of the left arm were sensibly less than those of the right.

The causes of this luxation are predisposing, or occasional. These we pass over, as being chiefly hypothetical. The operation Dr. Mayer recommends for the relief of this deformity he calls *Osteotomia angularis partialis scapulæ*. He describes six stages in its performance. 1. Division of the skin and sub-scapularis. 2. Trepanning the spine of the scapulæ. 3. Sawing-out of the wedge of bone. 4. Sawing through between the coracoid process and the neck of the scapula. 5. Breaking the joint-pan, and removal of head of scapula. 6. Uniting the wound, and dressing. Although Dr. Mayer describes this operation as if it had been performed upon the living subject, it is not stated to have been performed in the case minutely detailed as to anatomical diagnosis; nor is anything said about observed results of the operation.

Injectons of Chloroform-vapour into the Uterine Cavity to Relieve Pain.

By M. ARAN. (Bull. de Thér., Jan., 1855.)

M. ARAN, extending the local application of the vapour of chloroform in uterine affections, recommended by Dr. Hardy, of Dublin, has adapted to Hardy's apparatus a hollow uterine sound, pierced at the end by two openings; this is passed into the uterine cavity. Caution is advised not to inject the vapour too suddenly, lest the uterus be distended; but done gradually, it is said that instant relief is given to uterine pain. Five cases are reported: in three the effect was favour-

able; in one of these, a case of post-puerperal metritis, pain was completely suspended, and on a second injection altogether stopped; in a second, a case of chronic metritis, with an irritable condition of the uterus, two injections produced a permanent amelioration; in the third, a case of retroflexion, in which the intra-uterine pessary could not be borne, after a few injections the instrument could be worn for several days at a time. In the two other cases, the effect was not so marked: in one, of retroflexion with chronic inflammation, the injections at first caused great pain, it is supposed from being forced too rapidly, but relief followed; in the other, of obstinate dysmenorrhœa with colics and nervous phenomena, relief was but momentary, whilst the injection of a few drops of laudanum into the uterine cavity gave ease which lasted for twenty-four hours.

III. LABOUR.

1. *Observations on the Temperature of Lying-in Women.* By C. HECKER. (Annal. du Charité-Krankenhaus zu Berlin. 5 Jahrg. 2 Heft, 1854.)
2. *Induction of Premature Labour on Account of Eclampsia by the Colpeurynter.* By SCHILLINGER. (Schmidt's Jahrb., 1855, No. 2.)
3. *Case of Rupture of the Uterus; Gastrotomy; Recovery.* By JOHN K. MASON, M.D. (New York Journal of Medicine, Jan. 1855.)
4. *Cæsarean Section ending Fatally.* By Dr. LA ROCHE. (Med. Zeitung, 13 Dec., 1854.)
5. *Cæsarean Section Twice successfully Performed on the same Subject.* By W. H. MERINAR, M.D. Misso. (New York Journal of Medicine, Jan. 1855.)
6. *Cæsarean Section in a Rachitic Woman.* By Prof. DUBOIS. (Gaz. des Hôp., 6 Fév., 1855.)

1. THE observations of Dr. HECKER embrace an inquiry into the temperature of the body in lying-in women at different periods, from immediately after delivery down to the end of the puerperal state. The results arrived at do not appear to be very definite: they must be accepted with caution. The following are the principal conclusions:

1. In 35 cases, the thermometer was placed in the vagina immediately after delivery, this being in all cases natural. In many of these cases, the thermometer indicated a marked increase of heat. This bore no constant relation to the duration of the whole labour, or of the expulsive stage, but rather seemed to depend upon the intensity and rapid succession of the pains.

2. In the first stage of the puerperal state, a sinking of the thermometer was observed; this was the more remarkable in proportion to the elevation of the temperature immediately after delivery. On an average, the thermometer stood at the lowest point twenty-four hours after delivery.

3. After this time there was, in the minority of the cases, an even temperature, interrupted only by evening exacerbations and morning remissions. In most cases, the production of heat rose considerably.

4. This elevation of heat was connected with a decided increase in the frequency of the pulse; it bore no constant relation to the condition of the breasts; it was only when it attained a certain degree, that a reaction of the whole system was observed in the form of milk-fever.

5. The period when the thermometer reached its greatest height was very various. On an average, it was seventy-seven hours after delivery.

6. The rise of temperature appeared to be less frequent amongst pluriparæ than primiparæ.

7. The same was also observed in lying-in women who did not suckle their infants.

8. After the maximum temperature, a fall was observed, as in those diseases which determine through lysis; less frequently, there was a fall like that in the

critical determination of diseases; later, a stage of inanition was observable by means of the thermometer.

9. Puerperal diseases are not adapted to thermometrical studies; it is only exceptionally that useful indications can be drawn from observations relating to temperature.

2. A strongly-built primipara, aged 20, fell into convulsions, with loss of consciousness, in her sixth month; the urine contained a great quantity of albumen; the legs and abdomen were œdematous. Cold affusions to head, mustard cataplasms, chloroform, were useless. To bring on labour, India-rubber bladders were applied to the breasts, and at the same time, the colpeurynter to the uterus; besides this, the os uteri was dilated by the finger. The most violent convulsions during the operation were moderated by chloroform. The child breathed some hours. Consciousness returned on the birth of the child. The albuminous character of the urine lasted, gradually lessening, till the fourteenth day. The woman recovered slowly.

3. Dr. Mason's patient was in labour with her sixth child. Rupture of the uterus occurred in the act of defæcating; immediately on its occurrence, she complained of intense, agonizing, burning pain in the right side. The head of the child receded, and could be felt through the abdominal parietes. Twelve hours after rupture, Dr. Neil performed gastrotomy. The child, which was hydrocephalic, was removed, together with the placenta and large quantities of coagula. In one month, the patient had quite recovered.

4. The patient was twenty-eight years old, of middle stature, well-nourished, and well-built. She had never suffered from rachitis or osteo-malacia. On examination during labour, the pains having ceased, and the liquor amnii having escaped six hours, a growth from the promontory was felt, projecting so as to contract the antero-posterior diameter to two and a half inches. The midwife had mistaken this growth for a second head. Only a small portion of the living head had penetrated into the pelvis. The patient had as yet suffered so little from her labour, that she was in full strength. Cæsarian section was determined upon. The use of chloroform had no effect in depriving her of sensation, and was given up. A strong living child was withdrawn with some trouble, the wound in the uterus being made too small in relation to bulk of child. Hæmorrhage followed extraction of child, but was arrested by speedy separation of placenta, which adhered to the posterior wall. The patient seemed cheerful for the first two days; the lochia flowed; skin cool; pulse small, frequent, and hard; hiccough. Severe pain on third day. Thirty leeches applied to abdomen; calomel and morphia; hiccough subsided. After apparent amendment, she died on the fifth day. Autopsy refused by relations.

5. Dr. MERINAR's case is that of a woman aged twenty-four, who was taken in labour on the 14th of July, 1852. There was great deformity of pelvis; its antero-posterior diameter not exceeding two inches. Irritative fever set in, and great tenderness over abdomen. Gastrotomy was performed, and a dead male child extracted. No very severe symptoms followed; and on the 20th of September she had entirely recovered. On the 22nd of May, 1854, she was again in labour. The same conditions rendered gastrotomy again necessary. The incision was made parallel with the first; and a living male child removed. But little hæmorrhage. On the fourth day after the operation somewhat severe symptoms set in: but on the 28th of August both mother and child were enjoying good health.

6. The woman who was the subject of Cæsarean section by M. DUBOIS came into the Hôpital des Cliniques on the 25th of January. She was thirty-two years old, rachitic, and a primipara. Very short, only one millimetre sixteen centimetres. There was œdema of the legs, and albuminuria. M. Dubois has observed that

eclampsia is more common in the rachitic than in others, but observes that in these also it might have been connected with albuminuria, which had not been noticed. The patient was at the full time on admission, and in labour. The narrowness of pelvis ascertained, M. Dubois decided in favour of Cæsarean section, without attempting cephalotripsy. On the 26th the incisions were made under chloroform. The child was extracted alive. The extraction of the placenta was followed by considerable hæmorrhage. The chloroform had annulled the consciousness of the pain of the operation, but not the reality. She knew nothing of it, but the agitation and exoitation manifested prove that she was in pain throughout the operation. She took a small dose of opium at night. A knuckle of intestine made way out of abdomen, and being much inflated with gas, punctures were made in order to reduce it. Vomiting came on next day, fainting, and she died. The autopsy:—No trace of inflammation or internal hæmorrhage; only a small clot in the iliac fossa. Pelvis: antero-pubic diameter fifty-four millimetres. The symphysis pubis was completely ossified, and projected into pelvis; the transverse diameter was twelve centimetres, and the two oblique eleven centimetres.

MEDICAL INTELLIGENCE.

Nurses for the Poor.

THE unremitting exertions of Dr. Sieveking to accomplish the twofold object of providing nurses for the poor, and of finding in this way appropriate occupation for the unemployed women of the labouring classes, are being apparently crowned with success. A committee of the Epidemiological Society have ascertained that there are in the various workhouses of England a large body of unemployed able-bodied women (13,352 altogether, of whom 5,634 are of good character), who might very readily be trained to act as nurses. The committee propose—

I. That the master and matron of every workhouse shall give such female inmates a routine of occupation that shall afford them a knowledge of the duties required in the management of the sick.

To carry out the plan proposed it would be requisite, that a general order of the Poor-Law Board be issued to every board of guardians, directing this, and the following provisions, to be enforced in their respective workhouses. This plan would entail no organic change in the classification and management of the inmates. Each female on being admitted would be put to the employment for which she appeared best fitted. After receiving the necessary instruction in the culinary and domestic department, she would be transferred to the infirmary; where, under the superintendence of the matron, nurse, and medical officer, she would be able to acquire a proper knowledge of the duties of nurse.

II. That the medical officer of each workhouse, as soon as he shall consider an inmate competent to undertake the nursing of the sick out of the workhouse, shall certify to that effect.

The master and matron of the workhouse would regulate the matters of detail with regard to the earlier stages of the training, and judge of those whose behaviour, character, and general aptitude would qualify them to be trusted to attend upon the sick. The medical officer would determine the character of the certificate, and the period when it should be given.

III. That a register shall be kept at the workhouse of all those who have been certified by the medical officer as qualified nurses.

The registers would, collectively, form a source from which nurses might be selected, not only for private individuals, but also for public institutions. A means of livelihood would thus be opened to the workhouse inmate, and her position would be raised.

This plan appears so simple, and at the same time so likely to be of the greatest use both to the nurses and to the nursed, that we trust the Epidemiological Society may be successful in obtaining for it the sanction of the Government.

The Examination for the East India Company's Medical Service.

The late examination of candidates for commissions as assistant-surgeons in the East India Company's Service, heralds a new era in the public departments. It cannot now be long before all appointments held under government will become the rewards of industry and talent, and will no longer be the usurped property of rank and wealth, or the means by which the government of the country buys the support of the representatives of the people. To our own profession it is impossible to overrate the importance of the new system, both in giving a stimulus to medical education in this country, and to the cultivation of science in India.

The number of candidates at the last examination was limited, on account principally of the drain of the younger medical men to the Crimea. The same cause will doubtless operate more or less till peace be declared, or till the increased number of entries into the profession somewhat restore the balance between the supply and demand. Eventually, however, we entertain no doubt that the competition for the Company's appointments will be very great.

We subjoin the substance of the Report made by the Examiners to the President of the Board of Control, and the names of the successful candidates, arranged in order of merit.

"The examination commenced on the 8th, and terminated on the 11th of January. On Monday and Tuesday, January 8th and 9th, the candidates were examined, in writing, in medicine, surgery, anatomy, and physiology, and natural history. Three hours were allotted to each subject. Copies of the questions are annexed to this letter. On Wednesday we proceeded to the oral examination. Each candidate was questioned on the various subjects for an hour. Thursday, January 11th, was occupied with practical examinations in medicine and surgery. These were conducted at University College Hospital, where convenient rooms had been placed at our disposal by the authorities. Here were assembled from various sources (not from the wards of the hospital) a number of patients with well-marked medical and surgical complaints. Each candidate was called upon to examine a medical and a surgical case, and then to write briefly his opinion of their nature, and the plan of treatment he would adopt. The examiners in surgery and in anatomy then tested the candidates in operations on the dead body; one capital and one minor operation being assigned by lot to each candidate."

Names of the successful Candidates.

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|--------------------------------------|---|
| 1. Marr, George, M.D., L.R.C.S.E. | 12. Niven, Wm., M.D., M.R.C.S. Ed. |
| 2. Chuckerbutty, S. C. G., M.D. | 13. Lowe, Thomas, M.R.C.S. Eng. |
| 3. Brake, John, M.R.C.S. Eng. | 14. Cook, Henry, M.R.C.S. Eng. |
| 4. Hill, Marcus G., M.R.C.S. Eng. | 15. Dick, Robert, M.D., M.R.C.S. |
| 5. Mantell, Alfred A., M.R.C.S. Eng. | 16. Atkins, Henry, M.R.C.S. Eng. |
| 6. Collison, John B., M.R.C.S. Eng. | 17. Webster, Henry, M.R.C.S. Eng. |
| 7. King, John B., M.R.C.S. Eng. | 18. Watson, Richard, L.R.C.S. I. |
| 8. Morgan, Arthur, L.R.C.S. I. | 19. Dunman, George, M.R.C.S. Eng. |
| 9. Brown, James, M.R.C.S. Ed. | 20. Drew, Charles, M.R.C.S. Eng. |
| 10. Doyle, William, F.R.C.S. I. | 21. Falconer, D. M., M.D., M.R.C.S. Ed. |
| 11. Lloyd, Edmund E., M.R.C.S. Eng. | 22. Foy, James A., M.R.C.S. Eng. |

The printed questions may be seen in the parliamentary return of the copy of the Report, ordered to be printed, 29th January, 1855; they have also been published in the January numbers of the 'Medical Times and Gazette.'

BOOKS RECEIVED FOR REVIEW.

Unsoundness of Mind in Relation to Criminal Acts. (Essay to which the first Sugden Prize was awarded.) By J. C. Bucknill, M.D. Lond. London, 1854.

Report on the Mortality and Public Health of Oxford during the years 1849, 1850. (With an Appendix on the Social and Sanitary Condition.) Oxford, 1854.

Untersuchungen und Experimente als Beitrag zur Pathogenese des Icterus, und der Acuten gelben Atrople der Leber. Von Dr. Th. von Dusch. Leipzig, 1854.

On the Nature, Signs, and Treatment of Childbed Fever. By C. H. Melgs, M.D. Philadelphia, 1854.

On the Construction and Organization of Hospitals for the Insane. By F. S. Kirkbride, M.D. Philadelphia, 1854.

The Practitioner's Pharmacopœia and Universal Formulary. By John Foote, M.R.C.S.E. London, 1855.

Eutherapeia; or, an Examination of the Principles of Medical Science. By Robert Garner, Surgeon to the North Staffordshire Infirmary, &c. London, 1855.

Notes on some of the Developmental and Functional Relations of certain portions of the Cranium. Selected by F. W. Pavy, M.D., from the Lectures on Anatomy by John Hilton, F.R.S. London, 1855.

Rules of the Society for Improving the Condition of the Insane; and the Prize Essay on the Changes which have taken place since the time of Pinel in the Moral Management of the Insane. By Daniel H. Tuke, M.D. London, 1854.

Transactions of the Belfast Clinical and Pathological Society for the Session 1853-54. Belfast, 1854.

Pharmacopœia Norvegica. (Regiâ auctoritate edita.) Christiania, 1854.

The Diagnosis of Surgical Cancer. (The Liston Prize Essay for 1854.) By John Zachariah Laurence, Surgeon to the Northern and Farringdon Dispensaries. London, 1855.

On the Mode of Communication of Cholera. By John Snow, M.D. Second Edition. London, 1855.

De la Longévité Humaine et de la Quantité de Vie sur la Globe. Par P. Flourens. Paris, 1854.

The Retrospect of Medicine. By W. Braithwaite. Vol. xxx. July—December, 1854.

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INDEX TO VOL. XV.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

	PAGE		PAGE
Absorption, Lehmann on	102	Bright's Disease, subdivisions of . . .	123
Age, old, Van Oven on	369	Brown, Mr., on Diseases of Women .	315
Alderson, Dr., on a peculiar skin dis- ease	401	Bruck, Dr., on connective tissue . . .	554
Allantoin in urine	276	Cancer of the breast	46
Ammonia in the breath	275	operations in . . .	48
Anatomy, Morbid, Jones and Sieve- king on	346	Van der Kolk on	390
Fürster on	358	Mr. Laurence on	474
Anæsthesia, use of electricity in . . .	145	of the pulmonary artery . . .	566
Animals, ages of	370	Carpenter, Dr., on Comparative Phy- siology	176
Army, Medical Service of	411	Cartilage, structure of	561
Arsenic, poisoning by	293	Cell, the cancer, around tumours . . .	390
Arteries, wounds of	279	Chambers, Dr., on Mollities Ossium .	401
Asphyxia, fatal case of	404	Chemistry, Report on	531
Atrophy, cerebral	215	Chevallier, M., on poisoning by car- bonic oxide	294
Ballard, Dr., on Pain after Food . . .	407	Children, Diseases of, Dr. West on .	174
Belfast, Pathological Society of . . .	473	Rilliet and Bar- thez on	332
Belladonna in scarlet fever	77	Chloroform, death from	293
Bladder, rupture of	288	Cholera, Dr. Snow on	449
Blood, leucine in	273	Circulation, Capillary	372
of spleen	5	Coleridge, Mr., his idea of life . . .	311
Mr. Tod on	177	Conarium, structure of	564
Dr. Williams on	180	Copper, poisoning by	293
Vierordt on	265	Corpuscles, Malpighian, of the spleen	11
in gout and rheumatism	402	Cranium, capacity of	213
corpuscles, enumeration of . . .	558	Development of, Mr. Hilton on	469
Bone, structure of	561	Crases, meaning of the term	350
Brain, atrophy of	215	Crises, modern opinions on	327
varying amount of blood in . . .	352	Day, Dr., Report on Animal Chemistry by	531
Braun, Dr., on Midwifery	51	Deaf and dumb in Ireland	152
Breast, Velpeau on	35		
inflammation of	36		
abscess of	38		
tumours of	39		

	PAGE		PAGE
Death, Signs of	71	Heart, disease of, in Hindoos	568
Sudden, Dr. Granville on	134	Hernia, Mr. Ward on	180
sudden, frequency of	137	Radical Cure of	283, 402
Dropsy, Scarlatinal	229	Herpes Tonsurans	565
Duchenne, Dr., on Electricity	138	Hilton, Mr., on the Cranium	469
East, tubercular disease in the	569	Hæmorrhage, production of	349
Eclampsia, Dr. Braun on	53	Hooping Cough, Medication of the	
Electricity, Application of, in Medicine	138	Larynx in	174
diagnostic use of	144	Hufeland, Dr., on scarlet fever	82
Epilepsy, Dr. Radcliffe on	118	Inflammation, Virchow on	324
Epispadias	278	origin of exudation in	329
Eye, Diseases of, Dr. Mackenzie on	174	Dr. Jones on	349
accommodation power of	272	Insane, Moral Management of	471
Faradisation in paralysis	145	Insanity, Dr. Bucknill on	207
Fat, absorption of	110	Pathology of	207
Fever, Virchow on	324	Dr. Winslow on	25
Fever, Scarlet, Prevention of	77	Ireland, Disease in	151
Fistula, vesico-vaginal	321	Iris, Dr. Ruiter on	271
Fleming, Dr., on a Mode of Producing		Irritability, muscular, Dr. Radcliffe on	119
Anæsthesia	524	Joints, Diseases of	57
Flourens on Human Longevity.	369	shoulder, inflammation of	58
Fluids, animal, histology of	558	elbow, inflammation of	65
Food, nutritive power of	117	Jones, Dr., on Disease of the Stomach	403
digestibility of	115	Jones and Sieveking on Morbid Ana-	
Pain after, Dr. Ballard on	407	tomy	346
Adulteration of, Dr. Hassall on	468	G. M., Esq., on Excision of the	
Force, osmotic	103	Knee-joint	402
Förster on Morbid Anatomy	358	Josut, M., on the Signs of Death	71
Fracture of joints	67	Juice, gastric	261
Fresenius, Dr., on Chemistry	173	Jurisprudence, Medical, Dr. Taylor on	466
Gairdner, Dr., on the Nosology of		Keloid, Dr. Addison on	401
Pleuritis	242	Knee-joint, Excision of	402
Garrod, Dr., on the Blood of Gout	402, 405	Labour, affections during	574
Glands, lymphatic, structure of	563	Larynx, Topical Medication of	174
nervous ganglia of	562	Laurence, Mr., on Cancer	474
Gorup-Besanez on Chemistry	173	Lehmann, Prof., on Absorption	102
Gout, Dr. Garrod on	405	Lexicon, Dr. Maynes'	178
Graham, Professor, on osmose	103	Life, the ideas of	307
Granville, Dr., on Sudden Death	134	chemical explanation of	312
Gray, Mr., on the Spleen.	1	Light, Polarized, Dr. Pereira on	178
Greenhill, Dr., on the Mortality of		Lithotomy, Mr. Allarton on	473
Oxford	469	Liver, leucine in	273
Gunsberg, Dr., on histo-genesis	553	London, annual mortality of	135
Hahnemann, Dr., on Belladonna in		Longevity, Human, Flourens on	369
Scarlet Fever	77	Lungs, Diseases of, Dr. Clark on	176
Hand, diseases of	68	Luxation of joints	66
Hassall, Dr., on Food	468	Lymph, micrological characters of	53
		Lyons, Dr., Annals of Micrology, by	553

	PAGE		PAGE
Mackenzie, Dr., on Diseases of the Eye	174	Publications, New, Summary of	178, 474
Mamma, atrophy of	571	Pulse, effect of labour on	286
Marcet, Dr., on Excrement	270	Pyrexia, Virchow on	324
Mayne, Dr., Lexicon by	178	Quévenne, M., on Ferruginous Pre- parations	472
Medicine, Reports on	273	Radcliffe, Dr., on Epilepsy	118
Forensic	288	Record, Bibliographical	173
Micrology, Annals of	553	Reuling, Dr., on Ammonia in the Blood and Breath	266, 275
Midwifery, Dr. Braun on	50	Reynolds, Dr., on Vertigo	463
Reports on	284	Rilliet and Barthez on the Diseases of Children	332
Chloroform in, Dr. Murphy on	360	Ruiter, Dr., on the Iris	271
Quarterly Report on	571	Scarlatina, Prevention of	77
Mollities Ossium	401	Section, Cæsarian	574
Murphy, Dr., on Chloroform in Mid- wifery	360	Snow, Dr., on Cholera	449
Nerves, their implication in fever	326	Society, Pathological, Transactions of the	177
regeneration	557	Spine, Distortion of	405
histology of	561	Spleen, Mr. Gray on	1
Neubauer, Dr., on the Chemistry of the Urine	173	development of	2
Neurologic Researches, by Dr. Wagner	178	weight of	3
Newcastle, Explosion at, Dr. Taylor on	296	blood of	5
Odling, Dr., on Chemistry	173	uses of	21
Ophthalmoscope, mode of using	502	Sputa, tuberculous	495
Orchitis, treatment of, by collodion	283	Statham, Mr., on Excision of the As- tragalus	400
Osmotic force	103	Stomach, Disease of	403
Osteology, Medico-legal	290	Sugar, absorption of	108
Oxford, Mortality of	469	Surgery, Report on	277
Pachydermatocele	404	Survivorship in drowning	289
Paralysis, electricity in	142	Sweat, urea in	277
condition of muscular irrita- bility in	142	Sympathetic nerve, effects of division of	272
saturnine, electricity in	149	Taylor, Dr. Alfred S., on Medical Jurisprudence	467
Pathology, Virchow on	469	Tissue, metamorphosis of, in fever	325
Pereira, Dr., on Polarized Light	178	permanent, micrology of	554
Pericardium, paracentesis of	276	connective	554
Perineum, rupture of	316	regeneration of	557
Peripneumonia notha	257	Tod, Mr., on the Blood	177
Portusis, Dr. Smith on	406	Toxicology	293
Phthisis calcuosa	276	Tripe, Dr., on Scarlatinal Dropsy	229
Physiology, Comparative	176	Transactions, Medico-Chirurgical	400
Annals of	261	Tubercle, Dr. Hall on	477
its objects	301	Tuke, Dr., on the Management of the Insane	471
Archives of	472	Tumours of the breast	39
Placenta, Diseases of	156		
Pleuritis, opinions of the ancients on	242		
Pneumonia, nosology of	242		

	PAGE		PAGE
Tumours, cystic.	281	Virchow, Prof., on Fever	325
Tarsal	400	on Connective Tissue	555
Typhus, Absence of, in the Tropics	571	Vitality, hypotheses of	312
Uræmic poisoning, treatment of	241	Walton, Haynes, Esq., on Tarsal Tu- mour	400
Urine, Dr. Neubauer on	173	Ward, Mr., on Hernia	180
Mosler on	268	Watson, Dr., on the Larynx	174
Bencke on	268	Water, propagation of cholera by	449
hippuric acid in	270	Wells, Mr. Spencer, on Hernia	402
allantoine in	276	West, Dr., on Diseases of Children	174
Uterus, Prolapsus of	284	Wilde, Mr., on Disease in Ireland	151
New Mode of Dilating the Os of	284	Wilks, Dr., on Bright's Disease	122
Extirpation of	571	Winslow, Dr., on Insanity	25
Van der Kolk on Cancer	390	Women, Diseases of, Mr. Brown on	315
Van Oven, Dr., on Old Age	369	lying-in, temperature of	574
Velpeau, M., on the Breast	35	Zealanders, New, Customs and Dis- eases of	521
Vertigo, Dr. Reynolds on	463		

END OF VOL. XV.



